Private and Public Provision of Counseling to Job-Seekers: Evidence from a Large Controlled Experiment *

L. Behaghel†, B. Crépon‡, and M. Gurgand§

October 9, 2013

This paper reports the results of a large-scale randomized controlled experiment which compares two ways of delivering intensive counseling programs for job-seekers at risk of long-term unemployment. In the public version, the program is implemented by government caseworkers who are given more resources and more time to work with the beneficiaries. In the private version, private firms bid to offer a similar service, and are paid part at enrollment and part at delivery. The intention-to-treat estimates of both programs are not statistically different, but many more workers were enrolled in the private program, implying an effect per beneficiary which is twice as large under the public as under the private program (10.2 versus 4.5 percentage points increase in exit to employment after six months). We show that this is not due to differential selection into the programs (at least based on observables). Instead, there is suggestive evidence that the private firms may have insufficiently mastered the counseling technology, and exercised less effort on those who had the best chance to find a job in the absence of the program. This highlights the incentive problems in designing contracts for these services.

Keywords: job placement, counseling, randomized experiment
JEL: J68, J64, C93.

*This paper benefited from comments by Joe Doyle, Esther Duflo, Sylvie Lambert, Thomas Le Barbanchon, Thomas Lemieux, John Martin, Thomas Piketty, Julie Subervie, Katya Zhuravskaya, and anonymous referees, as well as seminar participants at CREST, Ecole thématique du CNRS, Evry University, IFS, NBER Summer Institute, Paris School of Economics and Tinbergen Institute. We would like to thank the ANPE, Unédic and DARES for their involvement in the experiment from which the data are drawn, and for their financial support of the evaluation. In particular, we thank François Aventur, Céline Gratadour, Thomas Le Barbanchon, Béatrice Sédillot, Claude Seibel and Tarik Tamri. We are grateful to Julien Guitard who contributed to the design and implementation of the experiment on the research side. We also thank Lucie Gadenne and Hélène Blake for research assistance. The authors declare that they have no relevant or material financial interest that relates to the research described in this paper.

†Paris School of Economics (Inra) and Crest, 48, boulevard Jourdan, 75014 Paris, France; luc.behaghel@pse.ens.fr.

‡Crest, 15 Boulevard Gabriel Peri, 92245 Malakoff, France; crepon@ensae.fr.

§Paris School of Economics (CNRS) and Crest, 48, boulevard Jourdan, 75014 Paris, France; gurgand@pse.ens.fr.

1 Introduction

Job-search counseling policies have received increasing attention both from policymakers and researchers in many countries over recent decades. Evaluations show that they are generally effective, especially compared with more traditional active labor market policies, such as training and subsidized employment (Card, Kluve and Weber, 2010). Since the late 1990s, governments have enabled the rapid expansion of job-search assistance to a large number of unemployed persons by relying more and more on publicly funded private providers (Finn, 2011). Australia (since 1998) and the Netherlands (since 2001) have allowed private providers to completely or partially take over individual assistance to job-seekers. Similarly, in the US, a significant minority of states has started contracting out case management and eligibility determination to private providers (e.g. McConnell et al., 2003). These early movers have been emulated by others, with strong support from international organizations such as the OECD and the European Union. In France, this enthusiasm led to the launch of large-scale counseling programs provided by private contractors for job-seekers who were hard to place. The fee associated with the whole service delivery to a job-seeker was as high as 4,000 euros, far above the 600 euros of the very same program simultaneously launched by the Public Employment Service – a difference that reflected how large expected gains were.

While there is a growing literature evaluating job-search programs[1], to our knowledge, very few of these papers allow direct comparison of the effects of public vs. private provision of the service. The theoretical literature that examines the implications of outsourcing public services

[1]See the meta-analysis in Card, Kluve and Weber (2010). Dolton and O'Neill (1996), Dolton and O'Neill (2002) and Blundell et al. (2004) provide evaluations for the UK; for instance, Blundell et al. (2004) find that a mandatory job search program in the UK increased outflows to jobs by 20%. Meyer (1995) and Ashenfelter, Ashmore and Dêschenes (2005) report results for the US. By contrast, analyzing a Dutch randomized experiment, Van den Berg and Van der Klaauw (2006) find no evidence that counseling and monitoring affected the exit rate to work. More recently, Graversen and van Ours (2008), Rosholm (2008), Bernhard and Wolff (2008) and Hägglund (2009) find positive effects for Denmark, Germany and Sweden. Autor and Houseman (2010) show that job placement firms tend to fulfill their obligations by means of temporary help jobs, also observing that this leads to significantly lower labor market integration.

to private companies underscores a basic trade-off. Private entrepreneurs may have stronger incentives to invest in cost-saving and quality-enhancing technologies. However, when quality or effort is imperfectly observable, their incentives to engage in pure cost reduction may be too strong.[2] Specifically, the effort of the private job-placement contractors is hard to monitor and is only partly reflected in placement outcomes, given the role played by the job-seekers' own efforts and characteristics. The typical outsourcing contract assigns the private provider a fixed payment per job-seeker at enrollment, and a conditional payment at delivery (job placement). Such contracts may have two types of perverse effects, depending on the exact payment structure. If the conditional payment is relatively large, private providers may maximize their profits by selecting the job-seekers they enroll to have the best labor market prospects (cream-skimming); but if the fixed part of payment is relatively large, they may enroll any job-seeker and just offer a bare minimum of services (parking). Contracting issues may thus undermine the advantages of market discipline.

This paper reports on a large-scale randomized experiment testing an intensive job-seeker assistance program for those at risk of long-term unemployment, which was simultaneously provided by the public employment agency and private contractors in France. The two arms of the new program followed identical principles and provided much more intensive assistance than the standard track followed by job-seekers: in either arm, the caseload ratio was limited to 40 job-seekers per personal advisor, compared to about 120 in the standard track. Contractors providing the private program were paid partially when the job-seeker entered the program and partially on delivery, if the job-seeker found a job within six months and kept it for at least six months.

[2]See in particular Grossman and Hart (1986), Shleifer (1998), Hart, Shleifer and Vishny (1997), and Besley and Ghatak (2001). In their recent survey on the impact of outsourcing public services, Andersson and Jordahl (2011) find that private providers tend to be more efficient than public providers for services where effort and quality is easily observed, thus where contracting presents few obstacles (such as garbage collection). In contrast, the conclusion is mixed or reversed for activities where contracting is difficult (they mention prisons and residential youth care).

The experimental design assigns job-seekers to one of three groups at the beginning of their unemployment period. The first is a control group where they receive the standard services provided by the Public Employment Services (PES). The second group is assigned to the public intensive program and the third, to the private intensive program. Job-seekers assigned to one of the treatment groups are free to enter that program or not; if not, they are sent to the standard track (the same program as received by the control group). We analyze results on a sample of 43,977 job-seekers eligible for unemployment benefits throughout the country.

Intention-to-treat (ITT) estimates show that the two treatment programs perform quite similarly, although the public program has a slightly stronger impact. However, entry rates into the programs differ strongly: take-up in the private program (40%) is one third higher than in the public program (32%). As a result, local average treatment effects (LATE) on program recipients are much larger in the public program: we estimate that the public program increases exit to employment by 10.2 percentage points after six months (a sizable increase, given counterfactual job-finding rates are around 20%), whereas the corresponding private program impact is 4.5 percentage points. Further, the public program works very rapidly: after three months, transitions to employment are already increased by 11 points, whereas the impact is yet insignificant in the private program. We perform a simple cost-benefit analysis based on the number of days of unemployment benefit receipt and find that the private program does not reduce that number, but the public program reduces it by 18 days (in a 365-day window). As a result, the private program generates a large and significant increase in total net expenses, whereas the public program generates a sizable, although statistically insignificant, reduction.

We examine various explanations for the gap in the programs' effectiveness. The large, unconditional payment private contractors received when enrolling job-seekers could have given them incentives to maximize enrollment, and thus led to the higher take-up of a possibly very specific

population of job-seekers for whom intense counseling was ineffective. However, based on a decomposition, we find that the public program still dominates the private one for job-seekers who are comparable in terms of a large set of covariates (in contrast, different population structure between the two programs plays no role in the performance gap). Based on additional but more tentative evidence, we put forward three plausible explanations for this. First, the private program is significantly less effective with the most employable job-seekers, suggesting that private counselors may have provided them with less support on the premise that they would find a job anyway. Second, given indications of heterogeneous performance across the different private providers, we suspect that some of them may have achieved less of a handle on the counseling technology than their public counterparts. And third, counselors in the private program did not sanction job-seekers who failed to comply with the search requirements associated with unemployment benefits.

Our results are in sharp contrast with policymaker enthusiasm for outsourcing job-search services; however, the few available empirical evaluations reach broadly consistent conclusions. Bennemarker, Grönqvist and Öckert (2009) conduct a randomized trial in Sweden and find some positive employment effects to contracting employment services out to private companies, but it is unclear whether these should be attributed to the more intensive counseling or to the fact that the service was contracted out. In our experiment, we compare similar counseling practices at similar intensities so that we are more able to inform about the impact of the private/public management structure. Also in Sweden, Jönsson and Thoursie (2012) use a randomized trial to compare public and private programs of similar cost and intensity meant to rehabilitate those on long-term sick leave, and find no differences between the two. In Germany, Krug and Stephan (2011) randomly assign job-seekers to mandatory counseling programs and find that

the public version performs significantly better than the private one. However, none of these papers analyzes the reasons for the lack of value added from outsourcing.

The rest of the paper is organized as follows. The next section describes the public and private programs. Section 3 presents the experiment. Section 4 gives the main findings, directly derived from the experimental setting. Section 5 explores heterogeneity of treatment impact and how it can help interpret the differences between the two programs. Section 6 presents elements of cost-benefit analysis and Section 7 concludes.

2 Public and Private Programs

2.1 The public program

The French Public Employment Service (PES) has a long tradition of offering a wide range of counseling services to a broad and diverse population of job-seekers. To meet the needs of specific subpopulations, the PES regularly creates new counseling products – among these are programs on achieving self-knowledge (strengths, weaknesses, market values, reasonable expectations); knowing about firms (what they need and are searching for, what their constraints are); learning search methods (search channels, writing resumes, applications, interviews); and researching the job field to get precise knowledge of relevant local opportunities and networks.

In 2007, the PES launched a counseling program for 40,000 job-seekers at risk of long-term unemployment but without peripheral problems (social, psychological or addiction-related). The target is, within the six-month duration of the program, to find the job-seeker a durable job, which is defined as lasting at least six months. The program assigns the job-seeker a "personal advisor," who has a caseload reduced to 40 clients and who meets with the client weekly and searches for job offers with her, sometimes applying directly in her name. This is a significant

increase in the human resources dedicated to one-on-one assistance compared to the usual track, where caseworkers are assigned an average of 120 job-seekers, with whom they meet monthly. The program seeks to strengthen the relationships between advisors, job-seekers and hiring firms, and encourages the empowerment and motivation of those enrolled by having them sign a charter.[3]

The PES recruited caseworkers for the new program from among their existing staff by means of a special call for tender, then compensated for their reassignment with a hiring drive. The means available to deliver the standard counseling program thus remained almost constant. In most instances, aseworkers in the new intensive program met with their clients in dedicated offices outside local agencies. [4]

The price of the new program is not precisely measurable, since the PES's accounting procedure at the time of its inception did not separate out precisely the costs of each service. There are, however, estimations that provide orders of magnitude and put it at 657 euros per client. This includes caseworkers' wages as well as the increased costs resulting from the modified service structure, as well as the equipment involved. This is far above the cost of the standard track, 120 euros according to the same method.

2.2 The private program

Before 2005, the Public employment agency *(Agence Nationale pour l'Emploi*,ANPE) had a monopoly on the placement of unemployed job-seekers. The Social Cohesion Act (*loi de cohésion*

[3]Formally, and in contrast to comparable job search experiments in the US (Meyer, 1995; Ashenfelter, Ashmore and Dêschenes, 2005), the treatment does not directly include stricter enforcement of search requirements, although the more frequent interactions with caseworkers may be viewed as increased monitoring.

