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

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Abstract

Contracting out public services to private firms has ambiguous effects when quality is imperfectly observable. Using a randomized experiment over a national sample in France, we compare the efficiency of the public employment service (PES) vs. private providers in delivering very similar intensive job-search counseling. The impact of each program is assessed with respect to the standard, low-intensity track offered by the public employment agency to the unemployed. We find that job-search assistance increases exit rates to employment by 15 to 35%. But the impact of the public program is about twice as large as that of the private program, at least during the first 6 months after random assignment. Combining a simple theoretical model with empirical findings, we argue that the contracts with private providers fail to solve the underlying agency problem. The failure is not due to cream-skimming; rather, it seems that profit-maximizing private providers find it optimal to enroll as many job-seekers as they can, but to make minimum effort on the placement of some of them (parking).

1 Introduction

Job-search counseling policies have received increasing attention both from policy makers and researchers in many countries over the last decades. Evaluations show that they are generally efficient, especially compared with more traditional active labor market policies, such as training and subsidized employment (Card et al., 2010). Until the late 1990s, OECD countries typically involved a broad range of public and semi-public education institutions as well as private firms in the training of job-seekers, but public employment agencies kept a monopoly on individual assistance and the matching of job-seekers with vacancies. The recent and rapid expansion of job-search assistance to a large number of unemployed persons has increasingly relied on publicly-funded private providers (Finn, 2011). Australia (in 1998) and the Netherlands (in 2001) have allowed private providers to 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 in welfare-to-work programs.[1] These early movers have been emulated by others, with strong support from international organizations such as the OECD and the European Union (EU). In particular, in 1998, the European Commission urged EU member states to open up publicly-funded employment services to private providers in order to increase efficiency.[2] Whether these changes have actually improved the well-being of job-seekers and reduced the costs of services is the topic of highly polemical public debates.[3]

Outsourcing public services to private firms raises classical economic issues when either actions

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[1] In 1996, the Personal Responsibility and Work Opportunity Reconciliation Act gave more autonomy to individual states for implementing a new welfare program: "Temporary Assistance for Needy Families" (TANF). Several states and localities (including Wisconsin and New York City) contracted out all services, including eligibility for TANF financial assistance and case management, to private providers. See McConnell et al., 2003.

[2] "It is essential to redefine the role and tasks of the PESs in relation to the other providers of commercial or non-commercial services: for example, possibility of the PESs withdrawing from activities which can be carried out more effectively by other employment services, possibility of outsourcing certain tasks" (EC, 1998).

[3] In France, for instance, the government's decision to outsource the case management of 320,000 job-seekers to private providers in 2010 and 2011 made the front page of a national newspaper, under the headline "Making money on the backs of job-seekers" (L'Humanité, 2009), while a member of parliament challenged the government, stating that: "The fierce competition of these firms [private providers of job-search assistance] against one another bears almost exclusively on costs, to the greatest prejudice of the service to persons made vulnerable by the loss of a job" (Assemblée nationale, 2009).

or quality are not perfectly observable. In the job-search counseling environment, one might imagine that the actions to be implemented are observable and can be monitored. However, the unemployed are strongly heterogeneous and the actual effort targeted on each individual is impossible to control. The fact that the effort and ability of the unemployed person (her capacity to find a job on her own) are largely unobservable limits what can be inferred from observed outcomes. As a result, cream-skimming (by which the providers only enroll job-seekers with the best labor market prospects) and parking (consisting in a bare minimum of services for harder-to-place clients) can be optimal strategies if the incentive structure created by the government contract does not rule them out. Agency problems may thus counterbalance the perception that market discipline induces private firms to be more efficient than PESs.

This paper provides novel empirical evidence on this issue. It analyzes one episode in the opening-up to private contractors of the job-seeker placement market in France, in 2007-2008. This episode allows a direct comparison of the placement impacts of two highly comparable job-search programs, one provided in-house by the public employment agency (ANPE), the other one outsourced to private providers by the unemployment benefit fund (Unédic).[4] The context is as follows: at the end of 2006, Unédic contracted out to private companies to provide job-search assistance to unemployed job-seekers eligible for unemployment benefits. The stated goal was to make savings: Unédic expected the unemployment benefits saved thanks to the job-seekers' faster return to work to outweigh the cost of job-search assistance. An additional goal was to create competitors to the public employment agency, just after a law passed in 2005 had abolished the ANPE's monopoly on the placement of job-seekers. The ANPE responded by developing a "twin" job-search program, provided in-house by dedicated personal advisors (caseworkers). The two programs provide much more intensive assistance than the standard track followed by job-seekers: in each program, the caseload ratio is limited to 40 job-seekers

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[4]In the French system, the two organizations were separate at that time, the former being a State agency and the latter being managed by employers' organizations and trade unions.

per personal advisor, compared to about 120 in the standard track. Although each program is planned to serve about 40,000 job-seekers over one year, they are considered experimental programs and are jointly evaluated, using a randomized controlled trial. We present the results of this evaluation, conducted in about 400 local public employment offices and involving more than 200,000 job-seekers.

There are two basic results. First, intensive counseling does increase transitions to employment in the two programs. After 6 months, for instance, exit rates to employment increase by 4 to 9 percentage points for program recipients, a 15 to 35% increase on the counterfactual exit rates (around 25% under the standard track). Second, impacts are consistently higher for the public program as compared with the private one.

We also find evidence that private contractors respond closely to the detailed incentives of their contract with Unédic. In the second part of the paper, we therefore investigate whether the incentives given to private contractors may explain their lower performance. Outsourcing job-search assistance frequently involves a two-tier payment to the contractors: an (unconditional) upfront payment when the job-seeker signs in, and a (conditional) payment if the job-seeker is placed on time in an eligible job. This contract structure aims to find a balance between the risk of cream-skimming and the risk of parking. We propose a simple agency model to take this trade-off into account. We find evidence that the upfront payment in our setting is large enough to induce private contractors to maximize program enrollment rather than to cream-skim, but that they probably also have an incentive to park those job-seekers on whom job-search assistance has limited impact. This analysis implies that the lower performance of private contractors could result from a difficulty in setting the right incentives.

There is a growing literature evaluating job-search programs[5] but, to our knowledge, very

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[5]See the meta-analysis in Card et al. (2010). Dolton and O'Neill (1996, 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 et al. (2005) report results for the US. By contrast, analyzing a Dutch randomized experiment, van den Berg and van der Klauw (2006) find no evidence that counseling and monitoring affected the exit rate to work. More recently, Rosholm (2008), Bernhard and Wolf

few of these papers allow direct comparison of the effect of public vs. private provision of the service. One paper close to ours is Bennemarker at al. (2009), who evaluate the effectiveness of contracting-out employment services to private placement agencies using a randomized trial. They tend to find some positive employment effects,[6] but it is unclear whether these effects should be attributed to the more intensive counseling per se (independently from its provider), or to the fact that the service was contracted out to private companies. In our experiment, we compare similar counseling practices, with similar intensities, so that the different outcomes are more closely dependent on ownership structure. Jonsson and Thoursie (2012) also use a randomized trial to compare public and private provision of vocational rehabilitation of Swedish individuals on long-term sick leave, where the two programs are similar and of comparable cost. They find no differences between the two treatments. Another related paper is Krug and Stephan (2011): job-seekers are randomly assigned to a mandatory counseling program provided either by private contractors or by the public employment offices in Germany. Consistent with our paper, the authors find that the in-house counseling program performs significantly better than the private one. However, those papers do not analyse the reasons for the lack of value-added from outsourcing.

Our paper also relates to the vast theoretical literature (Grossman and Hart, 1986, Shleifer, 1998, Hart et al., 1997, Besley and Ghatak, 2001) that examines the implications of firm ownership on the provision of public goods or public services. Private entrepreneurs may have stronger incentives to invest in cost-saving and quality-enhancing technologies. However, when quality is imperfectly observable, their incentives to engage in pure cost reduction may be too strong. 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

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(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

[6]The point estimates are positive, but quite imprecise due to the low take-up rate and the limited sample size, so that only one estimate is significant at 10%.

effort and quality is easily observed, thus where contracting difficulty is limited (such as garbage collection). In contrast, the conclusion is mixed or reversed for activities where contracting is difficult (they mention prisons or residential youth care).[7]

We consider here a version of this general question that applies to agency issues arising in the employment policy context. A few papers have investigated the impact of incentives in this context. Heckman, Heinrich and Smith (2002) analyze the impact of performance standards used to monitor (public) job-training centers in the context of the Job Training Partnership Act (JTPA). They do not find much room for cream-skimming; correspondingly, Heckman and Smith (2004) find that factors beyond the control of providers (personal choices, awareness of program eligibility) explain most of the selection into programs. Courty and Marschke (2004), also studying the JTPA, find evidence that job-training centers 'game' the incentive system by manipulating the dates of trainees' graduation to smooth performance over good and bad years. Koning and Heinrich (2010) analyze the impact of changes in the contract made with public providers in welfare-to-work programs in the Netherlands. Their results suggest that cream-skimming and parking are on average limited, even though they are more significant for the subgroup of disability insurance recipients. Overall, our results corroborate the idea that the risk of cream-skimming may have been overstated, leading to contracts with suboptimal incentives. The lower performance of private providers may well have been partly written in advance in their contracts. Of course, just as Heckman et al. (2002) conclude, "Nothing in this paper says that a successful [outsourcing contract] cannot be devised". The results however, call for careful implementation, involving perhaps some more experimentation.

The next section outlines the labor market context in France in 2007 and describes the public and private programs. Section 3 presents the experiment. Section 4 gives the main findings, directly derived from the randomized setting, while section 5 explores the role of contracts

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[7]They also argue that plausibly exogenous variation in outsourcing is the exception in the literature, so that available empirical evidence is subject to uncertainty.

in explaining the private program's lower performance. The subsequent section discusses the results, and the last section concludes.

2 The programs

Since the 1980s, the French labor market has been characterized by high unemployment rates and persistent long-term unemployment. At the beginning of the two programs, unemployment was decreasing but 8.4% of the labor force was still unemployed; of these, about 30% had been unemployed for a year or more.