[4]PES employees were free to apply for the new program or not; selection occurred at the national level. Caseworkers enrolled were heterogeneous; in particular, the new program included both seasoned caseworkers and young ones without much experience. On average, however, enrolled caseworkers shared the characteristic of being highly motivated. They were attracted by the possibility of participating in a program that allowed them to do their job in good conditions, with more time to focus on the specific needs of each job-seeker due to a lower caseload and better equipment.

sociale, January 2005) opened up this market to private companies. In 2005-2006, the French unemployment benefits provider (Unédic), distinct from ANPE, started experimenting with intensive counseling, provided through contracts with private companies and targeted at the inflow of claimants identified as at risk of long-term unemployment. Unédic's program had the same components as the intensive program provided by the PES and also increased human resources dedicated to counseling: it lasted six months, involved a weekly meeting between job-seeker the caseworker, and imposed a limit of 40 job-seekers per caseworker. While the measure of success was the same, finding the job-seeker a job lasting at least six months, Unidec had an additional requirement, that work time should exceed 110 hours per month.[5]

In 2007, Unédic decided to scale up this program, targeting 41,000 job-seekers among those eligible for at least a year of unemployment benefits. Private providers were selected through a bidding process, conducted separately in 16 areas.[6] The private providers eventually selected belonged to three groups: temporary agencies, consultancies specializing in the placement of workers after mass layoffs, and international placement firms (from Australia and the Netherlands). Out of the 43,977 job-seekers in this experiment, 36% were registered in an area where the private contractor was a temporary agency; 36% in an area where it was a consultancy; and 28% in an area where it was an international placement firm. The payment structure was common to all regions: 30% of the maximum payment was paid upfront, when the job-seeker enrolled in the program; the remaining 70% was conditional on placement, 35% if a job was found within six months, and the remaining 35% if the worker was still employed after six months. The maximum payment per worker resulted from the bidding process: it varied from one region

[5]Capelier and Mizrahi (2008) conduct a qualitative comparison of the two programs and find very few differences. According to another qualitative study by Divay (2009), the methods used by providers in the private program are not particularly innovative, compared with what the PES has been doing since the 1990s. What is new, however, is the intensity of both programs.

[6]There were 25 bids organized. In this paper, we report the impact of programs only when both were simultaneously implemented. We eliminated Alsace and Midi-Pyrénées regions, as significant departures from experimental protocol occurred there. Therefore, we only consider contractors selected in 15 of the 25 bids, and they were operating in only four French regions: Paris, North, Rhône-Alpes and Lorraine.

to another and ranged from 3,000 to 3,947 euros (Vivès, 2009). This means that the minimum payment per worker was 900 euros (the upfront payment when the job-seeker was not placed within six months), and the maximum payment was 3,947 euros (when the worker was durably placed in a job, in the regions with the highest bidding price).[7] Comparing these costs with that of the public program, it is apparent that the private program cannot be cost-effective unless it is far more efficient. One possibility is that Unédic wished to encourage the formation of an as-yet-nonexistent market for counseling agencies, and set generous prices to attract them.

The Ministry of Labor ran a telephone survey on a subsample of the unemployed enrolled in the experiment in March 2008 – i.e., 9 to 11 months after their assignment to one of the three groups. Gratadour and Le Barbanchon (2009) used the information in the survey to provide a more precise idea of the programs' actual contents. They showed that both the private and public programs substantially increased the number of meetings with caseworkers, as expected. They also showed that job-seekers received more frequent trainings on search methods and channel-identification, e.g., firm targeting and Internet searches. However, neither of the two programs significantly increased the number of job offers presented to job-seekers, which is surprising since the goal of the intensive programs was to forge stronger relationships between job-seekers, counselors, and hiring firms (for more details, see Behaghel, Crepon and Gurgand, 2012).

[7] One may wonder why contracts were designed with such large upfront payments, despite the obvious incentive problem. We believe that this is mainly due to the political economy context of the experiment. Unédic was eager to create a private market for placement, previously nonexistent in France. Firms would need to bear large fixed costs and would not enter the market unless they had some assurance that these costs would be covered. This infant industry argument should have implied, if anything, that the principal would make a lump-sum transfer to the firms, rather than a transfer indexed on the number of enrolled individuals, but this was never considered (and would have raised moral hazard issues of its own). Imitation was also at play: as detailed below, the chosen contract structure is close to outsourcing contracts used in several other European countries.

3 Experimental Design and Data

3.1 Experimental design

The public and private programs were evaluated jointly in a randomized controlled trial. The two programs were developed in the same Local Employment Agencies and were opened to the same population. Eligibility was restricted to job-seekers entering unemployment and entitled to at least one year of benefits.

Randomization was used to create three experimental groups: the control group (assigned to the usual track, without intensive counseling), and two treatment groups (assigned to the public or private program). It took place during the first interview at the local PES office (upon registering as unemployed). Once the caseworker had assessed the job-seeker's eligibility, he ran an Extranet program to randomly assign her to treatment 1 (public program), treatment 2 (private program) or the control group. The probabilities of assignment to each group varied locally so as to maximize the statistical power of the evaluation while complying with the quantitative objectives of each program (each local area had targets in terms of recipients of the two programs). This often implied very high probabilities of assignment to the private program (up to 85%) and much lower probabilities of assignment to the public program (down to 6%) and control (down to 9%).[8]

After this randomization, the employment service agent told the job-seeker which track she was offered. Job-seekers assigned to an intensive track were free to turn it down but were denied participation in the other intensive track and were redirected to the standard track. Job-seekers assigned to the standard track were denied participation in either intensive program. Job-seekers were subsequently contacted by PES staff for the usual track, by a dedicated caseworker from

[8]Our estimation procedure, detailed below, accounts for locally based assignment probabilities using weights.

the public intensive program, or by one of the private firms. Job-seekers from the two treatment groups entered the program by signing a specific agreement; if they refused to sign, did not show up, or were eventually found not to meet the criteria of the intensive program, they went back to the usual track.[9]As a consequence, the three-pronged experiment we consider amounts to the juxtaposition of two two-pronged experiments, each involving the control group and one treatment group. They could theoretically be analyzed separately and, given their one-sided noncompliance, the two-stage least square estimates we can produce from each experiment can be interpreted as Treatment on the Treated parameters (Bloom, 1984; Angrist and Pischke, 2009).

The random assignment took place over 12 months, from January 2007 to December 2007, in 216 local public employment offices in 4 of the 22 French administrative regions. Overall, 43,977 job-seekers entered the evaluation sample considered here. Among them 4,565 (10.4%) were assigned to the control group, 3,385 (7.7%) to the public program and 36,027 (81.9%) to the private program (see Table 1).[10]

3.2 Data

3.2.1 Administrative and survey data

Our primary source of information is the PES administrative records. They provide basic socio-demographic information on the job-seekers involved in the experiment. They also allow us to follow job-seekers for 12 months after random assignment and to compute the duration of all

[9]A significant share of each treatment group (about 60% on average) did not actually enter the program they were assigned to. These high rates of noncompliance along with the unbalanced assignment probabilities are factors limiting the precision of the evaluation. Fortunately, these are counteracted by the large samples.

[10]The table seems to imply that the private program was implemented at much larger scale than the public one. This was, however, not the case. Both programs targeted about 40,000 job-seekers; however, the public program targeted job-seekers who were only eligible for short-term unemployment benefits, in addition to the long-term unemployed. We drop those job-seekers from our sample, as they have no counterpart in the private program. Results on these other populations can be found in Behaghel, Crepon and Gurgand (2012).

registered unemployment spells. This information is relevant as long as the unemployed remain unemployed. However, the end of a registered unemployment spell may be due to quite different events, and this information is frequently missing, in which case it is usually labeled as "unknown exit." The use of "exit from unemployment" without knowledge of the type of exit is an important source of bias in evaluation (Card, Chetty and Weber, 2007). In our data 15,876 spells out of the 43,977 job-seekers involved in the experiment (36.1%) left unemployment during the year and for 7,802 of them (49%) the information about the type of exit was missing (see Table 1). From this single source of information, therefore, we cannot tell whether an exit from registered unemployment was due to job placement or to a "discouraged job-seeker" effect or to any other reason. To overcome this problem, as part of the experimental design, an independent survey company was commissioned to conduct a very short phone survey on a subsample of workers whose destination upon leaving unemployment was not identified in the administrative records. The questionnaire was extremely focused so as to mimic the form that job-seekers are supposed to fill out upon exiting registered unemployment. It had a maximum of four questions. We used the first: "Question 1. During the month of ..., you stopped being registered at the PES. What was the reason?" Table 1 reports the sampling probabilities. They were optimized to partly correct for the imbalance of assignment rates between treatment and control groups. To avoid recall error, the survey was conducted monthly on those who had recently left the unemployment registers, during a period of 12 months after the initial assignment. Response rate to this survey was 51% on average, with limited difference between the three experimental groups.

To measure transitions from registered unemployment to employment at various horizons, we use information from the administrative record and, if missing, information from that survey. As shown from computations in Table 1, the overall attrition rate, collapsing files and survey information, was limited to 9%, and well balanced among the three experimental groups. We

treat subsequent non-response as random. Each month new individuals to be followed through the short survey had to be sampled. As a result, the number of individuals used in regressions at different horizons is different from one horizon to another. For example, our whole sample has 43,977 observations but our regressions at six months use 37,952 observations.

In the regressions, we will use weights computed as the inverse of the product of estimated assignment probabilities (which differ across regions) and estimated survey probabilities (which differ across region and time), to ensure representativeness of the initial sample and avoid imbalances between assignment groups.

The first type of employment outcome we consider is the transitions from unemployment to any employment at different horizons. We label it "**Exit from PES registers to employment**." We also consider additional outcome variables. Job-seekers are allowed to hold a job while remaining registered as unemployed. These jobs can sometimes last for a long period of time and involve a large number of hours worked. The private program sets explicit criteria of success for private contractors. In order to be acceptable for a first additional payment, the job has to involve a working time of at least 110 hours per month. However, this criterion does not require job-seekers to formally leave unemployment registers. Some eligible jobs can thus occur during the unemployment spell. This is the reason why we consider a second employment outcome variable, extending "Exit from PES registers to employment" to also include jobs held while unemployed if their working time is above 110 hours per month. We label this outcome "**Any employment**." Finally, the criterion for a job to be grounds for full payment to the private contractor is that it lasted at least six months. We construct a third outcome labeled "**Employment eligible for payment**" which restricts the previous employment outcome to employment spells that were not followed by a new registration into unemployment for at least six months or to jobs held while unemployed that lasted at least six months.

Our analysis mainly focuses on employment outcomes at six months, because it corresponds to the end of the counseling service. As such, the objectives of both programs are to bring job-seekers back to employment within six months, and payment to the private contractors is conditional on this. However, we also look at employment outcomes at other horizons: three months and 12 months. We consider outcomes at three months to measure a fast impact on employment, which is desirable in particular if the objective is to save on unemployment benefits. We also look at employment outcome 12 months after random assignment so as to be able to evaluate medium-term effects of the counseling programs.

We also analyze other types of exit from unemployment, in particular striking off and exit for health reasons. These variables can be computed from the administrative records and our short survey in the same way as the outcome "Exit to employment."

Finally, we used three other important files: the assignment file, the private contractor file and the file registering entry into the public program. The assignment file was linked to the Extranet application used to implement the randomization. Private contractors kept a file in which they registered job-seekers who actually entered the program. It was the basis for the first part of their payment: the forms signed by the job-seeker served as proof of her enrollment. We used this file to measure entry into the private program. For the public program, entry was registered in the PES management file.

3.3 Descriptive statistics and balancing tests

Data from the PES register contain substantial demographic information: highest diploma obtained, gender, age, family status, number of children, former type of occupation, nationality, region. It also contains information about the search process: reason for unemployment, experience in the desired job, statistical risk of long-term unemployment, wage target as well as the

number of previous unemployment spells. Table 2 presents summary statistics of a large set of such covariates, as well as the results of balancing tests. The first three columns present the mean value of each variable over the three different assignment samples. The last three columns present balancing test results: the first column compares job-seekers assigned to the private program with those assigned to the standard track, the second compares job-seekers assigned to the public program with those assigned to the standard track and the last column compares the three populations.

Job-seekers involved in the experiment are quite heterogeneous. A large share of them have some education (31% went to college), but 19% are high school dropouts. The main reason for them to be unemployed is "personal layoff" (40%), but that is then followed by "end of fixed term contract" (23.3%).[11] Their wage target is rather at the bottom of the French wage distribution: 30% of them have a wage target around the minimum wage (the minimum wage in 2007 was 1,280 euros and the average wage in the private sector was 2,660 euros) but 18% have a wage target above 2,200 euros which is close to the median wage in France.

The table also presents balancing tests. When compared across the three samples, all the mean values in the table are very close. Most of the time the test does not reject equality. We also consider joint tests. For each of the two intensive programs, they are performed as the joint nullity of the whole set of covariates in an OLS regression of the program assignment variable performed on a sample that includes only job-seekers assigned to that program or the standard track. The corresponding p-values are respectively 45% and 54%. Last, we consider the joint nullity of the whole set of the previous parameters and obtain a p-value of 12%.

[11] Layoff for personal reasons is based either on a fault of the employee or on other reasons (incompetence, repeated absences). It might occur without notice or severance pay due.

4 Results

4.1 Intention-to-treat estimations

We first consider intention-to-treat estimates for various employment outcomes at six months. We implement weighted OLS regressions on the model:

$$y = \beta_{Pub} Z_{Pub} + \beta_{Priv} Z_{Priv} + Xb + u \tag{1}$$

In this equation Z_{Priv} and Z_{Pub} are the assignment variables to private and public programs and X is a set of control variables.

Results are presented in Table 3. The table has three sets of columns corresponding to the three employment outcomes presented in Section 3.2.1. Each set of columns presents results obtained without and with a set of covariates.[12] The first line of the table gives the estimated value of β_{Pub} and the second the estimated value of β_{Priv} in equation (1). The table also presents the p-value of the test of $\beta_{Pub} = \beta_{Priv}$.

The table shows that being assigned to the public program increases the probability of going back to employment within six months by 3.2 percentage points. Being assigned to the private program also increases significantly the chances of going back to employment, but the measured effect is only 1.8 percentage points.[13] However, although it is substantially smaller than the effect of the public program, the difference is not statistically significant as can be shown from the p-value in the table. The table also shows that standard errors of the estimated impacts are much smaller for the private program (0.9) than for the public program (1.2). This due to the large difference in the number of job-seekers assigned to the two different programs. As expected,

[12] The full set of control variables is listed in Table 2.

[13] These effect must be interpreted as the difference between treated and control job-seekers in the same local labor market. Crepon et al. (2013) show that displacement effects can make such estimations an upper bound of the true impact of such policy in equilibrium.

the inclusion of control variables neither leads to major changes in estimated coefficients nor to any improvement in standard errors.[14]

As can be seen from the table, "Any employment" is quite a different outcome variable as compared to "Exit from PES registers to employment." Indeed, the control mean for this variable is 35.8% which is more than 50% higher than the baseline "Exit from PES registers to employment" variable (23.0%). The private program's impact on this outcome is almost unchanged but it is somewhat lower for the public program. The effect is now 2.3 percentage points, significant at the 10% level only for the public program, and 1.9 percentage points for the private program significant at the 5% level.[15] The effect of the two programs on the last employment outcome "Employment eligible for payment" is presented in the last two columns. Both programs have a positive and quite similar impact on this outcome. Being assigned to the public program increases the chances of going back to a job that lasts at least six months by 2.2 percentage points, whereas assignment to the private program leads to an increase of 2 percentage points.

4.2 Program participation

The first-stage regression is presented in Table 4. The table has two sets of columns. The first set of column is related to entry into the public program. It corresponds to the OLS estimation of

$$T_{Pub} = \theta_{Pub} Z_{Pub} + \theta_{Priv} Z_{Priv} + \theta_{Standard}(1 - Z_{Pub} - Z_{Priv}) + Xb + u. \quad (2)$$

[14]Given the difference in the number of job-seekers assigned to the two programs, one might worry that the public program was implemented at a very small scale, making the comparison with the private program unfair. However, this is not the case: as noted above, the public program was simultaneously developed for other populations of job-seekers which we do not consider here, because they have no counterpart in the private program. Accounting for all job-seekers served, the two programs reached about 40,000 job-seekers each in 2007.

[15]Most of these jobs were, however, short-run jobs. As shown in the last column of the table, only 21.4% of the job-seekers found a job that lasted more than six months. This means that only 21.4/35.8= 59.8% of jobs counted as "Any employment" lasted more than six months. By comparison, the proportion of job-seekers that exited unemployment to employment and did not register again in the next six month in the control group can be computed from the sample as 17.9%. This means that, according to the same computation, 17.9/23=77.8% of jobs counted as "Exit from PES registers to employment" lasted more than six months.

In this equation θ_{Pub} is the entry rate into the public program of job-seekers assigned to the public program. We expect this coefficient to be large and significant. The coefficient θ_{Priv} is the entry rate into the public program of job-seekers assigned to the private program. Similarly $\theta_{Standard}$ is the entry rate into the public program of job-seekers assigned to the control group. As assignment variables sum to one, this model is estimated without a constant term (centering control variables X). In the second set of columns, the table also provides the estimation of entry into the private program:

$$T_{Priv} = \lambda_{Pub} Z_{Pub} + \lambda_{Priv} Z_{Priv} + \lambda_{Standard}(1 - Z_{Pub} - Z_{Priv}) + Xb + u, \tag{3}$$

which receives the same interpretation.

One important result of Table 4 is that, in contradiction with the experimental protocol, some job-seekers assigned to the control group entered one of the two treatments, while some assigned to a given treatment entered the other one. The orders of magnitude are small: less than 3% entered the private program without being assigned to it; the proportion is below 0.5% for the public program. Based on our monitoring of the experiment, these imperfections seem to be mostly due to private contractors pressing the PES to send more job-seekers. To address their demands, some PES agents selected job-seekers on the unemployment registers and gave their contacts to the private contractors, who contacted them outside the experiment. The same job-seekers could in parallel enter the experiment through the randomization tool, and thus receive a different assignment. In the case with only one treatment group, the fact that some members of the control group receive the treatment does not prevent a causal interpretation of the LATE parameter: provided a monotonicity assumption (Angrist, Imbens and Rubin, 1996), the so-called "encouragement design" (Duflo, Glennerster and Kremer, 2008) only requires that the random assignment increases the probability to enter the treatment. In the case with two

treatment groups, however, a stronger form of compliance is needed in order to interpret the IV parameters causally in the potential outcomes framework. In particular, a sufficient condition is that only those assigned to one of the treatments enter that treatment – i.e. θ_{Priv}, $\theta_{Standard}$, λ_{Pub} and $\lambda_{standard}$ in equations 2 and 3 are all equal to 0. This condition is not verified here, as the estimates for these parameters are statistically significant, even though they are small. In Behaghel, Crepon and Gurgand (2013), we show that the effect of this type of imperfect compliance depends on the size of the groups entering a treatment in violation of the initial experimental design. More precisely, it can be shown that the quantity that matters is the difference in the proportions of individuals entering a program when assigned to the control group and when assigned to the other program. This difference is very small in the present case. Indeed for the private program it is $2.9\% - 2.1\% = 0.8\%$ and $0.5\% - 0.4\% = 0.1\%$ for the public program. In what follows, we therefore consider the effect of this imperfect compliance as negligible. We therefore maintain our interpretation of the IV parameters as Treatment on the Treated parameters (see section 3.1).

4.3 Treatment effect

Table 5 presents instrumental variable estimations of the model

$$y = \alpha_{Pub} T_{Pub} + \alpha_{Priv} T_{Priv} + Xb + u \tag{4}$$

where the public and private program treatment variables, T_{Pub} and T_{Priv}, are instrumented using the assignment variables Z_{Pub} and Z_{Priv}.

As the estimated intention-to-treat parameters were close, the large difference in the entry rates into each program is expected to translate into a substantial difference in the treatment effects.

The table reports estimates of α_{Pub} and α_{Priv} for the same three outcomes as above, with and without controls. The table also gives the counterfactual means, defined as the difference between the mean outcome of those who benefited from the program and the estimated effect. The probability of exiting from unemployment and finding a job within six months following random assignment increased by 10.2 percentage points for those who benefited from the public program. This is more than twice the effect of the private program, which is only 4.5 percentage points. The difference between the two program impacts is significant at the 10% level. The effect of the public program is substantial in absolute terms. It can be compared to the control group mean, which is 23.0%, or the counterfactual mean, which is 20.7%. Participation in the program implies an increase by roughly 50% in the chances of going back to employment. In comparison the 4.5 percentage point effect of participation in the private program represents an increase of only 22% of the counterfactual mean.[16]

The difference between the private and the public program narrows as we consider the two other employment outcome variables. This is interesting to notice, as these employment outcomes are closer to the requirements made in private operator's contract. For example, if we consider the last employment outcome "Employment eligible for payment," the participation in the public program increases the chances of finding a long-lasting job within six months by 7.2 percentage points and the participation in the private program increases the chances of finding such a job by 5 percentage points. However, even if the difference is no longer significant, for "Any employment" and "Employment eligible for payment," the difference remains positive.

Figure 1 presents the LATE results for the outcome variable "Exit to Employment" after 3, 6, 9 and 12 months. The figure clearly shows that the effect of the public program was already very large three months after random assignment and that it remains large over the whole period.

[16]Another usual way to gauge the size of an effect is to compare it to the standard deviation of the outcome variable. Here, the effect of the public program represents 24.3% of the standard deviation of the dependent variable and the effect of the private program, 10.7%.

One year after random assignment there still exists a positive effect, which is quite substantial. By contrast, the private program has a very small and insignificant effect initially, which then progressively increases. It is important to notice that the private program was introduced with the objective of having job-seekers rapidly back to employment so as to save on unemployment benefits. The figure clearly shows that the public program dominates the private one in that respect.

Figure 1 also compares the results of the previous LATE estimations with those obtained using control variable OLS regressions. The OLS regressions considered here are based on the whole sample using all three groups of assignment and the full set of control variables described in Table 2. OLS estimations yield biased estimates, especially in the short run. In the long run the bias is smaller but still quite large, even though confidence intervals overlap with those of the LATE estimates.

5 Why are private providers lagging behind?

Why do private operators have a weak effect in comparison to the public program? We see three main possible explanations for this difference. The first is that the contract structure private contractors have with the PES provides them with specific incentives. The second is that job-seekers may decide to enroll and to invest their time differently when they are assigned to private and public programs. The last one is that private contractors may master the counseling technology less efficiently. These different explanations can operate through two channels. The first is a selection effect: the populations entering the public and private programs may not be the same; the second is that programs may have different impacts on the very same populations.

In this section, we provide evidence that the second channel is at work: holding the population of recipients fixed, the public program still tends to be more effective. By contrast, we do not find

much evidence of the selection effect, i.e. that the public program enrolls in larger proportions job-seekers for whom counseling is predicted to be particularly effective, given their observable characteristics. We argue that this leaves a mix of technology differences and incentives issues as a likely explanation for the private program's weaker results.

5.1 Selection effects

One possible reason why the effects of the two programs appear different is selection: programs could have identical effects but the the population could be heterogeneous and the job-seekers entering programs would be different. This is more than just a statistical issue in the sense that, if programs' effects are different, it literally means that chances to find a job depend on the program a job-seeker enters. So there is a cost for individuals of entering the private program rather than the public program. On the other hand, if selection explains the difference in estimated LATEs, then the issue is, rather, targeting. It may be the case indeed that the private program focuses on the hardest to place job-seekers and offers them the most they can get from counseling.