Before 2005, the French public employment agency (ANPE) had a monopoly on the placement of unemployed job-seekers. The Social Cohesion Act (*loi de cohésion sociale*, January 2005) opened up this market to private companies. In 2005-2006, the French unemployment benefit provider (Unédic) started experimenting with supplying intensive counseling to unemployment benefit claimants who had a high statistical risk of long-term unemployment. Private firms provided the counseling. 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. In what follows, we call this program the "private program". Its main characteristics are as follows: each job-seeker receives intensive counseling during up to six months. Private companies are selected through a bidding process, conducted separately in 16 regions. The 11 private providers eventually selected belong to three groups: temporary agencies, consultancies specialized in the placement of workers after mass layoffs, and international firms (from Australia and the Netherlands) specialized in the placement of job-seekers. The payment structure is common to all regions: 30% of the maximum payment is paid upfront, when the job-seeker is enrolled in the program; the remaining 70% is conditional on placement: 35% is paid if the job-seeker finds a job within six months, and the remaining 35% is paid if the worker is still employed after 6 months. The maximum payment per worker results from the bidding process: it varies from one region to

another and ranges between 3,000 and 3,947 euros (Vivès, 2009). In other words, the minimum payment per worker is 900 euros (the upfront payment when the job-seeker is not placed within six months), and the maximum payment is 3,947 euros (when the worker is durably placed in a job, in the regions with the highest bidding price). There are restrictions on the employment contracts that trigger the conditional part of the payment: the contract must be open-ended or, if it is fixed-term, for at least six months. Moreover, the working time must exceed 110 hours per month. Unédic's involvement in the placement of job-seekers has explicit financial motives: the goal is to save on benefits by quickly placing job-seekers at risk of long-term unemployment.

Simultaneously but separately, the French employment public agency (ANPE) launched its own in-house program of intensive counseling. In addition to the population targeted by Unédic, this program also targeted job-seekers who were not eligible for unemployment benefits. Each job-seeker was also to receive intensive counseling during up to six months and the target was to start the program with 40,000 job-seekers in 2007. In what follows, the ANPE in-house program is called the "public program".

From a political economy perspective, it is noteworthy that the public program was triggered by the new competition from the private sector. In 2006, Unédic claimed that the local experiments conducted with private companies in 2005-2006 had been highly successful. The only evidence provided for this claim was that placement rates were high. However, no proper evaluation was conducted and the take-up in these early programs was low: only 30% of the job-seekers sent to the private companies actually entered the counseling program. Selection bias was an obvious problem. These dubious results were nonetheless used by Unédic as an argument in the latent conflict with the ANPE, which was accused of being inefficient. This motivated the requirement to run a randomized controlled trial to get transparent results – a *première* in the evaluation of labor market policies in France.

The exact content of the public and private programs varies locally. However, the basic

structure is the same everywhere and across the two programs. The key feature is to assign a dedicated "personal advisor" with a much lower caseload to the job-seeker; in short, more human resources and a more integrated follow-up. This translates into at least one weekly contact (by e-mail or telephone) and one monthly face-to-face meeting between the job-seeker and the caseworker. In the private program, the terms of reference imply that a caseworker may not assist more than 40 job-seekers; this is also the average caseload ratio in the public program. Compared to the usual track, where a contact is supposed to take place every month and where ANPE agents assist on average 120 job-seekers, this is a significant increase in the human resources dedicated to assisting the job-seeker. In addition to using similar inputs, the two programs share a common approach and similar tools aimed at empowering the job-seeker.[8] Job-seekers are enrolled by signing a charter, and their 6-month trajectory within the program is organized around an individual action plan, the objectives of which are periodically reviewed.[9]

One way to quantify the treatment received by beneficiaries of the two programs (apart from the dramatic decrease in caseload ratios) is a survey run by the Ministry of Labor on a subsample of unemployed from the inflow that were subject to the random assignment 4 to 6 months after the start of the experiment. They were interviewed by telephone in March 2008, i.e., 9 to 11 months after their assignment (Gratadour and Le Barbanchon, 2009). Table 1 analyzes this data on areas where the programs were developed jointly for the same population of job-seekers.[10] First, the frequency of face-to-face interactions with caseworkers increases significantly in the two programs, compared to the standard track. Interestingly, however, the private program increases the differences between its job-seekers, whereas the public program reduces them. The public program increases the proportion of job-seekers receiving an intermediary level of service (1 to 3

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[8]Capelier and Mizrahi (2008) conduct a qualitative comparison of the two programs, finding very few differences in methods and principles. 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 the two programs.

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

[10]As detailed below, since only part of the job-seekers actually participated in the program they were assigned to, we compute 2SLS estimates instrumenting participation with assignment, see equation 1.

meetings per month) from 40% to 80% and reduces the proportion receiving very little service. In contrast, the private program increases the proportion of those meeting their caseworker very frequently (once a week or more) and only moderately reduces the proportion of those meeting him very rarely (less than once a month). Second, neither of the two programs significantly increases the number of job offers presented to the job-seeker. If anything, the private program tends to reduce it (though not significantly). Last, the survey yields information on the type of training sessions received by the program recipients. Sessions aimed at quickly finding a job (firm targeting, job interview rehearsals, search on the Internet) are more frequent in both programs, compared to the standard track. Longer term investments (skills assessment, longer vocational training programs) are less frequent, while lighter types of assistance (help with writing résumés or cover letters) do not change significantly, except for more frequent help with résumés in the private program. Overall, the two programs appear to be similar and quite standard activation programs. However, the private program differs from the public one in having a more contrasted allocation of caseworker time to different job-seekers. We discuss these facts in more depth in section 6 below.

3 Experimental design

The public and the private programs were evaluated jointly in a randomized controlled trial.[11] The populations concerned by the two programs partially overlap. Unlike Unédic, which restricted the program to job-seekers entering unemployment and entitled to at least one year of benefits, the ANPE decided to also offer intensive counseling to two other groups: the newly unemployed who were entitled to benefits for less than one year, and the long-term unemployed. Moreover, the two programs were not developed in exactly the same regions: some regions only

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[11]The evaluation was conducted by Behaghel, Crépon, Guitard, and Gurgand. The final report is available in French (Behaghel, Crépon and Gurgand, 2009). This paper considerably extends the results and the analysis of the agency problem when contracting out to private providers.

had one program or the other, while some had both. Overall, this creates 5 sub-populations, based on unemployment duration, benefit eligibility and location. Only the newly unemployed with sufficient benefit entitlement in areas providing the two programs could be assigned to the two competing treatments (figure 1). They thus play a key role in the comparison between the private and the public programs.

By policy choice, participation in the two programs is voluntary. Correspondingly, the experimental design individually randomized whether one or the other of the two programs was offered to a given job-seeker. More specifically, within each of the 5 sub-populations defined above, randomization was used to create two or three experimental groups: the control group, assigned to the usual track (without intensive counseling), and one or two treatment groups (assigned to the public program or assigned to the private program). The randomization took place during the first interview at the local ANPE office (that is to say, when first registering as unemployed). Once the caseworker had assessed the job-seeker's eligibility, he ran an application on an Extranet to randomly assign him to treatment 1 (public program), treatment 2 (private program) or to the control group (usual track). The probabilities of assignment to each group varied locally and across the different sub-populations 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 treatment 2 (the private program, up to 85%) and much lower probabilities of assignment to treatment 1 (the public program, down to 6%) and to control (down to 9%).

After this randomization, the employment service agent told the job-seeker which track he was being offered. The job-seeker was free to refuse the more intensive tracks. Depending on the assignment and on his decision during the first interview, he was subsequently contacted by ANPE staff for the usual track, by a dedicated caseworker from the ANPE-provided intensive

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program, or by one of the private firms paid by Unédic. Job-seekers of the two treatment groups effectively entered the program they were assigned to 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. Thus, a significant share of each treatment group (about 60% on average) did not actually enter the program they were assigned to. Following the usual terminology, they are non-compliers. The high rates of non-compliance were expected and had been taken into account in the statistical power calculations; along with the unbalanced assignment probabilities, they are a factor limiting the precision of the evaluation. This appears to be the price to pay for this large-scale, realistic setting; it is fortunately counteracted by the large samples.

The random assignment took place over 15 months, from January 2007 to March 2008, in 393 local public employment offices in 16 of the 22 French administrative regions. 67 offices only assigned job-seekers to the public program or the usual track, 91 assigned them only to the private program or the usual track, and the remaining 235 had all three possibilities. Overall, 219,033 job-seekers enter the evaluation sample.

The data is described in the appendix in more details. Administrative records held by the ANPE provide basic sociodemographic information on job-seekers and allow to compute the duration of all registered unemployment spells in the experimental sample. However, the reason for exiting the unemployment records is unknown in many cases. As part of the experimental design, an independent survey company was therefore commissioned to conduct a very short phone survey on a subsample of workers whose destination on leaving unemployment was not identified in the administrative records. The combination of the administrative information and that survey of about 40,000 job-seekers yields high and balanced response rates.

The first three columns of Table 2 provide evidence that the randomization led to comparable experimental groups. They display sample means for a variety of covariates in control (1st col-

umn) and treatment groups (2nd column). Differences are small, and rarely significant (column 3), as expected when the different groups are drawn randomly from the same population.[12]

3.1 Program participation

As noted above, program participation is voluntary, so that job-seekers assigned to one of the more intensive programs are more likely to enter that program, but are far from systematically doing so. As a consequence, program recipients are a selected sample from the initial population. Technically, this "encouragement design" still allows to estimate the causal impact of the programs on the "compliers".[13] However, it is important to characterize these compliers in order to interpret the effects better.

On average, program participation is around 40%.[14] Table 2 clearly shows that while assignment to the different programs is random, actual participation is not. Table 3 analyzes program participation in a linear probability model, with participation in the programs as the dependent variable. Columns 1-4 show that less educated tend to participate less, while middle-aged job-seekers, foreigners and those who lost their job in a mass layoff tend to participate more. An interesting question is whether selection into intensive counseling differs between the private and the public program. To address this question, the sample in columns 5 and 6 is restricted to the inflow with unemployment benefits, in areas where both programs were implemented: job-seekers assigned to the two programs are thus ex ante statistically identical. Column 5 shows a large,

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[12]The next two columns are analyzed below.

[13]The statistical appendix shows that this interpretation holds even in our case with two treatment groups (and one control group).