There are various reasons why the private and public programs may not enroll the same job-seekers. A first reason may have to do with the incentives faced by private providers. Private companies are paid around 1,000 euros, when a job-seeker merely enters their program. This large upfront payment may provide them with substantial incentives to enroll any job-seeker they can. As a result, teams in the private program may have encouraged entrance of job-seekers for whom expected impact of counseling is weak. Counselors in the public program do not receive such incentives.[17]

A second possible reason for differential selection into the two programs lies in the behavior

[17]In the public program, which runs on a limited budget, enrolling more job-seekers does not bring additional resources.

of the job-seekers themselves. It has been shown that assignment to a program may act as a threat upon job-seekers. Both Black et al. (2003) and Rosholm and Svarer (2008) find evidence that assignment to a program as such may indeed speed up a person's exit from unemployment. In contrast, Crepon et al. (2010) do not find such an effect. The job-seeker's decision to enter a program may depend on his/her perception of costs and benefits and this perception might be different for private and public programs.[18]

On the empirical side, there is also evidence that selection is potentially an issue. Indeed, both the program effects and enrollment decisions are heterogeneous in the population. Table 6 presents the estimated program effects when the sample is split by gender, age categories and reasons for unemployment, and it points to a substantial heterogeneity in the impact of the program. First, the programs appear to be mostly effective on women.[19] We also observe large differences in effects with respect to age, the programs being strongly effective for young people (aged below 30). This is interesting as counseling programs usually have little effect on young people (Card, Kluve and Weber, 2010). A last dimension of heterogeneity has to do with the reason for becoming unemployed. Here, we isolate layoffs for personal reasons, as they may be perceived as a bad signal and as the workers thus laid off are usually harder to place. As can be seen from the table, the effect of the program in the long run is positive and large on this category of job-seekers for both programs. The effect on the other type of job-seekers is far smaller and not significant in the long run.

In order to examine how strongly the selection of different types of job-seekers explains the

[18]Using the survey of participants carried out by the French Ministry of Labor, Gratadour and Le Barbanchon (2009) provide some information about the timing of entry into the programs. Information is weak, however, as the surveyed population that did not enter the programs was under-sampled and only involved around 300 individuals. It shows that around 50% of the selection occurs early in the entry process, in much the same way for both types of programs. These figures imply that self-selection might be an explanation for the low take-up rate into programs but less of an explanation for the differential in take-up rates between the two programs.

[19]This is consistent with results reviewed by Bergemann and Van den Berg (2008), who find that labor market interventions usually have either the same effect for men and women or a stronger effect for women than for men. As an exception, Crepon et al. (2013) find that a reinforced counseling program for young graduate people and find large effects for men and no effect for women.

differential in programs' impacts, we decompose the difference between the two LATE program impacts into a selection effect and a difference between program impacts over a same population:

$$E\left(\Delta_{priv}|Priv\right)-E\left(\Delta_{pub}|Pub\right)=\left[E\left(\Delta_{priv}|Priv\right)-E\left(\Delta_{priv}|Pub\right)\right]+\left[E\left(\Delta_{priv}|Pub\right)-E\left(\Delta_{pub}|Pub\right)\right] \tag{5}$$

where Δ_{priv} is private (reps. public) program impact and the conditioning is on the private (resp. public) program entrant population. The first term in this decomposition is the selection effect. It measures the difference between the impact of the private program across two different populations: those entering the private program and those entering the public program. The second term in the decomposition is the difference between the two program impacts on a given population: that of public program entrants. Such a decomposition requires to compute an estimate of the private program effect on the population of public program entrants. We show in the Appendix that under a weak form of conditional independence assumption, such an effect can be simply estimated using outcome variables weighted by a ratio of propensities to enter the public and private programs conditional on a set of covariates.[20] This is similar to the decomposition proposed by DiNardo, Fortin and Lemieux (1996).

Table 7 presents the results of such a decomposition. The first and third lines simply replicate estimated programs' impacts on their entrants from Table 5, while the second line reports the estimated impact of the private program on public program's entrants. The last three lines present the decomposition. As can be seen from the table the selection effect is always small and not statistically different from zero. This tells us that selection is not a first-order issue driving the observed difference between the two programs. Notice, however, that it is always positive: therefore the difference between the two programs widens if evaluated on the population of public

[20] In substance, that hypothesis requires that, conditional on a set of covariates and being an entrant in *some* program, the impact of the private program would be on average similar for private and public entrants.

program entrants. For the first outcome variable, we notice that this leads to the difference being now statistically significant at the 5% level.

5.2 Explaining differential effects of the two programs on similar populations

In this section we want to address the difference in program efficiency for similar job-seekers that would be enrolled in either program. Three main explanations for these differences are examined: contract incentives, the counseling technology and job-seeker motivation.

When looking at contract incentives as a potential explanation for the observed differences between the two programs, a natural heterogeneity dimension to focus on is employability. Private contractors receive about 2,000 euros from placing a job-seeker in a job. They may find it optimal to focus their effort on the least employable and rely on the high chances of the most employable job-seekers to find a job by themselves and generate payment. Such a behavior is known as parking and consists in enrolling job-seekers but denying them the services they are supposed to receive. It could occur here for the most employable job-seekers if they invest more in the search process and if, in addition, the search process exhibits decreasing returns.[21]

We measure employability using exit towards employment after six months for job-seekers assigned to the standard track, using a logit model based on the set of covariates listed in Table 2. Regression results are presented in Table A-1 and the left hand-side panel of Figure 2 shows the distribution of the predicted exit on the basis of this model for the control group and the private program entrants. As can be seen from the figure, the propensity to exit from unemployment spontaneously is widely dispersed in the population. Moreover, the figure also shows that the

[21]There is also qualitative evidence that parking behavior occurred in the private program. Divay (2009) independently conducted interviews with caseworkers and managers in two private providers' offices. She reports that although caseworkers do not talk about parking, they describe a highly standardized procedure that clearly saves on the private providers' main cost: the caseworkers' time. Each job-seeker meets with the caseworker weekly, for a well defined thirty-minute sequence that one caseworker describes in the following terms: "Offer coffee, review last week's objectives, suggest actions, perhaps make a call, set new objectives for the coming week, arrange a new appointment, and accompany the person to the door. This is the procedure!" (Divay, 2009).

distribution over the private program entrants is almost the same as the distribution over the control group. When looking at the mean gap between the two populations, we indeed find a very small and insignificant difference. It implies that there was no cream-skimming from the private program. The figure on the right hand-side panel also reports the distribution of employability among private and public program entrants. Although differences are also small, they are however larger, the public program entrants distribution being slightly right shifted. When looking at the difference in means between public and private entrants, we find a small positive difference significant at the 5% level. This indicates that, cream-skimming occurred more for the public than for the private program, if at all.

Noting E employability, we estimate an equation in which program participation variables are interacted with the measure of employability and its square:

$$\begin{aligned} y =&(\alpha_{0,pub} + \alpha_{1,pub}E + \alpha_{2,pub}E^2)T_{pub} \\ &+(\alpha_{0,priv} + \alpha_{1,priv}E + \alpha_{2,priv}E_i^2)T_{priv} + Xb + a_1E + a_2E^2 + u \end{aligned} \tag{6}$$

using the six instrumental variables Z_p, Z_pE and Z_pE^2 with Z_p being assignment to private and to public programs. Results are presented in Table 8. The table gives the coefficients of employability of first- and second-order power.[22] A clear difference between the two programs comes to light. The effect of the public intervention does not depend on employability; the test largely accepts the hypothesis of homogeneity no matter what outcome variable is selected. Conversely, the effect of the private intervention depends strongly and significantly on employability: the greater the degree of employability, the less impact the program has. One important point to note is that the difference between the private and the public program appears more clearly. Indeed, we do find a significant difference (at the 10% threshold) for the two last employment

[22]Employability is normalized before taking the square, and the two variables are centered before being interacted with the treatment variables.

outcomes whereas we did not observe any such difference in Table 5. In the end, we can conclude that workers with similar expected employability benefited differently from the two program in a way consistent with private contractors' incentives.

However, it is unlikely that contract's incentives alone explain the difference between private and public programs effects for a population that both programs would enroll. The mastering of the supervision technology by heterogeneous private operators is a natural alternative candidate. To address it, we look at heterogeneity of effects with respect to the type of private contractors: temporary help agencies, consultancies specialized in the placement of unemployed job-seekers, and large international job placement firms. There is no data that allows us to characterize the services provided by the different operators during the program, and qualitative evidence suggests that they broadly followed the same approach. However, temporary help agencies have long been recognized as likely to play an important part in placing people with little labor market prospects into work (Katz and Krueger, 1999), although this suggestion has not received strong empirical validation (Autor and Houseman, 2010). Consultancies specialized in the placement of workers after mass layoffs may have an advantage in coaching practices, as placement under these circumstances often involves helping the job-seeker shift to a new type of job or a new industry. Large international job placement firms may have a similar expertise in counseling practices, but less knowledge of the local labor market context.

One caveat when analyzing the comparative performance of private operators by type, is that they work in areas that are not chosen at random, and with specific re-employment opportunities.[23] To address this issue, we measure each private program effect *relative to the public program effect in the same area*. The results are displayed in Table 9. For each outcome variable taken into consideration, the table first presents the results of the private program for each

[23]Table A-1 shows that the chances of returning to work in regions where temporary help agencies or job placement firms operate are higher than in regions where consultancies are involved.

type of operator. It then presents the results of the public program in the corresponding area. Finally the public-private difference is displayed. As the table shows, the different contractors have quite homogeneous performances. For the first two employment outcomes considered the test of a same effect is accepted by large. But, they operate in different contexts: this is captured by the heterogeneous impacts of the local public program: p-values for the homogeneity test are close to or less than 10% for each of the three employment outcomes. As a result, looking at the column labeled "difference," we can see that temporary help agencies are significantly less effective than consultancies, with international placement firms in-between.

Finally, the impact of counseling programs also depends on job-seekers' efforts and how closely they stick to the program. Programs of strengthened supervision aim at keeping the job-seeker's commitment strong, and, although this is not their main point, they also have the goal of checking to see that he/she is not slacking off. The main menace is to have someone struck off the program, which happens rarely in France, and entails the loss of unemployment benefits for the job-seeker. Private operators could use that threat. However, their terms of reference did not include the task of sanctioning inactive job-seekers, and private companies had no incentives to exclude a job-seeker from the program, as they could hope that she could still find a job, triggering the conditional payment.

In order to study this point, we estimate the impact of the two programs on different types of exit from the PES files in Table 10 (not just to employment). Results clearly show how important is the distinction between exit to employment and exit from PES files (Card, Chetty and Weber, 2007). In particular, the private program has a negative effect on exit from PES files after six months, which is due to a large and significant negative impact on exits without a job, outweighing the positive impact on exits to employment documented in the previous sections. Exits without a job belong to two broad categories: sanctions and withdrawal from the labor

force (due to retirement, training, health issues, maternity or parental leave). As shown in column 4 of Table 10, exclusion from unemployment benefits for not fulfilling one's obligations are significantly *less* frequent when the job-seeker is enrolled in the private program. In fact, virtually no job-seeker exits the unemployment register as a result of administrative sanction when her case is managed by the private program. Similarly, withdrawal from the labor force is less frequent when the job-seeker is enrolled in the private program (a 5.1 percentage point decrease). Regarding the public program, we do not find any impact on sanctions. The estimated impact on withdrawal from the labor force is sizable, but only marginally significant. To sum up we can conclude from these estimates either that job-seekers were not less motivated when enrolled in the private program than when enrolled in the public program or that caseworkers in the private program did not apply sanctions.

Overall, the additional evidence presented in this section suggests that the incentives set by the private providers' contract, as well as their incomplete mastery of the counseling technology may explain the lower impact of the private program. Problems with mastering the counseling technology may have to do with the fact that this was the first large-scale attempt in France to contract these services to private providers.[24] In contrast, the private providers' contracts are not specific to the French experience – they are similar to those frequently used in other OECD countries over the period.[25]

[24]However, the question of how to get rid of providers who do not perform well and learn too slowly is common to all countries; see OECD (2012) on Australia.

[25]For instance, contract providers in UK "Employment Zones" received a three-tier payment (at enrollment, at job entry and after 13 weeks of job retention). Contracts in Germany varied, but often resembled the French ones – e.g. a contract with Ingeus, an early and influential private provider in the French job-seeker placement market, involved a 56% upfront payment, 10% on job placement, 17% after 13 weeks and 17% after 26 weeks of sustained employment. In the Netherlands, even though fully conditional payments ("no cure, no pay") were introduced progressively in the mid-2000s, the typical "no cure, less pay" contract involved a 10 or 20% payment on completion of an agreed action plan, a fixed payment of about 40% six months after commencement, and another 40% or 50% after two months in a job with a minimum six-month contract (Finn, 2011).

6 Other Outcomes and Elements of Cost-Benefit Analysis

In this section we present the results of cost-effectiveness analysis and our tentative cost-benefit analysis. Unfortunately, we had no access to information that would have enabled us to perform a whole cost-benefit analysis as performed, for example, in labor market programs evaluation analyzed in Meyer (1995). This would require unemployment benefits and wage earnings data, and we were denied access to both these data sources. Unemployment benefits data can however be approximated, and this enables us to compute a rough back-of-the-envelope cost-benefit analysis for the PES, ignoring however benefit arising from taxes collected on wage earnings. As mentioned earlier, the rule set by the private program is that eligible job-seekers must have at least 365 days of payment eligibility at the time of randomization. Therefore, we can simply count the number of days those persons spent unemployed (even over different spells) during a year. The measure can be refined to exclude days during which the job-seeker was registered as unemployed while working (on a part-time basis). This gives us information about the number of days unemployment benefits have been paid.

To convert this number into amounts of unemployment benefits, we construct a rough measure of daily benefits multiplying the target wage reported by each job-seeker by common values of replacement rates.[26] The costs of the private program are computed assuming a conservative maximum payment of 3000 euros and using the contract structure: 30% of the fee paid when the job-seeker enters the program, 35% if she finds a job within six months (using the "Any employment" outcome) and 35% if she stayed six months in employment (using the "Employment

[26]More precisely, we used 72%, 68% or 64% as replacement rates for respectively wages below 1300 euros, between 1300 and 1900 euros, and above 1900 euros. We consider the mean value of the wage category and assumed a wage target of 2600 euros for job-seekers with a wage target above 2200 euros. For the roughly 10% of job-seekers with no wage target, we imputed a value based on the predicted probabilities to have a wage target of each possible types using an ordered logit model of the wage target when it is available using all the covariates listed in Table 2.

eligible for payment" variable). Costs of the standard track and the public program are estimated at 120 and 657 euros, respectively.[27]

Table 11 presents program effects computed using these variables. The upper panel presents intention-to-treat estimates and the lower panel presents local average treatment effects. The private program clearly performs poorly both in absolute terms and compared to the public program. First, it increases assistance costs by 524 euros per assigned job-seeker and 1321 euros by job-seeker entering the program. This is largely above the corresponding cost impacts of the public program (172 euros and 539 euros, per assigned job-seeker and enrolled job-seeker, respectively). Second, the effect of the private program on the number of days on the lists is almost zero and non-significant. This is mainly due to the fact that the program has no significant effect on exit from the lists as explained in the previous section. In contrast, the public program reduces the number of days on the list for those who entered the program by 20.6 days, and by 6.6 days for those assigned to the program. These results are consistent with the reduction of 0.5 to 4 weeks reported in Meyer (1995). For 1000 euros invested in the program, the public program reduces the number of days on the list by $6.6/172 \times 1000 = 38$ days, while the private programs reduces it by $0.4/524 \times 1000 = 1$ day only. Using our crude measure of unemployment benefits payments, this implies that the private program saves a small and non-significant amount (50 euros) per assigned job-seeker while the public program saves 589 euros per assigned job-seeker (1723 per enrolled job-seeker). Putting costs and benefits together, the private program implies an increase in total costs (benefits and assistance), significant at the 10% level, of 474 euros per assigned job-seeker (and 1162 euros per enrolled job-seeker). We should note, however, that

[27]It is not easy to compute the cost of programs organized in-house by the PES, because the PES has not developed the accounting tools to identify the cost of each component of services. Accounting services of the PES provided us with estimates of the overall cost of the public program: counselor wages, equipments, rents (as the public program teams were usually located in separate buildings), and *support costs*. This information can be used to compute a per-participant cost. We got similar information about the costs of the standard track.

even if the public program performs better, its impact on total expenses is not well measured. The point estimates suggests some savings, but this is not significantly different from zero.

7 Conclusion

In this paper we analyze the impact of a reinforced counseling program for at risk of long-term unemployment job-seekers when offered by private contractors or by the public employment service. The entry of private contractors into the market of job-seeker placement was the consequence of the end of the monopoly held by the public employment service. It was accompanied by quite optimistic expectations about associated employment gains.

To sum up, counseling is found to be effective (in the sense of accelerating placement), but private providers appear to be less efficient, especially in the short run, and much less cost-effective than the public program. Our crude cost-benefit analysis clearly shows that the private program is on average ineffective and leads to a net cost per job-seeker. In contrast, the public program reduced that net cost.

This difference does not come from differential selection of job-seekers into each program, whereby the private program would have attracted harder-to-treat individuals. It thus reflects lower effectiveness of the private providers. These results can be understood by looking at the difference between the way the employment services market was opened in Australia and how this was done in other countries. The Australian quasi-market was characterized by regulation and the implementation of a strong monitoring procedure (OECD, 2012). Specific tools were developed to identify low-performing providers and to prevent private providers from parking some of their assigned job-seekers. Although the validity of these tools might be questionable and they may have distortion effects (Heckman, Heinrich and Smith, 2002), they do the first-order job of making providers aware that the quality of their services is observed. This is in sharp contrast

with the procedure that has been followed in other countries – and in the present case – which consisted mainly of organizing bidding processes without any attempts to measure performance. It is not surprising that our analysis leads us to see the mastering of the counseling technology and parking as the main potential explanations. The solution adopted by most countries when dealing with the opening of employment services markets seems to miss the objective, which is to improve the quality of the services job-seekers may benefit from and lower their costs.

This point is implicitly acknowledged by a recent review commissioned by the European Commission: "The development and management of subcontracting systems is a complex task for policy makers and public officials. There is a sharp and continuous 'learning curve' and it takes time to learn how to steer the system to minimize perverse incentives and to capture the efficiencies and innovation that independent contractors may offer. It may be that the gains from subcontracting emerge over time when, as in Australia, public officials can exclude poorer performers, increase competition and improve the performance management of subcontractors" (Finn, 2011).

Still, in the short run, this paper, together with other recent research, shows that there is no prima facie case that private provision ensures a better or less expensive service or that market discipline naturally emerges as an efficiency-enhancing feature. Policy enthusiasm for outsourcing placement services still lacks support from rigorous research.

References

Andersson, Fredrik, and Henrik Jordahl. 2011. "Outsourcing Public Services: Ownership, Competition, Quality and Contracting." Research Institute of Industrial Economics Working Paper Series 874.

Angrist, Joshua D., and Jörn-Stephen Pischke. 2009. "Mostly Harmless Econometrics: An Empiricist?s Companion." *Statistical Papers*, 52(2): 503–504.

Angrist, Joshua D., Guido Imbens, and Donald Rubin. 1996. "Identification of Causal Effects Using Instrumental Variables." *Journal of the American Statistical Association*, 91(434): 444 – 455.

Ashenfelter, Orley, David Ashmore, and Olivier Dêschenes. 2005. "Do Unemployment Insurance Recipients Actively Seek Work? Evidence from Randomized Trials in four U.S. States." *Journal of Econometrics*, 125(1-2).

Autor, David, and Susan Houseman. 2010. "Do Temporary Help Jobs Improve Labor Market Outcomes for Low-Skilled Workers? Evidence from 'Work First'." *American Economic Journal: Applied Economics*, 2(3): 96–128.

Behaghel, Luc, Bruno Crepon, and Marc Gurgand. 2012. "Private and Public Provision of Counseling to Job-Seekers : Evidence from a Large Controlled Experiment." *IZA Discussion Paper*, 6518.

Behaghel, Luc, Bruno Crepon, and Marc Gurgand. 2013. "Robustness of the encouragement design in two treatment RCTs." *Working paper, IZA*.

Bennemarker, Helge, Erik Grönqvist, and Björn Öckert. 2009. "Effects of outsourcing employment services: evidence from a randomized experiment." IFAU - Institute for Evaluation of Labour Market and Education Policy Working Paper Series 2009:23.

Bergemann, Annette, and Gerard J. Van den Berg. 2008. "Active Labor Market Policy Effects for Women in Europe ? A Survey." *Annales d'Economie et de Statistique*, 91/92: 385–408.

Bernhard, Sarah, and Joachim Wolff. 2008. "Contracting out placement services in Germany : is assignment to private providers effective for needy job-seekers?" IAB Discussion Paper 200805.

Besley, Timothy, and Maitreesh Ghatak. 2001. "Government Versus Private Ownership Of Public Goods." *The Quarterly Journal of Economics*, 116(4): 1343–1372.

Black, Dan A., Jeffrey A. Smith, Mark C. Berger, and Brett J. Noel. 2003. "Is the Threat of Reemployment Services More Effective Than the Services Themselves? Evidence from Random Assignment in the UI System." *American Economic Review*, 93(4): 1313–1327.

Bloom, Howard S. 1984. "Accounting for No-shows in Experimental Evaluation Designs." *Evaluation Review*, 8: 225–246.

Blundell, Richard, Monica Costa Dias, Costas Meghir, and John Van Reenen. 2004. "Evaluating the Employment Impact of a Mandatory Job Search Program." *Journal of the European Economic Association*, 2(4): 569–606.

Capelier, T., and R. Mizrahi. 2008. "L'accompagnement renforcé des demandeurs d'emploi. L'évaluation qualitative de la mise en oeuvre des expérimentations." *Report to the French Minister of Labor*.

Card, David, Jochen Kluve, and Andrea Weber. 2010. "Active Labour Market Policy Evaluations: A Meta-Analysis." *The Economic Journal*, 120(548): F452–F477.

Card, David, Raj Chetty, and Andrea Weber. 2007. "The Spike at Benefit Exhaustion: Leaving the Unemployment System or Starting a New Job?" *American Economic Review*, 97(2): 113–118.

Crepon, Bruno, Esther Duflo, Marc Gurgand, Roland Rathelot, and Philippe Zamora. 2013. "Do Labor Market Policies have Displacement Effects? Evidence from a Clustered Randomized Experiment." *Quarterly Journal of Economics*, 128(2): 531–580.

Crepon, Bruno, Marc Ferracci, Gregory Jolivet, and Gerard J. Van den Berg. 2010. "Analyzing the Anticipation of Treatments with Data on Notification Dates." Centre de Recherche en Economie et Statistique Working Papers 2010-41.

DiNardo, John, Nicole M. Fortin, and Thomas Lemieux. 1996. "Institutions and the Distribution of Wages, 1973-1992: A Semi-parmetric Approach." *Econometrica*, 64: 1001–1044.

Divay, Sophie. 2009. "Nouveaux opérateurs privés du service public de l'emploi. Les pratiques des conseillers sont-elles novatrices?" *Travail et Emploi*, , (119).

Dolton, Peter, and Donal O'Neill. 1996. "Unemployment Duration and the Restart Effect: Some Experimental Evidence." *Economic Journal*, 106(435): 387–400.

Dolton, Peter, and Donal O'Neill. 2002. "The Long-Run Effects of Unemployment Monitoring and Work-Search Programs: Experimental Evidence from the United Kingdom." *Journal of Labor Economics*, 20(2): 381–403.

Duflo, Esther, Rachel Glennerster, and Michael Kremer. 2008. "Using Randomization in Development Economics Research: A Toolkit." , ed. T. Paul Schultz and John A. Strauss Vol. 4 of *Handbook of Development Economics*, Chapter 61, 3895–3962. Elsevier.

Finn, Dan. 2011. "Sub-contracting in Public Employment Services. Review of Research Findings and Literature on Recent Trends and Business Models." *Report to DG Employment, Social Affairs and Inclusion at the European Commission*.

Gratadour, Celine, and Thomas Le Barbanchon. 2009. 41(2).

Graversen, B Krogh, and Jan C. van Ours. 2008. "Activating unemployed workers works; Experimental evidence from Denmark." *Economics Letters*, 100(2): 308–31.