[14]The reasons for that are diverse, and we get back to them below. Some job-seekers are eventually found not to be eligible (in particular, eligibility for unemployment benefits is sometimes poorly assessed during the first meeting at which randomization takes place). Others may not want to participate, either for their own reasons or because they are discouraged by caseworkers in the programs. Last, administrative failures resulted in some job-seekers not being told which track they were offered, and private companies or ANPE teams in charge of the public program were not always given the appropriate lists of job-seekers they were meant to contact. Overall, non-compliance results from a mix of self-selection, selection by the caseworkers (and possibly creaming), and random misallocation. Although our feeling from monitoring the experiment is that all these occurred, we cannot directly document it. Lower take-up rates for job-seekers who were not eligible for benefits and for long-term job-seekers are consistent with the perception that these job-seekers are estranged from the employment services (higher proportion of discouraged job-seekers, and job-seekers who do not depend on the public employment services for unemployment benefits).

statistically significant difference in participation, from 32.1% in the public program to 43.8% in the private one. Column 6 checks whether the increase in participation is the same across different subgroups (bottom part of the table, interacting assignment to the private program with the different covariates). Two interaction terms are statistically significant, meaning that participation in the private program was particularly high for older job-seekers (an additional 5.3 percentage point increase in the participation rate) and for workers who had lost their job in a mass layoff (an additional 4.6 percentage point increase). There is thus evidence that the private program attracted or retained a larger fraction of the assigned job-seekers, and some evidence that the populations of recipients ended up being somewhat different, with compliers in the private program having less favorable characteristics in terms of job finding. Section 5 analyzes the corresponding selection issues further.

4   Impact of private and public counseling programs

In this section, we present instrumental variable estimates of the impact of the two programs (public and private), on various populations. Specifically, we estimate

$$Y = a + c_1(T = 1) + c_2(T = 2) + u \tag{1}$$

by 2SLS using random assignment to the public and private programs (respectively, $(Z = 1)$ and $(Z = 2)$, where $(Z = z)$ denotes the indicator variable for $Z = z$) as instruments for treatment by these programs (denoted by $(T = 1)$ and $(T = 2)$).[15]

Equation 1 is estimated separately for recently registered unemployed people ("inflow") and for individuals who had been unemployed at least 3 months at the time of randomization ("stock"). In the inflow, we also distinguish between unemployed individuals with and without entitlement

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[15]Given voluntary participation in the program and the resulting low take-up rates, we focus on LATEs rather than on intention-to-treat (ITT) effects. ITT effects are harder to interpret: we postpone their discussion to section 5 on the basis of a simple theoretical model.

to unemployment benefit (UB). To account for the fact that assignment probabilities varied across periods and areas, observations are weighted by the inverse of the assignment probability. Therefore, estimates can be interpreted as LATE on the compliers of the initial population.[16]

4.1   Impact on exit to employment

In this subsection, we consider the impact of the two programs on exit from unemployment to employment. The outcome in Table 4 is whether the person has left unemployment registers to take a job within 3 months (resp. 6, 9 and 12 months) of randomization. The first implication of this table is that intensive counseling does increase transitions to employment: concentrating resources on unemployed individuals to help them search is effective. This is in line with most empirical research. Orders of magnitude are fairly high. For instance, the baseline exit rate in the control group is 27% after 6 months in the inflow with UB, compared with an impact of 4.2 points in the private program and 9.1 points in the public program. Accordingly, the exit rate is 30% in the inflow with no UB, compared with an impact of 7.6 points; in the stock, the baseline exit rate is only 22% and the impact is 3.6 points.[17] The impacts tend to increase somewhat after the first months of intervention, especially in the private program. Conversely, although the horizon is not sufficient for a strong statement, it seems that the impact tends to stabilize by 12 months.

The second main fact is that impacts are stronger for the public program than for the private one. This cannot be judged precisely based on Table 4, because all areas are pooled. As the regions in which the two programs are applied only partially overlap, the different impacts may be driven by differences in local characteristics of the labor markets or of the job-seekers. In

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[16]In the presence of heterogeneous treatment effects, this is arguably better than using area × period fixed effects. One can show that the fixed effect estimator artificially gives more weight to areas/periods where the probabilities of assignment to the control and the treatment groups are close to each other.

[17]Note that counterfactual exit rates, i.e. exit rates that program recipients would have in the absence of treatment, are not exactly equal to exit rates in the control group. Indeed, we estimate local average treatment effects on compliers; compliers differ from one program to another and may have different counterfactual exit rates. The consequences are discussed in section 5 and table 8. However, this does not modify the general picture, so that we simply use exit rates in the control group as a reference throughout this section.

order to make the comparison meaningful, Table 5 presents the impact on inflow with UB, by areas. In the first two columns, the sample is restricted to areas where both programs were offered: whether an individual from such an area was assigned to one program or the other is random. The left column is the impact under the private program and the right column the impact under the public program. The last two columns are program impacts in places were only one program was in place.

In areas where the two programs were in place, we can see very strong differences: the public program's impact is 11 points after 3 months, whereas the private program's impact is negligible and non-significant at this early horizon. After 6 months, the gap between the two programs narrows, but the public program's impact remains twice as large until after 9 months. The difference only gets smaller after one year. It is extremely clear that the impact of the public program is immediate and the differences in survival rates are maintained afterwards. In contrast, the private program is inefficient at the beginning of the counseling period: its impact comes later; it then remains stable over time and remains somewhat smaller than the public program. Another striking feature of Table 5 is that impacts are weaker in areas where each program stands alone. One possible interpretation is that the two programs are more effective in areas where they are competing (a stimulation effect). Notice, however, that effects may also vary by areas for many other reasons: differences in the local labor demand, in the pool of job-seekers, etc.[18]

Table 6 shows that the efficiency of counseling is not just the result of an artificial effort to send people to very short-term jobs. We do not measure directly the duration of jobs, but we know how long it takes for people to register as unemployed again. In Table 6, we therefore distinguish two kinds of exits from unemployment towards a job: those that last less than 6 months until a new unemployment spell starts, and those that last more than 6 months. We

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[18]The Appendix not for publication shows that the results are unchanged if we add observed variables as controls in the regression or if we include fixed effects for areas with the same probabilities of assignment to treatment, rather than weight observations as in our baseline tables.

show the impact of treatment on the probability of occurrence of each of these exits, 3 months and 6 months after assignment.[19] The first four columns of Table 6 compare the two programs in similar areas (as in Table 5).[20] They show that the public program is more efficient in two ways. First, it sends people into jobs associated with short-term exits (although this is imprecisely estimated on subsamples), which the private program does not do at all. Second, it is more efficient than the private program at sending out people for long periods of time.[21]

4.2 Alternative outcomes

As detailed in section 2, the private program sets explicit criteria of success for private contractors (placing the job-seeker in an eligible job within 6 months, for at least 6 months, where eligible jobs must have a working time of at least 110 hours per month) that may differ from the (less explicit) goals of the public program. It is therefore useful to compare the impact of the two programs over different criteria. Table 7 compares the two programs on different outcomes in areas where both programs are in operation, so that they are not driven by differences in labor market conditions or in initial populations. As shown already, the public program is more effective in terms of exit to employment. The coefficient for the private program is quite stable across outcomes. This implies that the effect is concentrated on the narrow target of "eligible jobs", with no additional impact on other types of jobs that do not trigger payment to the private contractors. In contrast, the public program improves placement not only in terms of long-term jobs, but also for other jobs that are not eligible for payment in the private program. Still, even for the outcome for which private providers focus their effort, the public provider remains 50% more efficient (although the difference is no longer statistically significant).

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[19]Given our one year window of observation, we could not observe these types of exits at longer horizons.

[20]Appendix Table 11 finds similar results when all types of populations are presented, all areas taken together.

[21]Appendix Table 12 explores treatment effect heterogeneity, by distinguishing between men and women, younger and older job-seekers, and different educational levels. We focus on exit to employment within 6 months after randomization. While there is considerable heterogeneity in terms of job-finding rates in the absence of the program, we find little evidence of program impact heterogeneity. In that sense, there is no evidence of an efficiency/equity trade-off. This is in line with Heckman et al. (2002), who report little evidence of (training) program effect heterogeneity, compared to the ex ante heterogeneity of trainees' labor market prospects.

Overall, the main basic fact is that the two programs are effective in improving the labor market outcomes of job-seekers, but private firms prove to be less efficient than the public service. A second finding is that private providers seem to respond closely to the detailed incentives of their contract. We now turn to the one hypothesis that builds upon these two findings: did the private providers perform less well than the public program because it was in their best interest, given the incentives they were offered?

5 Why are private providers lagging behind?

In this section, we address a prominent issue in the debate on outsourcing public services like job-search assistance: do the financial incentives offered to private providers lead to better outcomes? We argue that the inability of the principal (the unemployment insurance fund) to provide private firms with the incentive to act optimally is a plausible explanation for their surprisingly low performance, and we provide a set of stylized facts in support of this interpretation. We also discuss alternative explanations.

The argument can be summarized as follows. We assume that the two programs have access to the same technology and to identical pools of applicants (we discuss these assumptions further in section 5.4). In this context, the lower performance of the private program must be due to the fact that private providers apply the counseling technology less intensively and/or apply it to job-seekers on whom it is less effective. This may occur for at least two reasons. In the first case, private providers might be induced by their contract – specifically, the premium P_1 that they receive each time a job-seeker finds a job – to selectively enroll job-seekers with better job market prospects and discourage those with lower *own* job-finding rates, even though the latter may be the job-seekers for whom counseling is most effective. In the second case, private providers can be induced by the second feature of their contract – the large unconditional upfront payment P_0 that they receive each time a job-seeker signs in – to focus their effort on persuading as many

job-seekers as possible to enroll. Both cases – discouraging job-seekers for whom the program is effective or attracting job-seekers for whom it is ineffective – lead to lower program impact, by distorting the pool of enrollees towards those with low treatment effects. We develop a simple agency model in which the contract set by the principal (the UB fund) can lead providers to behave according to these two cases, depending on the size of the unconditional and conditional payments, (P_0, P_1). We show evidence that private providers actually behaved according to the second case, maximizing enrollment into the program rather than cream-skimming, and probably providing a bare minimum of service to some job-seekers ("parking"). The key reason for this is the very high upfront payment P_0 that largely covers the marginal cost of enrolling *any* job-seeker. In contrast, the public program, as it is managed in-house in the PES, has to internalize the scarcity of caseworkers, and has no incentive to maximize enrollment. This may explain why enrollees in the public program have on average stronger treatment effects than those in the private program.

To some extent, the evidence we bring is of course specific to the context of the job-search experiment that took place in France in 2007-2008. However, the basic arguments and the theoretical mechanisms at play are more general, especially given that the payment structure – a fixed upfront payment, plus a conditional payment if placement occurs within a given period – has become common in European countries that outsource the placement of job-seekers.[22] Moreover, upfront payments as large as one third of the maximum total payment are not exceptional. Our simple model and the perverse effects of a large upfront payment may therefore shed light on a general problem in the way job-search assistance has frequently been outsourced.