Grossman, Sanford J, and Oliver D Hart. 1986. "The Costs and Benefits of Ownership: A Theory of Vertical and Lateral Integration." *Journal of Political Economy*, 94(4): 691–719.

Hägglund, Pathric. 2009. "Experimental evidence from intensified placement efforts among unemployed in Sweden." IFAU - Institute for Evaluation of Labour Market and Education Policy Working Paper Series 2009:16.

Hart, Oliver, Andrei Shleifer, and Robert W. Vishny. 1997. "The Proper Scope of Government: Theory and an Application to Prisons." *Quartely Journal of Economics*, 122(4): 1127–61.

Heckman, James J., Carolyn Heinrich, and Jeffrey Smith. 2002. "The Performance of Performance Standards." *Journal of Human Resources*, 37(4): 778–811.

Jönsson, Lisa, and Peter S. Thoursie. 2012. "Does privatisation of vocational rehabilitation improve labour market opportunities? Evidence from a field experiment in Sweden." IFAU - Institute for Evaluation of Labour Market and Education Policy Working Paper Series 2012:2.

Katz, Lawrence, and Alan Krueger. 1999. "The High-pressure U.S. Labor Market of the 1990s." Princeton University, Department of Economics, Industrial Relations Section. Working Papers 795.

Krug, Gerhard, and Gesine Stephan. 2011. "Is Contracting-out Intensified Placement Services More Effective than In-House Production? Evidence from a Randomized Field Experiment." LASER Discussion Paper no 51.

McConnell, Sheena, Andrew Burnwick, Irma Perez-Johnson, and Pamela Winston. 2003. "Privatization in Practice: Case Studies of Contracting for TANF Case Management." *Final Report prepared by Mathmatica Policy Research Inc. for the Office of the Assistant Secretary for Planning and Evaluation, U.S. Department of Health and Human Services, Washington DC.*

Meyer, Bruce. 1995. "Lessons from the U.S. Unemployment Insurance Experiments." *Journal of Economic Literature*, 33(1): 91–131.

OECD. 2012. "Activating Jobseekers: How Australia Does It." *OECD Publishing.*

Rosholm, Michael. 2008. "Experimental Evidence on the Nature of the Danish Employment Miracle." Institute for the Study of Labor (IZA) IZA Discussion Papers 3620.

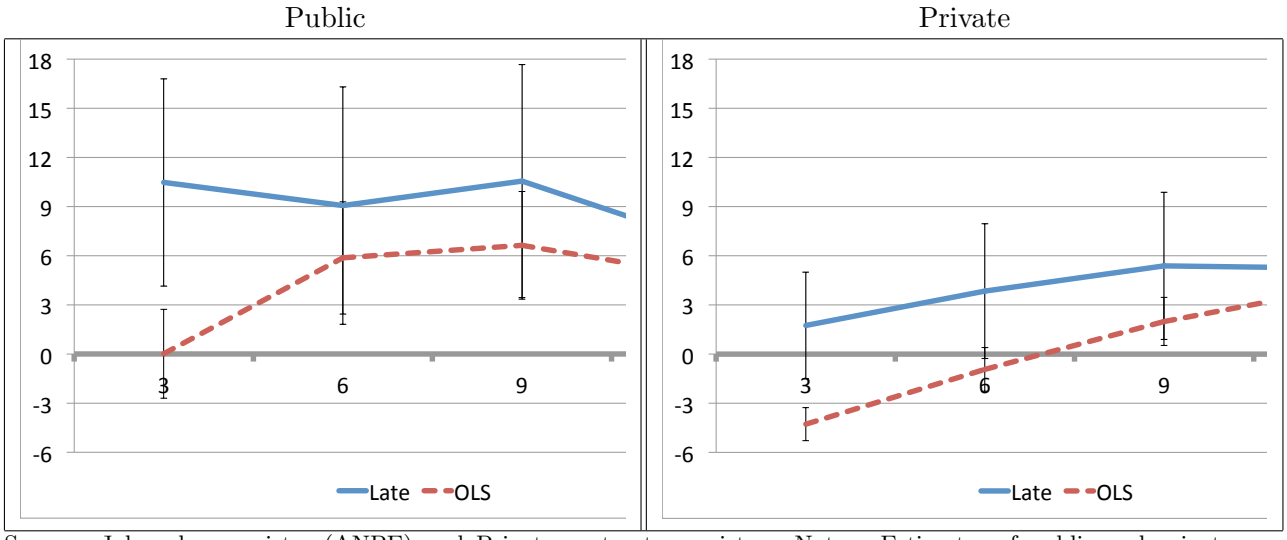
Rosholm, Michael, and Michael Svarer. 2008. "The Threat Effect of Active Labour Market Programmes Scandinavian Journal of Economics." *Scandinavian Journal of Economics*, 42.

Shleifer, Andrei. 1998. "State versus Private Ownership." *Journal of Economic Perspectives*, 12(4): 133–150.

Van den Berg, Gerard J., and Bas Van der Klaauw. 2006. "Counseling And Monitoring Of Unemployed Workers: Theory And Evidence From A Controlled Social Experiment." *International Economic Review*, 47(3): 895–936.

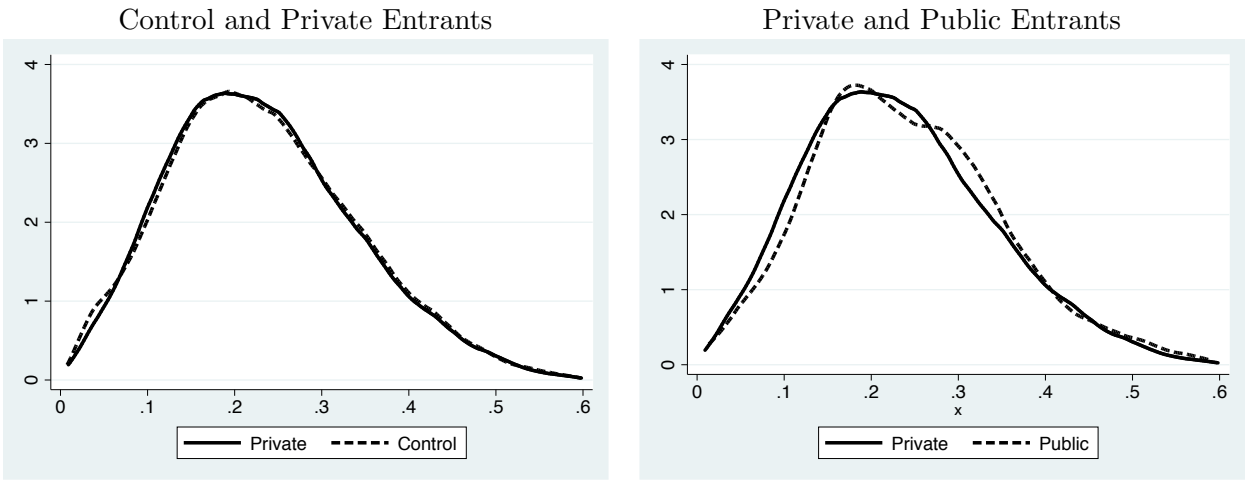
Vivès. 2009. "Emplois et politiques sociales." *ed. T. Barnay and F. Legendre XXIX journ Ìees de l'association d' Ìeconomie sociale.*

Figure 1: LATE and OLS estimates for public and private programs



Source: Job-seekers register (ANPE) and Private contractor register. Notes: Estimates of public and private program impacts 3, 6, 9 and 12 months after assignment, with 95% confidence intervals. "Exit from PES registers to employment" is the outcome. The LATE and OLS estimates use the control variables listed in Table 2. The LATE estimates use program assignment as an instrument for program entry (see Table 5).

Figure 2: Densities of the employability index



Source: Job-seekers register (ANPE), Private contractor register and surveys of job-seekers with unknown exit. Notes: The employability index is based on a weighted logit model of exit to unemployment on observed variables in the control group, see Table A-1 for the regression.

Table 1: Survey on employment outcomes and attrition

	Experimental sample	Exits from unemployment registers			Survey				Final attrition
		All	Unknown exit		Sampling		Respondents		
	#	#	#	%	#	%	#	%	%
	(a)	(b)	(c)	(c/b)	(d)	(d/c)	(e)	(e/d)	$(\frac{c}{a}(1-\frac{e}{d}))$
Assignment									
Private program	36027	12864	6274	49%	1980	32%	996	50%	9%
Public program	3385	1322	647	49%	647	100%	331	51%	9%
Standard track	4565	1690	881	52%	881	100%	473	54%	9%
All	43977	15876	7802	49%	3508	45%	1800	51%	9%

Source: Job-seekers register (ANPE), Private contractor register and surveys of job-seekers with unknown exit. Notes: The table provides information about assignment and surveys. Each line corresponds to a different assignment (private program, public program or standard track). Column (a) gives the number of job-seekers assigned to each group. Columns (b), (c) and (c/b) give the number of job-seekers leaving unemployment, the number of them leaving unemployment with unknown exit and their share. Column (d) and (d/c) give the number and share of surveyed job-seekers with unknown exit. Column (e) and (e/d) give the number of respondents and the response rate. The last column gives the share of job-seekers whose reason for exit remains unknown after the survey.

Table 2: Balancing tests

Variable Names	(1) Standard	(2) Private	(3) Public	(4) (2)=(1)	(5) (3)=(1)	(6) (3)=(2)=(1)
College education	30.9	31.9	31.8	.	.	.
Bac	20.5	19.5	18.9	.	.	.
Vocational	29.6	29.8	30.6	.	.	.
High school dropout	19.0	18.8	18.7	.	.	.
Manager	13.8	13.1	13.1	.	.	.
Technician	9.9	10.4	10.3	.	.	.
Skilled clerical worker	48.0	48.1	48.6	.	.	.
Unskilled clerical worker	13.5	14.4	13.7	.	.	.
Skilled blue collar	9.9	9.2	9.4	.	.	.
Unskilled blue collar	5.0	4.8	4.9	.	.	.
age below 26	15.3	16.1	16.4	.	.	.
Aged 26 to 35	35.1	34.6	33.5	.	.	.
Aged 36 to 45	25.3	25.8	26.3	.	.	.
Aged 46 to 55	19.5	19.1	19.2	.	.	.
Aged above 56	4.9	4.4	4.6	.	.	.
Woman	51.4	49.9	49.5	*	.	.
Married	45.7	46.2	46.2	.	.	.
No child	55.6	54.3	55.8	.	.	*
One child	18.2	18.5	18.6	.	.	.
More than one child	26.3	27.2	25.6	.	.	*
French	81.4	81.3	82.8	.	.	.
African	11.5	11.7	11.0	.	.	.
Other Nationality	7.1	7.0	6.2	.	.	.
Paris region	80.1	80.9	78.7	.	.	**
North	10.2	9.9	11.1	.	.	*
Other regions	9.6	9.2	10.2	.	.	.
Employment component level 1	22.9	23.0	22.1	.	.	.
Employment component level 2	56.1	55.5	57.1	.	.	.
Employment component missing	21.1	21.5	20.8	.	.	.
Economic Layoff	12.9	12.3	12.7	.	.	.
Personal Layoff	40.4	40.8	42.1	.	.	.
End of Fixed Term Contract	23.3	23.9	23.9	.	.	.
End of Temporary Work	5.7	5.1	5.1	.	.	.
Other reasons of unemployment	17.8	17.8	16.1	.	*	*
No exp in the job	14.3	15.0	14.3	.	.	.
1 to 5 years of exp in the job	44.4	44.4	44.1	.	.	.
More 5 years of exp in the job	41.3	40.7	41.6	.	.	.
Statistical risk level 2	38.8	39.3	39.7	.	.	.
Statistical risk level 3	36.4	35.0	36.0	*	.	.
Other Statistical risk	24.7	25.6	24.3	.	.	.
Search for a full time position	91.9	92.1	92.3	.	.	.
Sensitive suburban area	13.5	13.6	13.5	.	.	.
Wage target 1200-1349 euros	29.9	29.9	31.8	.	*	.
Wage target 1350-1549 euros	17.7	17.0	16.8	.	.	.

Continued on next page...