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[22]Examples include Germany, the Netherlands, Sweden, etc. See Winterhager (2006), Struyven and Steurs (2002), and Bennemarker et al. (2009).

5.1 An agency model with heterogeneous job-seekers

We start by formalizing the agency problem faced when outsourcing job-search assistance in a simple model.[23]

Consider a static (one period) agency model. The goal of the principal – the unemployment benefit provider, denoted by U – is to minimize its benefit spending B on a given pool of heterogeneous job-seekers entering unemployment. B is paid unless the job-seeker finds a job by herself, which occurs with probability λ.

A counseling technology is available that allows to increase job-finding rates from λ to $\lambda + \delta$, at cost c per job-seeker. It would therefore be privately optimal for U to provide counseling for all job-seekers such that $\delta B \geq c$. However, U decides to outsource the provision of counseling to private contractors, under a two-tier payment structure: (P_0, P_1). Job-seekers are heterogeneous in (λ, δ), i.e. in their propensity to find a job on their own (λ) and in the impact counseling has on them (δ).[24] The type of a job-seeker is not observed by the principal (U); it is however observed by the agent (the private provider) at the first meeting with the job-seeker. The agent thus has the possibility to screen job-seekers; he can also decide to accept the job-seeker but provide her with minimal service: in that case, the cost of enrolling the job-seeker is purely administrative (and denoted by c_a with $c_a < c$), but enrollment is ineffective: we assume that the job-finding rate then remains λ.

From the principal's point of view, the first-best solution is to enroll and treat all job-seekers

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[23]The problem of striking a balance between "cream-skimming" and "parking" when outsourcing the placement of job-seekers is clearly stated in Struyven (2004), but we are not aware of any formal analysis. Heckman et al. (2002) provide a very clear discussion of cream-skimming in a similar setting, but with a different incentive scheme (a performance standard) applied to public training centers. The problem is also reminiscent of standard problems in health economics on optimal reimbursement schemes to health-care providers – see in particular McClellan (2011). However, the main comparison in this context is between prospective payments (fixed, possibly risk-adjusted payment per patient) and fee-for-service reimbursement. For the placement of job-seekers, fee-for-service payments are rarely considered, but payments are indexed on an individual performance measure (finding a job or not). Performance measures are less obvious in the health care context and still play a limited role (McClellan, 2011). As a result, models from health economics do not directly apply in our setting. However, the variety of payment schemes considered in the health economics literature underscores the fact that the structure of payment chosen by Unédic is only one out of many possibilities. Our discussion holds only within that particular type of contract.

[24]For the sake of simplicity, c and B are assumed to be identical across job-seekers.

with $\delta B \geq c$ and not to enroll job-seekers with $\delta B < c$. However, cream-skimming occurs when enrollment is an increasing function of λ for some δ: people with little own labor market prospects are excluded from the treatment, even though their value of δ would justify treatment. Parking occurs when the agent enrolls but does not actually treat some job-seekers: given λ, this is likely to occur for job-seekers with small δ. Notice that there is a double problem with parking: some people with low gains from the program ($\delta < c/B$) can be enrolled and parked although they should be neither enrolled nor treated, from the principal's point of view; and some job-seekers with intermediate gains from the program can be enrolled and parked although they should be enrolled and treated, from the principal's point of view.

Consider the private provider's incentives. Its efforts can be of two types. The first type of effort is to register the job-seeker into the program and park him at cost c_a (administrative cost). The gain in this case is simply $\Pi_0 = P_0 + \lambda P_1 - c_a$. The second type of effort is to register the job-seeker into the program and provide her with the treatment at cost $c > c_a$. The gain in that case is $\Pi_1 = P_0 + (\lambda + \delta)P_1 - c$. The private provider's decision is quite simple. If $\Pi_0 \leq 0$ and $\Pi_1 \leq 0$, the job-seeker is not enrolled. If $\Pi_0 > 0$ but $\Pi_1 \leq \Pi_0$, the job-seeker is enrolled and parked. If $\Pi_1 > 0$ and $\Pi_1 > \Pi_0$, the job-seeker is enrolled and treated. These conditions define the types (λ, δ) of job-seekers that are either not enrolled, parked or treated.

Figures 2 and 3, drawn in the (λ, δ) space, make clear how choices P_0 and P_1 shape enrollment and treatment. Two cases are considered: one with P_0 larger and one with P_0 lower than administrative costs. In figure 2, $P_0 > c_a$ thus Π_0 is always positive. Then everybody is enrolled because it is always profitable for the firm to have individuals on its list. However, they do not all receive treatment and some of them are parked (area PA1). Only job-seekers with $\delta > (c - c_a)/P_1$ are treated (area TR1). In figure 3, $P_0 \leq c_a$. Now, cream-skimming appears because job-seekers with low λ ($\lambda \leq (c_a - P_0)/P_1$) are not enrolled, unless they have a large value of δ (area CR2).

Job-seekers with higher λ but low δ are worth enrolling but they are parked (area PA2). Finally, job-seekers with high δ are enrolled and treated.

Obviously, this two-tier payment structure is sub-optimal.[25] In both figures, the horizontal dashed line at c/B identifies the populations that should (above) or should not (below) be treated from the principal's point of view. Parked job-seekers are thus of two types: those that should not enter the program (from the principal's point of view) because their value of δ is too low ($\delta < c/B$) and those that should have received treatment($\delta \geq c/B$), but were not treated by the provider because P_1 was too low. In figure 2, if P_1 was set adequately, all individuals that should receive treatment would indeed receive it, but the remaining job-seekers would still be uselessly enrolled. In figure 3, some job-seekers that should be treated would remain un-enrolled. Regarding P_0, as it decreases, the share of individuals excluded from the treatment increases. More job-seekers that should not enter the program are excluded from it, but this is at the expense of excluding more job-seekers that should enter the program.

5.2   Do private and public providers cream-skim and park job-seekers?

This model predicts that, as the upfront payment P_0 increases, firms have an incentive to enroll more and more job-seekers, to the point that some of them are parked. As long as P_0 is larger than administrative costs c_a, general enrollment is optimal to the firm. We discuss here the orders of magnitude and show that this is likely to be the case for the private providers. We also discuss the public provider's behavior.

Price P_0 in private providers' contracts was set around 1,000 euros, which seems a large figure. Unofficial information on the cost of actual treatment (not parking) in the public program indicates a maximum of 600 euros per individual. It is therefore unlikely that 1,000 is below the administrative cost of enrollment with minimal effort. To illustrate, the contract with the

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[25]How first-best could be achieved with other forms of contract is beyond the scope of this discussion.

UB fund states that there should be no less than one caseworker per 40 job-seekers, with follow-up lasting 6 months at most. This sets a minimum labor cost. Correspondingly, minimum payment is thus 80 times $P_0 + \lambda P_1$ per caseworker. Given that λ is of the order of 30% after 6 months, this alone would pay an annual wage above 128,000 euros.[26] This is to be compared to the minimum wage (25,200 euros), the average wage in the service sector (40,000 euros) and the average manager's wage in the service sector (76,000 euros). Even if this is only a share of administrative costs, this price structure is therefore extremely likely to encourage private providers to enroll as many job-seekers as possible, whatever their types, even if they will only treat some of them efficiently.

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, p. 44). Moreover, the evidence reviewed in table 1 is consistent with more heterogeneity in caseworkers' involvement with job-seekers in the private program: while some of them are met very frequently, a significant share of job-seekers declare that they see the caseworker less than once a month.

Therefore, we expect private providers' behavior to be characterized by the case presented in figure 2 with $P_0 > c_a$: as many job-seekers as possible are enrolled and a share of them (depending on P_1) are parked. This could explain a lower impact relative to the public program if the behavior of caseworkers in the public program is different. One difficulty here is that there is

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[26]For a 6 month period, a caseworker has a portfolio of 40 job-seekers generating 40,000 euros, in addition 30% of them, i.e. 12, get a job within the 6 month, period generating 24,000 euros. For a six-month period, the parking strategy yields at least 64,000 euros, or 128,000 euros for the year.

no specific contract defining the incentives in the public program; moreover, other elements (non-financial incentives, altruistic behavior, etc.) may play an important role. However, caseworkers and managers in the public program share the general objectives of the PES. These objectives are set by the government. As of 2007-2008, the PES had to (i) act as the main intermediary between firms and job-seekers; (ii) direct job-seekers towards programs (e.g. training) funded by the State, the UB fund, and local authorities; (iii) manage the unemployment registers; (iv) counsel job-seekers (Convention tripartite, 2006). The PES chooses quantitative objectives based on these general goals. These objectives are of two broad types: the number of exits to employment (the job-seekers' perspective) and the number of job vacancies filled (the firms' perspective). These objectives are passed on to managers and caseworkers, even though maybe in a looser way than for the private providers. But the main difference is probably the fact that the public program has no incentive to maximize the enrollment of job-seekers, for at least two reasons: its main criterion of success is a placement rate (implying that job-seekers with lower labor market prospects and/or lower program impacts are to be avoided), and the human resources available for the program are limited. The public program's caseworkers come from the standard track, where they are a scarce resource. This is in sharp contrast with the private program, for which each additional job-seeker implies new resources (P_0). In addition, the ANPE's decision to develop the program for a broad population (instead of restricting eligibility to UB recipients) makes it possible to be selective while filling in the available slots and reaching the target of 40,000 program recipients.

Overall, a highly stylized representation of the public program's implicit "payoff" could be written as $\tilde{P}_0 + \tilde{P}_1(\lambda + \delta)$, just like for the private program, but with $\tilde{P}_0 << P_0$ ($\tilde{P}_0$ close to 0, and even possibly negative). The behavior implied by the previous model shows that in this case, caseworkers tend to cream-skim. Some job-seekers with low λ that should receive the treatment are excluded from the program, and figure 2 could be a fair approximation of the public program, with a view to contrasting it with the private one.

This representation has testable implications: we now turn to them and show that this model can explain the different performances of the two programs.

5.3 Empirical facts that the model can explain

Take-up. A first implication of the model is that private providers should maximize enrollment into their program unconditionally. And indeed, as already mentioned, program participation is substantially higher in the private program than in the public program: 43% instead of 32% in similar areas (see table 3), i.e. a 30% difference.