... table 2 continued

Variable Names	(1) Standard	(2) Private	(3) Public	(4) (2)=(1)	(5) (3)=(1)	(6) (3)=(2)=(1)
Wage target 1550-1799 euros	8.9	8.6	8.2	.	.	.
Wage target 1800-2200 euros	16.4	16.8	16.1	.	.	.
Wage target 2200 euros	17.7	17.3	18.2	.	.	.
No wage target	9.3	10.4	8.9	**	.	***
First unemployment spell	61.6	62.3	59.4	.	*	***
Insertion firm region	28.0	27.6	27.9	.	.	.
Temporary help region	35.3	35.9	38.0	.	**	**
Counseling firm region	36.7	36.5	34.2	.	**	**
Assigned first quarter	16.7	17.2	17.2	.	.	.
Assigned second quarter	34.9	35.3	35.3	.	.	.
Assigned third quarter	26.5	25.7	25.7	.	.	.
Assigned fourth quarter	26.5	25.7	25.7	.	.	.
Observations and joint test						
Whole sample	4565	36027	3385	.45	.54	.12
Temporary help region	1838	12632	1360	.94	.46	.29
Insertion firm region	1190	10051	932	.49	.96	.97
Counseling firm region	1537	13344	1093	.07*	.97	.29

Source: Job-seekers register (ANPE), Private contractor register. Notes: Upper panel: column (1), (2) and (3) report the mean value of variables over the sample of job-seekers assigned respectively to standard counseling, private counseling and public counseling. Columns (4) and (5) present significance levels for balancing tests comparing assigned to private or public counseling and assigned to the standard counseling scheme (*: significant at 10% level; **: significant at 5% level; ***: significant at 1% level). Column (6) presents the result of the joint balancing test over the three assigned populations. Lower panel: Columns (4) and (5) present p-values for the joint nullity tests of all coefficients, when regressing assignment to the private program (column (4)) or to the public program (column (5)) on all covariates listed in the upper panel. Column (6) presents p-values for the joint nullity test of all coefficients in both regressions. This is performed for the whole sample, or on subsamples of job-seekers registered in regions where the private contractor is of each of the listed types.

Table 3: Intention-to-treat effects on employment outcomes, after six months

	Exit from PES registers to employment		Any employment		Employment eligible to payment	
Assigned public	3.2***	2.8**	2.3*	2.0	2.2**	2.4**
	(1.2)	(1.2)	(1.3)	(1.2)	(1.1)	(1.1)
Assigned private	1.8**	1.5*	1.9**	1.6*	2.0**	1.8**
	(0.9)	(0.8)	(0.9)	(0.9)	(0.8)	(0.8)
Controls	no	yes	no	yes	no	yes
p-value private vs. public (%)	15.9	27.0	72.1	91.8	81.2	64.8
control mean	23.0	23.0	35.8	35.8	21.4	21.4

Source: Job-seekers register (ANPE), Private contractor register and surveys of job-seekers with unknown exit. Notes: Each column reports the results of weighted OLS regressions of an employment outcome variable, within six months of random assignment, on assignment variables. The first outcome ("exit from PES registers to employment") combines exit from the registers with information on employment status based on the job-seeker follow-up. The second outcome ("any employment") extends the former definition to account for jobs held while unemployed as long as they lasted at least 110 hours. The last outcome restricts the former category to exits to employment or jobs held while unemployed that lasted at least six months. For each outcome variable we consider weighted OLS regression with or without the covariates listed in Table 2. Weights are based on the assignment scheme and the sampling scheme of job-seekers with unknown exit. Robust standard errors in parentheses, 37,952 observations.

Table 4: First stage regression

Enter	Public		Private	
Assigned public	32.1***	32.0***	2.1***	1.5***
	(0.9)	(0.9)	(0.3)	(0.3)
Assigned private	0.5***	0.5***	42.8***	42.9***
	(0.1)	(0.1)	(0.4)	(0.3)
Assigned standard track	0.4***	0.4***	2.9***	2.8***
	(0.1)	(0.1)	(0.3)	(0.3)
Controls	No	Yes	No	Yes

Source: Job-seekers register (ANPE), Private contractor register and surveys of job-seekers with unknown exit. Notes: Weighted OLS regression of entry into public and private programs within six months of random assignment (first stage regressions of Table 5). Column (1) displays first-stage estimates for the public program without controls and intercept, column (2) adds the (centered) covariates listed in Table 2. Weights are consistent with outcome variables, see Table 3. Column (3) and (4) present the same results for entry into the private scheme. Regressions include the covariates listed in Table 2. Outcome variables defined in Table 3. Robust standard errors in parentheses, 37,952 observations.

Table 5: Local average treatment effects, after six months

	Exit from PES registers to employment		Any employment		Employment eligible to payment	
Enter public	10.2***	9.1**	7.3*	6.5*	7.2**	7.7**
	(3.8)	(3.7)	(4.1)	(3.9)	(3.6)	(3.5)
Enter private	4.5**	3.8*	4.8**	4.0*	5.0**	4.4**
	(2.1)	(2.1)	(2.3)	(2.2)	(2.1)	(2.0)
Controls	no	yes	no	yes	no	yes
p-value private vs. public (%)	6.3	7.8	43.1	42.0	45.9	23.0
control mean	23.0	23.0	35.8	35.8	21.4	21.4
counterfactual mean pub	20.7	21.9	38.2	39.0	21.4	20.9
counterfactual mean priv	19.7	20.4	34.3	35.0	18.4	19.1

Source: Job-seekers register (ANPE), Private contractor register and surveys of job-seekers with unknown exit. Notes: Weighted two-stage least square regressions of employment outcome variables on treatment variables, using assignment variables as instruments. First stage regressions are displayed in Table 4. Regressions include the covariates listed in Table 2. Outcome variables defined in Table 3. Robust standard errors in parentheses, 37,952 observations.

Table 6: Local average treatment effects on various subpopulations of interest

	Woman	Men	Less 29	30-44	Above 45	Pers Layoff	Other Unemployed
Exit from PES registers to employment after six months (37,956 obs.)							
Enter public	9.1*	8.1	18.3**	-0.1	12.5*	7.0	10.7**
	(5.0)	(5.5)	(7.2)	(5.6)	(6.4)	(5.3)	(5.1)
Enter private	6.9**	-0.2	13.5***	-4.0	3.8	4.2	3.9
	(2.8)	(3.1)	(4.4)	(3.2)	(3.1)	(3.0)	(2.9)
Observations	19,363	18,589	11,053	16,447	10,452	15,921	22,031
p-value private vs. public (%)	57.6	5.6	41.0	38.3	10.4	50.3	10.0
control mean	21.4	24.9	29.3	23.6	15.1	19.1	25.7
Exit from PES registers to employment after 12 months (33,500 obs.)							
Enter public	12.1**	1.2	21.0***	-4.5	8.7	10.6*	4.4
	(5.1)	(5.4)	(7.0)	(5.7)	(6.5)	(5.5)	(5.0)
Enter private	10.1***	-1.0	18.5***	-3.6	3.2	9.2***	2.7
	(3.2)	(3.5)	(4.9)	(3.7)	(3.8)	(3.5)	(3.2)
Observations	17,313	16,187	9,336	14,513	9,651	14,145	19,355
p-value private vs. public (%)	60.8	60.4	65.8	83.2	29.5	74.3	66.7
control mean	33.8	39.8	43.8	38.5	25.2	31.3	40.1

Source: Job-seekers register (ANPE), Private contractor register and surveys of job-seekers with unknown exit. Notes: Weighted two-stage least squares regressions of "exit from PES registers to employment" on treatment variables. Each column considers a different sub-population and presents results at 6 and 12 months. Regression includes the covariates listed in Table 2. Outcome variables defined in Table 3. Robust standard errors in parentheses.

Table 7: Decomposition of private and public local average treatment effects, after six months

	Exit from PES registers to employment	Any employment	Employment eligible to payment
Private Program on Private Entrants (a)	3.9	4.1	4.4
	(2.1)	(2.2)	(2.0)
Private Program on Public Entrants (b)	3.0	3.0	3.6
	(1.7)	(1.8)	(1.6)
Public Program on Public Entrants (c)	9.1	6.7	7.8
	(3.7)	(3.9)	(3.5)
Private/Public Difference (a)-(c)	-5.3	-2.6	-3.4
	(3.0)	(3.1)	(2.8)
Selection Effect (a)-(b)	0.9	1.1	0.8
	(0.7)	(0.8)	(0.7)
Private/Public Difference on Public Entrants (b)-(c)	-6.2**	-3.6	-4.2
	(3.1)	(3.2)	(2.9)

Source: Job-seekers register (ANPE), Private contractor register and surveys of job-seekers with unknown exit. Notes: The second line reports estimates using outcomes weighted by the propensity ratio (see Appendix). The first and third lines reproduce results from Table 5 (with minor changes due to the use of one step GMM to get the joint distributions). These three estimates are then combined to decompose the difference between private and public programs into a selection effect and the difference between the two programs impacts on job-seekers who enter the public program. Regression includes the covariates listed in Table 2. Outcomes are defined in Table 3. Robust standard errors in parentheses, 37,952 observations.

Table 8: Heterogeneous effects with respect to employability, after six months

	Exit from PES registers to employment		Any employment		Employment eligible to payment	
	private	public	private	public	private	public
			Employability			
Enter program	3.7*	9.2**	3.9*	6.5*	4.3**	7.8**
	(2.1)	(3.7)	(2.2)	(3.9)	(2.0)	(3.5)
Program × employability	-4.9**	-2.7	-4.6**	-0.2	-3.6*	0.4
	(2.1)	(3.9)	(2.3)	(4.2)	(2.0)	(3.6)
Program × employability^2	-1.0	3.4	-3.0	-0.2	-0.1	3.7
	(2.2)	(3.3)	(2.3)	(3.4)	(2.2)	(3.2)
p-values (%):						
Homogeneity private	5.3	54.8	2.2	99.7	20.2	45.3
Homogeneity both	4.9		1.9		7.0	
Same effect	11.1		8.3		5.7	

Source: Job-seekers register (ANPE), Private contractor register and surveys of job-seekers with unknown exit. Notes: The table presents weighted two-stage least square regressions where program participation variables have been interacted with powers of an "employability score". The employability score is obtained as the predicted value of a logit regression of exit from PES registers to employment using the whole set of covariates listed in Table 2. The score has been normalized and its square has been centered. Regressions include the covariates listed in Table 2 as well as the employability score. Outcomes are defined in Table 3. Robust standard errors in parentheses, 37,952 observations.

Table 9: Local average treatment effects, after six months, by operator type

Area	Exit from PES registers to employment			Any employment			Employment eligible to payment		
	private	public	difference	private	public	difference	private	public	difference
Temporary help	2.1	12.1**	-10.0**	2.7	10.6*	-7.9*	0.8	6.8	-6.0
	(3.5)	(5.8)	(4.6)	(3.6)	(6.0)	(4.7)	(3.4)	(5.5)	(4.3)
International	3.9	9.9	-5.9	7.4*	12.1*	-4.7	8.1**	13.7**	-5.6
placement firms	(3.7)	(6.8)	(5.4)	(3.9)	(7.1)	(5.6)	(3.5)	(6.4)	(5.1)
Consultancies for placement	5.7	3.6	2.1	2.4	-4.5	6.9	4.8	2.6	2.2
after mass layoffs	(3.8)	(6.7)	(5.5)	(4.1)	(7.4)	(5.9)	(3.5)	(6.4)	(5.2)
p-value (%)	28.0	8.0	11.0	20.2	9.6	18.5	6.2	10.2	34.5

Source: Job-seekers register (ANPE), Private contractor register and surveys of job-seekers with unknown exit. Notes: The upper panel presents weighted two-stage least square regressions of employment outcomes on participation into program interacted with dummy variables for area in which the private contractor is of one of the three possible types: temporary help agency, international placement firms, and consultancies specialized in the placement of workers after mass layoffs. Instruments are assignment variables interacted with the dummy variables. p-values correspond to the test that the three coefficients are equal. For each of the three outcomes, the first column present the private program's estimated effects, the second the public program's effects and the last one the difference between the two. Regressions include covariates listed in Table 2. Outcomes are defined in Table 3. Robust standard errors in parentheses, 37,952 observations.

Table 10: Local average treatment effects, after six months on other types of exit

	Any exit from PES registers	Exit from PES registers to employment	Other exit from PES register	From which	
				Struck off	Other
After six months (37,952 observations)					
Enter public	4.9	9.1**	-4.2	0.7	-4.9*
	(4.1)	(3.7)	(3.2)	(1.8)	(2.8)
Enter private	-4.2*	3.8*	-8.0***	-2.9***	-5.1***
	(2.4)	(2.1)	(1.9)	(1.0)	(1.7)
p-value private vs. public (%)	0.5	7.8	12.9	1.2	92.2
Control group mean	37.3	23.0	14.2	2.7	11.6
After 12 months (33,500 observations)					
Enter public	3.2	6.9*	-3.7	0.8	-4.5
	(3.6)	(3.7)	(3.3)	(1.9)	(3.0)
Enter private	-0.9	5.2**	-6.1***	-2.7**	-3.4*
	(2.3)	(2.4)	(2.2)	(1.2)	(2.0)
p-value private vs. public (%)	13.6	56.7	35.5	1.8	64.6
Control group mean	58.3	36.6	21.7	3.9	17.8

Source: Job-seekers register (ANPE), Private contractor register and surveys of job-seekers with unknown exit.
Notes: weighted two-stage least square regressions on different types of exits from PES files after 6 and 12 months. Regressions include the covariates listed in Table 2. Robust standard errors in parentheses.

Table 11: Costs and Benefits

	Cost	Days on lists		Paid UB	Total expenses
		all	without job		
Intention-to-treat					
Assigned public	171***	-6.4**	-6.5**	-574**	-402
	(8)	(2.8)	(2.8)	(287)	(287)
Assigned private	524***	0.7	-0.5	-51	473**
	(7)	(1.9)	(1.9)	(198)	(198)
p-value private vs. public (%)	0.0	0.1	0.6	2.1	0.0
Local Average Treatment Effect					
Enter public	539***	-18.7**	-19.9**	-1,679**	-1,140
	(10)	(8.3)	(8.7)	(850)	(849)
Enter private	1,321***	1.3	-3.6	-159	1,162**
	(10)	(4.9)	(5.5)	(507)	(506)
p-value private vs. public (%)	0.0	0.2	1.5	1.9	0.0
Control group mean	120	257	223	22003	22123

Source: Job-seekers register (ANPE), Private contractor register and surveys of job-seekers with unknown exit. Notes: weighted Intention-to-treat and two-stage least squares. "Cost" is the program cost accounting for employment outcomes, "Days on lists" is the number of days the job-seekers was registered on the PES registers, "Days on lists without partial employment" accounts for jobs held while still registered, "Paid UB" (unemployment benefits) is based on the "Days on lists without partial employment" variable and uses as a measure of unemployment benefits 80% of the wage target, "Total Expenses" is the sum of program costs and Paid UB. Regressions include the covariates listed in Table 2. Robust standard errors in parentheses, 43,977 observations.

Appendix

We show in this appendix that under the conditional independence assumption

$$\Delta_{priv} \perp pub,\ priv\,|x, pub \cup priv,$$

the average effect of the private program on those entering the public program can be obtained as the impact of the private program on outcomes weighted by the ratio of propensities to enter public and private programs:

$$E(\Delta_{priv}|pub) = E(\Delta_{priv} R(x)|priv),$$

where $R(x)$ is defined as

$$R(x) = \frac{P(pub|x)}{P(priv|x)} \frac{P(priv)}{P(pub)}.$$

We have

$$E(\Delta_{priv}|pub) = E\left(\Delta_{priv} \frac{f(\Delta_{priv}|x, pub, pub \cup priv) f(x|pub, pub \cup priv)}{f(\Delta_{priv}|x, priv, pub \cup priv) f(x|priv, pub \cup priv)} \middle| priv \right)$$

Under the conditional independence assumption, we also have

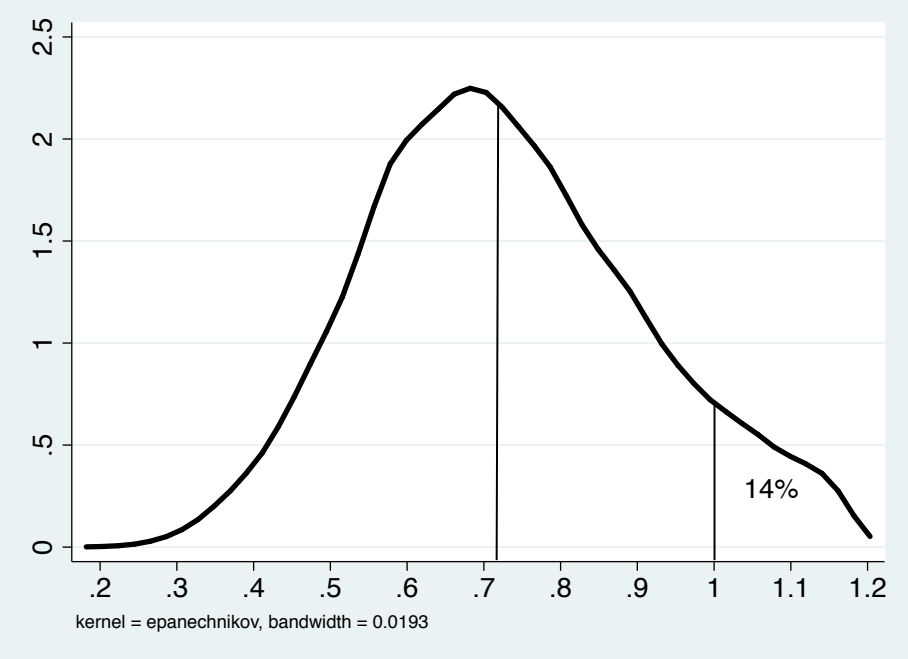
$$f(\Delta_{priv}|x, pub, pub \cup priv) = f(\Delta_{priv}|x, priv, pub \cup priv)$$

and we can rewrite

$$\frac{f(x|pub, pub \cup priv)}{f(x|priv, pub \cup priv)} \frac{f(x|pub)}{f(x|priv)} = \frac{P(pub|x)}{P(priv|x)} \frac{P(priv)}{P(pub)} = R(x)$$

To implement such a decomposition, we consider the whole set of covariates listed in Table 2. Table A-1 presents the results of logistic regressions of entry in each program. As can be seen, the main determinants are not the same in both cases: for instance, job-seekers presenting high statistical risk are more likely to enroll with private operators than with the public program; and first-time job-seekers and those seeking a full-time job are more likely to enroll with private operators than with the public program. The observable characteristics that we use lead to predicted propensities to enter programs that vary widely from one individual to another and from one program to another (see Figure A-1).

Figure A-1: Density of the relative propensities to enter programs



Source: Job-seekers register (ANPE), Private contractor register and surveys of job-seekers with unknown exit. Notes: ratio of propensities to enter public and private schemes and propensities. The vertical solid line represents the median. See Table A-1 for definition of propensities to enter programs.

Table A-1: Employability and propensity to enter private and public schemes

	(1) Employability		(2) Enter private		(3) Enter Public	
	coef.	se	coef.	se	coef.	se
College education	0.046**	(0.023)	0.003	(0.011)	0.016	(0.028)
Vocational	0.014	(0.022)	0.003	(0.011)	-0.019	(0.027)
High school dropout	-0.049*	(0.026)	-0.009	(0.013)	-0.028	(0.032)
Manager	-0.042	(0.042)	0.011	(0.023)	-0.018	(0.060)
Technician	-0.041	(0.040)	0.043**	(0.022)	0.033	(0.060)
Skilled clerical worker	-0.056	(0.039)	0.016	(0.019)	-0.021	(0.050)
Unskilled clerical worker	-0.038	(0.038)	-0.006	(0.020)	0.004	(0.053)
Skilled blue collar	-0.033	(0.039)	0.002	(0.022)	-0.036	(0.053)
Aged 26 to 35	-0.035	(0.022)	0.003	(0.013)	0.037	(0.031)
Aged 36 to 45	-0.037	(0.027)	0.039***	(0.015)	0.055	(0.038)
Aged 46 to 55	-0.095***	(0.026)	0.052***	(0.016)	0.066	(0.042)
Aged above 56	-0.186***	(0.018)	-0.094***	(0.019)	-0.078	(0.054)
Woman	-0.036**	(0.016)	0.019**	(0.008)	0.000	(0.020)
Married	0.000	(0.018)	0.004	(0.009)	0.002	(0.023)
One child	-0.018	(0.022)	0.021**	(0.011)	-0.011	(0.027)
More than one child	0.022	(0.023)	0.012	(0.010)	0.000	(0.028)

Continued on next page...

... table A-1 continued

	Employability		Enter private		Enter public	
French	-0.030	(0.034)	0.035**	(0.015)	0.069*	(0.040)
African	-0.052	(0.032)	0.018	(0.019)	0.080	(0.056)
Paris region	-0.014	(0.029)	0.030**	(0.014)	-0.039	(0.036)
North	0.001	(0.035)	0.190***	(0.017)	0.103**	(0.046)
Employment component level 1	0.123**	(0.050)	0.060**	(0.024)	0.163***	(0.063)
Employment component level 2	0.095**	(0.038)	0.075***	(0.023)	0.128**	(0.052)
Economic Layoff	-0.020	(0.028)	0.052***	(0.014)	-0.031	(0.035)
Personnal Layoff	-0.035	(0.022)	0.059***	(0.011)	0.006	(0.029)
End of Fixed Term Contract	0.057**	(0.026)	0.046***	(0.013)	-0.055*	(0.030)
End of Temporary Work	0.009	(0.037)	-0.035*	(0.021)	-0.134***	(0.039)
No exp in the job	-0.065***	(0.023)	-0.062***	(0.012)	-0.071**	(0.029)
1 to 5 years of exp in the job	-0.050***	(0.019)	-0.016*	(0.009)	-0.010	(0.024)
Statistical risk level 2	-0.056	(0.035)	0.050**	(0.021)	-0.109**	(0.048)
Statistical risk level 3	-0.119***	(0.036)	0.080***	(0.023)	-0.132***	(0.050)
Search for a full time position	0.045	(0.029)	0.152***	(0.012)	0.041	(0.034)
Sensitive suburban area	-0.065***	(0.021)	0.006	(0.011)	-0.040	(0.027)
Wage target 1350-1549 euros	0.020	(0.025)	0.031***	(0.012)	0.079***	(0.031)
Wage target 1550-1799 euros	0.018	(0.031)	0.026*	(0.015)	0.053	(0.040)
Wage target 1800-2200 euros	0.039	(0.028)	0.005	(0.012)	0.025	(0.031)
Wage target 2200 euros	0.030	(0.032)	0.014	(0.015)	-0.039	(0.037)
No Wage target	0.075**	(0.034)	-0.062***	(0.014)	-0.125***	(0.033)
First unemployment spell	-0.001	(0.015)	0.045***	(0.008)	0.014	(0.019)
Insertion firm region	0.044**	(0.021)	0.072***	(0.009)	0.053**	(0.025)
Temporary help region	0.036*	(0.020)	0.051***	(0.009)	0.048*	(0.024)
Assigned first quarter	0.029	(0.026)	-0.099***	(0.012)	-0.050**	(0.026)
Assigned second quarter	0.065***	(0.024)	-0.108***	(0.009)	-0.052**	(0.024)
Assigned third quarter	0.053**	(0.025)	-0.065***	(0.010)	-0.042*	(0.025)
Observations	4,155		30,728		3,069	

Source: Job-seekers register (ANPE), private contractor register and surveys on job-seekers with unknown exit. Notes: column (1) reports marginal effect of a weighted logit model of exit from PES registers to employment at 6 months using the sample of job seekers assigned to the standard track. Weights are based on the assignment scheme and the sampling scheme of job-seekers with unknown exit. Columns (2) and (3) report marginal effects of weighted logit models estimated on job-seekers assigned to the public and private programs, respectively. Weights are based on the assignment scheme. Robust standard errors in parentheses.