However, these take-up figures suggest that a significant share of the low participation must be explained by job-seekers' reluctance to enter the counseling programs. As detailed in section 5.4 below, based on survey data, many of the refusing job-seekers expect to find better jobs if they search on their own (or they report that they already have a job in view). This points to behavior where individuals with high λ are reluctant to enter, and expected value of jobs to them is lower in the program. We can represent this very simply by assuming that job-seekers who enter the program are such that:

$$(\lambda + \delta)V^T - \Psi > \lambda V^{NT},$$

where V^T is the perceived value of a job found within the program and V^{NT} that of a job found on one's own, with $V^T < V^{NT}$; and Ψ is a psychic cost of program participation. Under these assumptions, a share of the population will not enter the program: this is represented by the area below the rising line in figures 4 and 5, in a case where job-seeker self-selection is such that agency problems remain.[27] In the end, most of the low take-up may be explained

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[27]With a very high psychic cost Ψ, only high δ individuals would remain and the agency problem would no longer be apparent – but in this case, participation rates would be similar in both programs and none of the empirical facts presented below would be present.

by individual behavior, but the fact that take-up is relatively higher in the private program is still compatible with the fact that a high P_0 induces private firms to enroll as many people as possible. This behavior is again illustrated by Divay (2009), who reports that caseworkers make every possible effort to persuade all job-seekers to enroll: they play down the constraints of the program, suggest that refusals could be misinterpreted by the UB administration, etc. Interviewed caseworkers explain that their management's main concern is about unfilled slots.

Cream-skimming. The second implication of the model is that private providers should not cream-skim, whereas the public program might do so. Looking at figures 4 and 5, it is apparent that self-selection should bias enrollment towards individuals who have lower values of λ and this is indeed what the survey evidence already discussed seems to suggest. This should be mostly apparent for the private program, because no cream-skimming is expected to occur.

Thanks to the randomization design, we can compute the counterfactual outcomes of the compliers to the two programs if they had not been treated, $E[y_0|Comp_{pr}]$ and $E[y_0|Comp_{pub}]$.[28] This is a measure of λ in the model. As shown in details in the statistical Appendix, even though our setting has two treatment groups rather than one, the standard interpretation of the two-stage least squares (2SLS) estimands holds:

$$c_s = E[y_s - y_0|Comp_s]$$

with $s = pr, pub$, so that $E[y_0|Comp_s]$ (for compliers to program s) is identified as:

$$
\begin{aligned}
E[y_0|Comp_s] &= E[y_s|Comp_s] - E[y_s - y_0|Comp_s] \\
&= E[y|Z = s, Comp_s] - c_s
\end{aligned}
$$

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[28]Note that, as there are almost no "always takers", "compliers" here are almost the same as program participants.

(where Z is assignment to treatment). Estimates of impacts c_{pr} and c_{pub} have been presented in the previous section. The empirical counterpart of $E[y|Z = s, Comp_s]$ is directly observed. The resulting counterfactual own job-finding rates are presented in table 8 (Panel B) together with observed outcomes for compliers (Panel A) and outcomes in the control group (Panel C) for the two outcomes concerning the private providers. Using the outcome "found and kept eligible job", we can see that the quality (in terms of own job-finding rates) of the population treated by private providers is significantly lower than that of the total population (18.4% probability of getting such a job within 6 months, compared with 21.4% in the control group); the same is observed for the other outcome, although the differences are smaller. In contrast, the population that enters the public program is similar to the control group: this is compatible with the fact that self-selection in compensated by some cream-skimming. To sum up, there is no evidence of cream-skimming from private providers: they seem to maximize entry, even at the risk of enrolling job-seekers with low own job-finding rates, something that is rational under high P_0.

LATE A third stylized fact is that intention to treat (ITT) parameters should be closer between the private and public programs than are local average treatment effects (LATE). With reference to figures 4 and 5, ITTs are:

$$\text{ITT}_{pr} = E(\delta|\text{Area TR3}) \Pr(\text{Area TR3})$$

$$\text{ITT}_{pub} = E(\delta|\text{Area TR4}) \Pr(\text{Area TR4})$$

and

$$\text{LATE}_{pr} = \frac{E(\delta|\text{Area TR3}) \Pr(\text{Area TR3})}{\Pr(\text{Area TR3}) + \Pr(\text{Area PA3})}$$

$$\text{LATE}_{pub} = \frac{E(\delta|\text{Area TR4}) \Pr(\text{Area TR4})}{\Pr(\text{Area TR4}) + \Pr(\text{Area PA4})}$$

ITTs are presented in table 9. For the outcome "find and keep eligible job within 6 months", coefficients are 0.020 for private providers and 0.022 for the public provider respectively; for the outcome "find eligible job within 6 months", they are 0.15 and 0.20. Relative values of the LATEs are somewhat different (table 7): 0.050 vs. 0.072 and 0.039 vs. 0.063 respectively for the two outcomes.

Close ITTs are consistent with a situation where the populations *actively treated* by the two programs are not very different in terms of size and composition (in our figures, areas TR3 and TR4 are close). The difference in LATEs in turn implies that the enrollment rate is higher in the private program. This is consistent with the model's prediction that the public program does not enroll some job-seekers who would be enrolled but parked in the private program: they increase the LATE denominator, without improving its numerator. Put differently, LATEs differ while ITTs are similar because Pr(Area PA3) > Pr(Area PA4), with the difference between those two areas coming from individuals who are not treated in either case, and thus do not contribute to the ITTs.

In a nutshell, comparing ITTs and LATEs suggests that the difference in LATEs between the two programs comes from the fact that some of the additional job-seekers that are enrolled by the private program are not treated, or inefficiently so. In that sense, this model can explain the differences in LATEs observed initially. It is also compatible with additional empirical facts.

5.4 Alternative explanations

Our preferred explanation of the private program's lower performance is thus based on the assertion that private providers tried to enroll as many job-seekers as possible, including some for whom job-search assistance is ineffective. By contrast, the public program remained concentrated on a lower fraction of job-seekers for whom the technology is effective. We now compare this proposed explanation with other possible stories.

Job-seekers' self-selection. One way to account for differential participation in the two programs is differential self-selection by the job-seekers. It is possible that more job-seekers were willing to enter the private program, with the hope of finding services that were not provided within the PES (in either the standard track or the intensive public program). If these additional job-seekers are mistaken – and actually derive no benefit from the private providers' assistance –, their presence among program recipients will inflate enrollment rates but reduce the average program impact.

The reasons declared by job-seekers for enrolling or not into the programs, as reported in Gratadour (2009), do not support this unbalanced self-selection story. In particular, enrollees in the private and public programs declared very similar motivations. 84% of private program enrollees and 83% of public program enrollees said they wanted to "receive more training and assistance" in their search for a job, 80% and 83% (for private and public programs respectively) said they expected to "receive more job offers fitting [their] profile", 79% and 84% wanted to "benefit from a more intense and customized follow-up". If anything, enrollees were expecting slightly less from the private program. Similarly, the most frequently-cited reasons for not enrolling were "already having a job in view" (52% and 55% for job-seekers who did not enrol when assigned to the private and public programs respectively) or "expecting to find a job thanks to own network" (54% and 54%). In sum, even though these self-reported motivations are only indirect evidence, they do not indicate that job-seekers self-selected differently into the private program.

Learning. The next two alternative explanations relax the assumption that private providers were as efficient technically as the public program. Although private providers used the same approach as the public program, it is often argued that private providers were just starting this activity and needed time to become fully operational. Another argument is that private

providers did not have access to the appropriate pool of job offers, whereas the PES had built relationships with small and medium-size firms where it could place job-seekers. These two explanations are plausible. However, it seems unlikely that they tell the full story; we consider them in turn.

It may have taken some time for the private providers to become operational, to recruit and train the caseworkers, whereas the public service could allocate some of its experienced caseworkers to the intensive scheme. This would imply that the private program was less efficient during the first months of the experiment, and that its impact then caught up with that of the public program. In order to check this, we can compute program impacts separately by cohort of entry. Figure 6 traces program impact after 6 months, for the two programs, in areas where they are both present. This is plotted on the left-hand scale, where the impact is estimated separately by 2-month cohorts. This is to be compared with Table 5, where the average impact is estimated at 0.045 and 0.102 for private and public programs respectively. The figure shows that these average estimates hide very strong changes in impact over time. But this certainly does not go in the direction of learning-by-doing. The two programs had very strong impacts at the beginning, then their impact dropped essentially to zero, and recovered thereafter. The small sample sizes imply that estimates are now imprecise and the effects are significant only at the beginning and the end of the period. However, the likely reason for this switch appears on the right-hand scale, that shows the variation in the number of new recipients in the program. This number increased 3- to 4-fold during the first two months of the experiment. In both programs, efficiency fell when the number of recipients started to increase. This is compatible with the idea that such programs are efficient mostly thanks to the low unemployed/caseworker ratio. All in all, Figure 6 is very consistent with the general findings of the paper, but does not deliver evidence that private providers lagged behind because of learning-by-doing issues.

Access to job offers. As noted in section 2, there are indications that the private program caseworkers provided job-seekers with fewer job offers. However, the reason for this remains an open question: it may be that private providers were less efficient at collecting job offers, or that they invested less in that activity. The first explanation would be rather ironical, as a key argument for introducing private providers in job-search assistance was their better connection to the labor market (compared to a less market-oriented public employment agency). Private providers were indeed experienced players in the labor market: temporary agencies, consultancies specialized in the placement of workers after mass layoffs, or even international firms specialized in job-search assistance programs. Given that experience, a full explanation would have to account for their failure to invest successfully in job offer collection. Ultimately, this goes back to the incentives question.[29]

6 Discussion

The large field experiment we have just analyzed yields some basic facts – job search assistance accelerates exit to employment, and public provision is more cost-effective in our context. As such, these results may be highly context-specific. The evidence on unexpected perverse effects of the private providers' contracts allows to go one step further, by highlighting the traps and trade-offs faced when outsourcing such services. There remains however a disturbing question in the analysis: why would the UB fund design contracts with such large, suboptimal fixed payments P_0?

First, one should not dismiss the idea that outsourcing such services involves a trial-and-error process. Informal discussions with policy advisors suggest that the discussion on contract struc-

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[29]Interestingly, qualitative work at one private provider shows that the internal organization of work devolved limited resources to job offer collection. No specific staff was devoted to the activity, and caseworkers were supposed to spend only two hours per week on collecting job offers. Moreover, the caseworkers' financial incentives were almost uniquely based on placement results. Interviewed caseworkers argued that developing a portfolio of client firms with whom to place the job-seekers was not worth the effort: they rather targeted new firms on a case-by-case basis according to each job-seeker's profile, and limited capitalization effort was undertaken; after one year, a given caseworker said he had a portfolio of fewer than twenty client firms (Divay, 2009).

ture was actually limited. It is plausible that the UB fund failed to recognize the consequences of the large upfront payment. However, a similar structure (including a slightly higher upfront payment, from 30% to 35%!) was repeated for a new wave of contracts involving 320,000 job-seekers in 2010-2011: this suggests that learning on contracts is slow, or that it is not the full explanation.[30]

A second possibility is that our theoretical model is too simple: admittedly, a more complete agency model could rationalize large fixed payments. In particular, one could relax the assumption of risk-neutral private providers. Large fixed payments may then play a role in reassuring risk-averse private providers, as discussed for instance in Koning and Heinrich (2010), although it is not clear whether this would be the most appropriate mechanism. More generally, our simple model takes the two-tier contract structure as given, and just asks about the optimal share of conditional and fixed payments. This is a very restrictive class of contracts, and it would be very useful to investigate – and experiment with – a broader class of contracts.[31]

However, we believe that the main explanation has to do with the political economy context of the experiment. The UB fund was eager to create a previously nonexistent private market for placement. Firms likely to enter this market in France would need to bear large fixed costs and would not enter 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

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[30]Policy makers focused more attention on the other element of the contract – the winning bid price. As noted above, this ranged from 3,000 to 4,000 euros. These amounts are hard to benchmark; in the Netherlands, the price of trajectories seems to be of the same order of magnitude; but trajectories could last up to two years, compared to 6 months in France. A better benchmark would be the cost of the comparable public program. However, in the context of this evaluation, the PES abandoned its effort to evaluate the unit cost of the public program half-way through, arguing that the numbers would not be reliable. While imputing the private program costs in the PES accounting system was undoubtedly difficult, it had also clearly become politically incorrect to compare the costs of the public and private programs at a time when the decision had been taken to expand the private program dramatically.

[31]In particular, contracts could involve a fee Q for the right to deliver the program under the two tier payment (P_0, P_1). P_0 and P_1 could be set to align the objectives of the private providers with those of Unédic ($P_0 = c_a$ and $P_1 = B\frac{c-c_a}{c}$). Under such a contract, all job-seekers would be enrolled into the program and only job-seekers with $\delta > B/c$ would actually receive the treatment. Unédic could then request a fee $Q = NF_\delta(B/c)$, where N is the size of the population eligible for the program. Of course, this would have to be adjusted for the fact that the distribution of δ may not be known ex ante. Also, note that c_a is paid for job-seekers that are not treated, but this may be viewed as a cost which would be incurred anyway if these job-seekers were enrolled in the standard track.

transfer to the firms, rather than a transfer indexed on the number of enrolled individuals, but this was never considered (and would raise moral hazard issues of its own). Moreover, internal pressures (from employers' organizations within the UB fund and from the government) and external ones (from the example of other EU countries and from the European Commission) converged to promote private providers. The infant industry argument may have been used as a justification for offering attractive contracts to private providers. Imitation was also at play: the chosen contract structure is close to outsourcing contracts used in several other European countries.[32]

7 Conclusion

The episode analyzed in this paper offers stimulating evidence: to sum up, in the context under study, counseling is effective (in the sense of accelerating placement), but contracts to private providers (using a structure that has become common to many OECD countries) appear to be suboptimal. Similar findings were obtained by Krug and Stephan (2011) based on a field experiment in Germany. Interestingly, even though the outsourcing trend has spread to many countries over the past decade, it is not monotonic: for instance, in the Netherlands, the outsourcing of welfare services was partly reversed in 2007, as the social benefit administration resumed the delivery of welfare-to-work services for job-seekers with good labor market prospects (Koning and Heinrich, 2010). It is also interesting to note that the US Public Employment Office was created in the US at the turn of the 20th century to regulate the job-seeker placement market,

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[32]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 on 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. See Finn (2011) for a review.

which was dominated by for-profit employment agencies who exploited vulnerable individuals (Lee, 2009).

Clearly, this does not mean that private providers should not be used for job-search assistance: we find in particular some (admittedly fragile) evidence that competition from private providers may in part explain the performance of the public program. But public authorities, in France at least and in other countries perhaps, are still at the beginning of a learning curve. That 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 minimise 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, p. 32). An ideal direction for research would be to design outsourcing contracts that take into account micro evidence on job-seekers', caseworkers' and firms' behavior, and experiment them with robust evaluation methods.

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Figure 1: Experimental populations
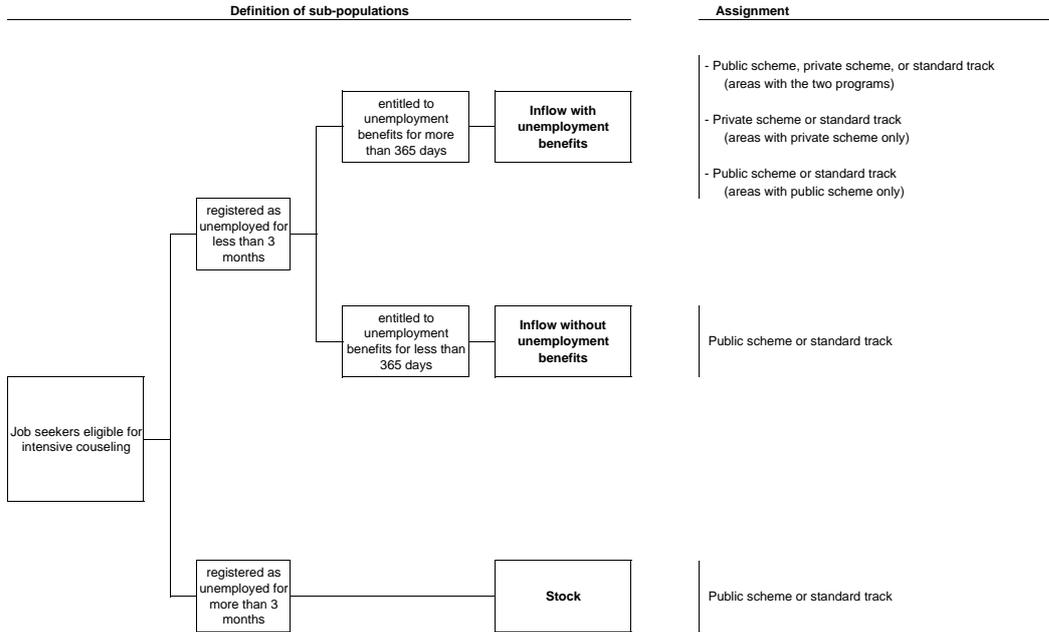
Figure 2: Private provider behavior large P_0
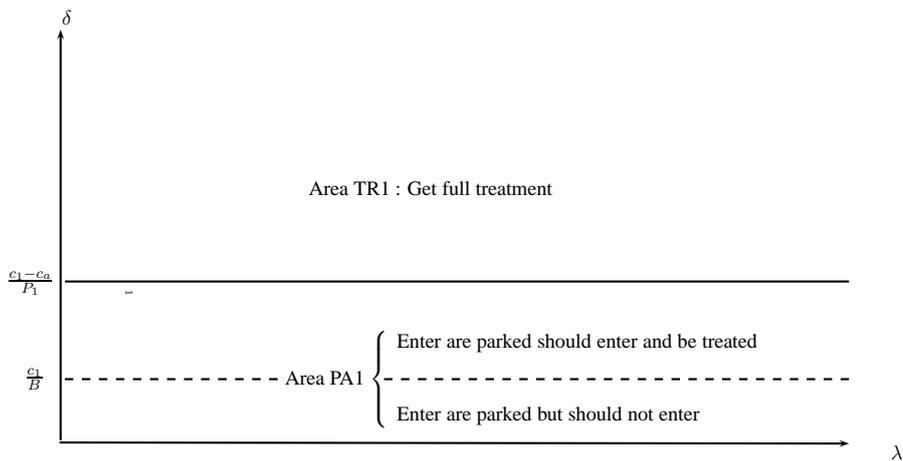
Area TR1 is the area of individuals that enter the program and receive treatment. Area PA1 is the area of individuals that enter the program but do not receive any treatment.

Figure 3: Private provider behavior small P_0
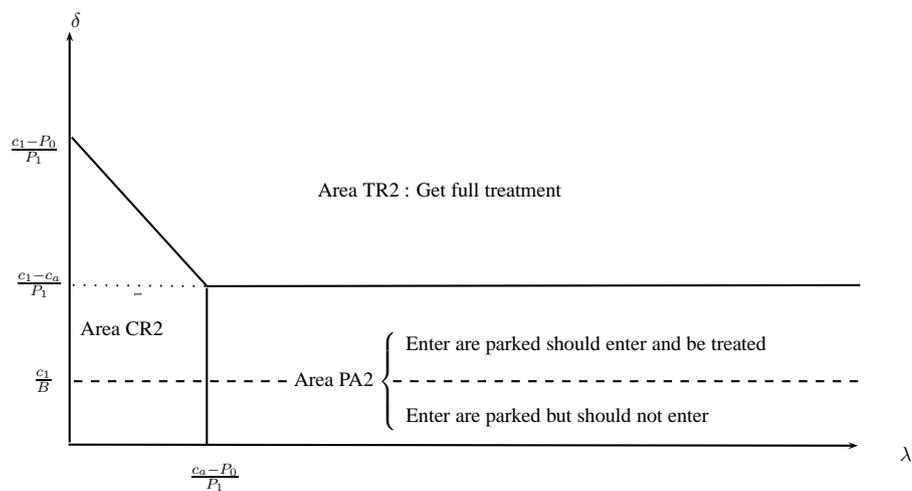
Area TR2 is the area of individuals that enter the program and receive treatment. Area PA2 is the area of individuals that enter the program but do not receive any treatment. Area CR2 is the area of individuals excluded from the treatment.

Figure 4: Private provider behavior large P_0 and decision to enter
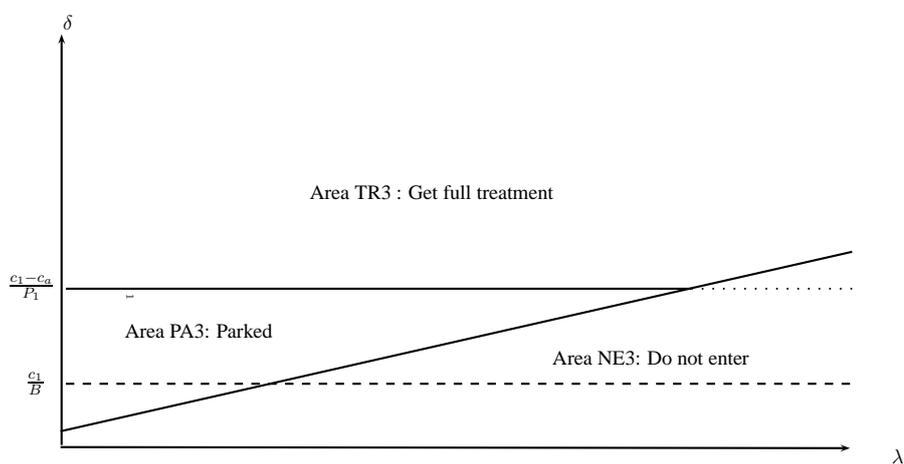
Area TR3 is the area of individuals that enter the program and receive treatment. Area PA3 is the area of individuals that enter the program but do not receive any treatment. Area NE3 is the area of individuals that decided not to enter the treatment

Figure 5: Private provider behavior small P_0 and decision to enter
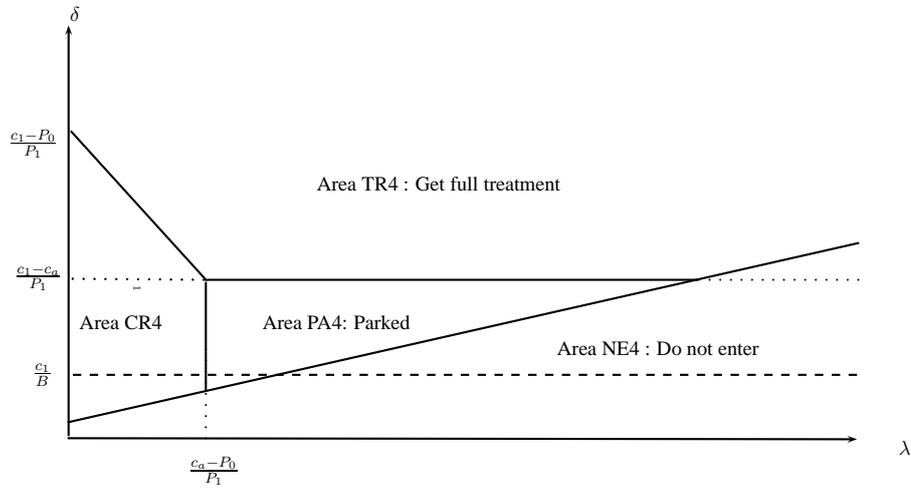
Area TR4 is the area of individuals that enter the program and receive treatment. Area PA4 is the area of individuals that enter the program but do not receive any treatment. Area CR4 is the area of individuals excluded from the treatment. Area NE4 is the area of individuals that decided not to enter the treatment

Figure 6: Program impact and cohort size over time: areas with both programs
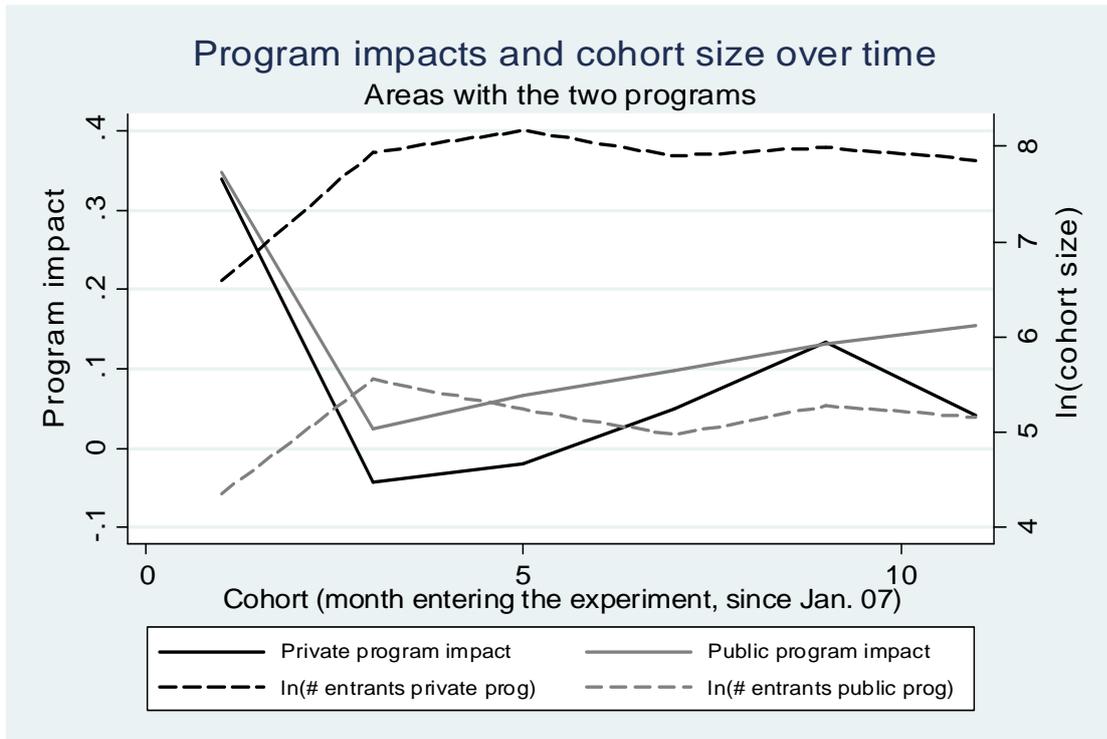
Table 1: Impact of public and private programs on counseling services (areas with both programs)

	Standard track (intercept)	Private program impact	Public program impact	p-value public vs. private
	Frequency of face-to-face meetings			
Once a week or more	0.12***	0.13	-0.07	0.01
	(0.03)	(0.08)	(0.10)	
2-3 times per month	0.06***	0.24***	0.18**	0.40
	(0.02)	(0.06)	(0.08)	
Once a month	0.35***	-0.16	0.26	0.00
	(0.05)	(0.11)	(0.16)	
Less than once a month	0.39***	-0.16	-0.34**	0.09
	(0.05)	(0.11)	(0.16)	
Never	0.07***	-0.05	-0.03	0.61
	(0.02)	(0.06)	(0.08)	
	Number of job offers presented			
None	0.29***	0.21*	0.01	0.09
	(0.04)	(0.11)	(0.15)	
1 to 5	0.38***	-0.07	-0.09	0.82
	(0.05)	(0.11)	(0.16)	
6 or more	0.32***	-0.19*	0.00	0.08
	(0.04)	(0.11)	(0.15)	
Unanswered	0.01	0.05*	0.08	0.51
	(0.01)	(0.03)	(0.05)	
	Training programs received			
Skills assessment	0.35***	-0.07	-0.22	0.16
	(0.05)	(0.11)	(0.15)	
Curriculum vitae	0.42***	0.20*	-0.07	0.02
	(0.05)	(0.11)	(0.16)	
Cover letter	0.41***	0.06	-0.14	0.10
	(0.05)	(0.11)	(0.16)	
Firms targeting	0.23***	0.32***	0.28**	0.71
	(0.04)	(0.10)	(0.14)	
Job interviews	0.24***	0.21**	0.11	0.30
	(0.04)	(0.10)	(0.14)	
Internet	0.29***	0.19*	0.28*	0.47
	(0.04)	(0.11)	(0.16)	
Longer vocational training	0.16***	-0.10	-0.13	0.66
	(0.03)	(0.08)	(0.11)	

Each line displays results from a separate 2SLS regression with program participation instrumented by program assignment. Observations are weighted by the inverse of the assignment probability. The last column displays the p-value for the test of equal program impacts. Robust standard errors in parenthesis. *, **, ***: significance at 10%, 5% and 1% levels.

Reading: 12% of job-seekers in the standard track see a caseworker once a week or more. Participation to the private program increases that probability by 13 pp, while participation to the public program reduces it by 7 pp. These effects are not statistically significant, but they are statistically different from one another.

Sample: Inflow with unemployment benefits, areas with the two programs.

Table 2: Descriptive characteristics among experimental groups and program recipients

Inflow with benefits

	Assigned Standard (1)	Assigned Private (2)	(1)-(2)	Entered Private (3)	(2)-(3)	Assigned Standard (1)	Assigned Public (2)	(1)-(2)	Entered Public (3)	(2)-(3)
College education	0.25	0.26		0.26		0.28	0.28		0.29	***
Vocational	0.37	0.38		0.38	*	0.38	0.38		0.38	
High school dropout	0.16	0.16		0.16	***	0.13	0.14		0.12	***
Aged 30-49	0.50	0.51		0.53	***	0.50	0.50		0.52	***
Aged above 50	0.16	0.15	**	0.15		0.12	0.11		0.10	***
woman	0.52	0.51		0.52		0.56	0.56		0.57	
French citizen	0.92	0.92		0.93	***	0.92	0.93		0.94	***
Married	0.44	0.45		0.45	**	0.43	0.43		0.43	
Lost job in mass layoff	0.53	0.53		0.56	***	0.29	0.30		0.31	
Number of children	0.80	0.82		0.84	**	0.81	0.79		0.81	

	Inflow without benefits					Stock				
	Assigned Standard (1)	Assigned Public (2)	(1)-(2)	Entered Public (3)	(2)-(3)	Assigned Standard (1)	Assigned Public (2)	(1)-(2)	Entered Public (3)	(2)-(3)
College education	0.30	0.31		0.33	***	0.26	0.27		0.28	***
Vocational	0.29	0.29		0.30	**	0.33	0.32		0.34	**
High school dropout	0.20	0.20		0.16	***	0.21	0.22		0.19	***
Aged 30-49	0.46	0.46		0.44	***	0.57	0.57		0.57	
Aged above 50	0.09	0.09		0.08	***	0.13	0.13		0.12	***
Woman	0.56	0.56		0.58	***	0.54	0.53	**	0.52	
French citizen	0.75	0.75		0.79	***	0.79	0.80		0.82	***
Married	0.39	0.39		0.38		0.44	0.43		0.44	
Lost job in mass layoff	0.12	0.11		0.11	***	0.31	0.31		0.31	
Number of children	0.82	0.84		0.77	***	0.96	0.93	*	0.90	*

Each cell displays a proportion (except number of children, which displays an average). Columns (1) and (2) characterize job-seekers by their random assignment; column (3) corresponds to those entering the programs. *, **, ***: significance at 10%, 5% and 1% levels for the differences between the coefficients in stated columns.

Sample: all areas, by assignment and sub-population.

Table 3: Program participation

	Private program	Public program			Public and private programs	
		inflow with benefits	inflow w/o benefits	Stock	(inflow with benefits, areas with 2 programs)	
Intercept	0.344***	0.338***	0.295***	0.296***	0.321***	0.285***
	(0.013)	(0.023)	(0.013)	(0.017)	(0.009)	(0.036)
College education	-0.004	0.004	0.019*	-0.002		-0.007
	(0.009)	(0.016)	(0.011)	(0.012)		(0.027)
Vocational	0.008	-0.018	0.008	0.001		-0.025
	(0.009)	(0.015)	(0.011)	(0.012)		(0.027)
High school dropout	-0.025**	-0.053***	-0.050***	-0.043***		-0.030
	(0.011)	(0.019)	(0.012)	(0.013)		(0.031)
Aged 30-49	0.033***	0.025*	-0.018**	0.000		0.011
	(0.008)	(0.013)	(0.009)	(0.010)		(0.023)
Aged above 50	0.016	-0.035*	-0.059***	-0.028**		-0.040
	(0.010)	(0.018)	(0.013)	(0.014)		(0.030)
Woman	0.003	0.004	0.021***	-0.014*		-0.001
	(0.007)	(0.011)	(0.008)	(0.008)		(0.019)
Foreigner	0.077***	0.076***	0.056***	0.041***		0.049*
	(0.009)	(0.018)	(0.009)	(0.011)		(0.026)
Married	0.006	0.003	0.026***	0.023**		0.010
	(0.007)	(0.013)	(0.009)	(0.009)		(0.022)
Lost job in mass layoff	0.055***	-0.011	-0.024**	-0.008		0.013
	(0.007)	(0.012)	(0.011)	(0.009)		(0.019)
Number of children	0.003	0.004	-0.006*	-0.003		-0.001
	(0.003)	(0.006)	(0.003)	(0.004)		(0.009)
Assigned to private programm (private)					0.108***	0.035
					(0.010)	(0.039)
Private × College education						0.006
						(0.029)
Private × Vocational						0.029
						(0.029)
Private × High school dropout						0.019
						(0.033)
Private × Aged 30-49						0.021
						(0.025)
Private × Aged above 50						0.053*
						(0.032)
Private × Woman						0.005
						(0.020)
Private × Foreigner						0.014
						(0.027)
Private × Married						-0.000
						(0.023)
Private × Lost job in mass layoff						0.046**
						(0.021)
Private × Number of children						0.002
						(0.010)
Observations	45,460	9,875	29,257	33,315	39,412	39,412
R-squared	0.466	0.407	0.342	0.317	0.421	0.426

Linear probability models of program participation among job-seekers assigned to intensive counseling. Each column is a different regression. Observations are weighted by the inverse of the assignment probability. Robust standard errors in parentheses. *, **, ***: significance at 10%, 5% and 1% levels.
Reading: 34.4% of the reference individuals (French male youth with HS diploma and no children) assigned to the private program entered that program. The rate is 7.7 percentage points higher among foreigners.
Sample: all areas, by program and sub-population.

Table 4: Private program and public program impacts on exit to employment: all areas

	Private program (Inflow with UB)	Public program Inflow with UB	Inflow with no UB	Stock
Within 3 months	0.016	0.090***	0.054***	0.023
	(0.014)	(0.025)	(0.016)	(0.016)
Within 6 months	0.042**	0.091***	0.076***	0.036*
	(0.017)	(0.029)	(0.017)	(0.019)
Within 9 months	0.058***	0.102***	0.069***	0.059***
	(0.020)	(0.028)	(0.017)	(0.019)
Within 12 months	0.056***	0.073**	0.064***	0.063***
	(0.020)	(0.029)	(0.017)	(0.020)
Sample size	68,390	68,390	63,941	86,702

Estimates from separate 2SLS models with program participation instrumented by program assignment. Each cell displays the impact of intensive counseling on a given subgroup within x months of assignment, except for columns (1) and (2) that are estimated jointly on the Inflow with UB population. Observations are weighted by the inverse of the assignment probability.

Robust standard errors in parentheses. *, **, ***: significance at 10%, 5% and 1% levels.

Reading: participation in the private program increases exit to employment by 1.6 pp within 3 months.

Sample: all areas, by program and sub-population.

Table 5: Private program and public program impact on exit to employment: inflow with UB, by areas

	Areas with both programs		Areas with one program	
	Private program	Public program	Private program	Public program
Within 3 months	0.021	0.110***	0.002	0.033*
	(0.017)	(0.033)	(0.023)	(0.019)
Within 6 months	0.045**	0.102***	0.034	0.060***
	(0.021)	(0.038)	(0.029)	(0.021)
Within 9 months	0.063***	0.119***	0.042	0.054**
	(0.024)	(0.037)	(0.032)	(0.021)
Within 12 months	0.064**	0.085**	0.032	0.038*
	(0.025)	(0.038)	(0.033)	(0.021)
Sample size	43,977	43,977	11,145	13,268

Estimates from separate 2SLS models with program participation instrumented by program assignment. Each cell displays the impact of intensive counseling on a given subgroup within x months of assignment, except for columns (1) and (2) that are estimated jointly on the Inflow with UB population. Observations are weighted by the inverse of the assignment probability.

Robust standard errors in parentheses. *, **, ***: significance at 10%, 5% and 1% levels.

Reading: participation in the private program increases exit to employment by 4.5 pp within 6 months, in areas where the public program is also implemented.

Sample: Inflow with UB only, distinguishing areas with 1 and 2 programs.

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Table 6: Private program and public program impact on exit to employment: inflow with UB, by areas and length of exit

| | Areas with both programs | | | | Areas with one program | | | |
| | Private program | | Public program | | Private program | | Public program | |
Exit length	< 6 months	≥ 6 months	< 6 mois	≥ 6 months	< 6 months	≥ 6 months	< 6 months	≥ 6 months
Within 3 months	-0.008	0.029*	0.032*	0.078***	0.006	-0.004	0.026**	0.007
	(0.009)	(0.015)	(0.018)	(0.029)	(0.012)	(0.021)	(0.011)	(0.016)
Within 6 months	-0.007	0.052***	0.028	0.075**	-0.002	0.036	0.026**	0.034*
	(0.011)	(0.020)	(0.021)	(0.035)	(0.016)	(0.027)	(0.013)	(0.019)
Sample size	43,977	43,977	43,977	43,977	11,145	11,145	13,268	13,268

Estimates from separate 2SLS models with program participation instrumented by program assignment. Each cell displays the impact of intensive counseling on a given subgroup within x months of assignment, except for columns (1) and (3) that are estimated jointly on the Inflow with UB population; Same for columns (2) and (4). Observations are weighted by the inverse of the assignment probability. Robust standard errors in parentheses. *, **, ***: significance at 10%, 5% and 1% levels.

Reading: participation in the private program decreases the probability of exit to employment for less than 6 months by 0.8 percentage point, but it increases the probability of more durable exits by 2.9 percentage points, in areas with both programs.

Sample: Inflow with UB only, distinguishing areas with 1 and 2 programs.

Table 7: Private and public program impacts, by outcome

	Impact after 3 months			Impact after 6 months		
	Exit to employment	Find eligible job	Find and keep eligible job	Exit to employment	Find eligible job	Find and keep eligible job
Private program	0.021	0.027	0.029*	0.045**	0.039	0.050**
	(0.017)	(0.020)	(0.016)	(0.021)	(0.023)	(0.021)
Public program	0.110***	0.098***	0.085***	0.102***	0.063*	0.072**
	(0.033)	(0.038)	(0.031)	(0.038)	(0.041)	(0.036)
Difference	-0.088***	-0.071**	-0.056**	-0.057*	-0.024	-0.022
	(0.027)	(0.031)	(0.026)	(0.031)	(0.032)	(0.029)
p-value	0.001	0.020	0.029	0.063	0.450	0.459
Sample size	43,977	43,977	43,977	43,977	43,977	43,977

2SLS estimates with program participation instrumented by program assignment. Each column is a separate regression. The third line measures the difference between the two coefficients and p-value is for the test that this difference is zero. Eligible jobs defined according to private companies' contract: $\geq$ 110 hours per month, $\geq$ 6 months. Observations are weighted by the inverse of the assignment probability. Robust standard errors in parentheses. *, **, ***: significance at 10%, 5% and 1% levels.

Reading: participation in the private program increases the probability of exit to employment within 3 months by 2.1 percentage points, while participation in the public program increases it by 11 percentage points. The 8.8 percentage point difference is significant at the 1% level.

Sample: only areas that applied both programs.

Table 8: Cream-skimming in private and public programs

	% found eligible job	% found and kept job
	Panel A: observed outcomes among compliers ($E(y_s|C_s)$)	
Private program	0.384	0.234
Public program	0.447	0.288
	Panel B: counterfactual outcomes for compliers with no treatment impact ($E(y_0|C_s)$)	
Private program	0.345	0.184
p-value diff with control	.47	.03
Public program	0.384	0.216
p-value diff with control	.35	.93
	Panel C: outcomes in control group	
Control	0.357	0.214

Actual and counterfactual outcomes among program compliers and control group job-seekers (see text, section 5.3 for details).

Counterfactual outcomes are computed from table 7 estimates. Eligible jobs defined according to private companies' contract:

$\geq$ 110 hours per month, $\geq$ 6 months. Observations are weighted by the inverse of the assignment probability.

Reading: 38.4% of private program compliers found an eligible job, while 34.5% would have found one without the program.

The rate is 35.7% in the control group.

Sample: only areas that applied both programs.

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Table 9: ITT Private and public program impacts, by outcome

| | ITT after 6 months | |
	Find eligible job	Find and keep eligible job
Private program	0.015*	0.020**
	(0.009)	(0.008)
Public program	0.020	0.022**
	(0.013)	(0.011)
Difference	-0.004	-0.002
	(0.017)	(0.015)
p-value	0.689	0.812

OLS estimates displaying impacts of assignment to public and private programs. Each column is a separate regression. The third line measures the difference between the two coefficients and p-value is for the test that this difference is zero. Eligible jobs defined according to private companies' contract: $\geq$ 110 hours per month, $\geq$ 6 months. Observations are weighted by the inverse of the assignment probability. Robust standard errors in parentheses. *, **, ***: significance at 10%, 5% and 1% levels.

Reading: assignment to the private programs increases chances of finding an eligible job by 1.5 pp.

Sample: only areas that applied both programs.

Appendix - Not for publication

Data appendix

The primary source of information is the administrative records held by the ANPE. They provide basic sociodemographic information on job-seekers and allow to compute the duration of all registered unemployment spells in the experimental sample. However, as is well-known and well illustrated by Card et al. (2007), the end of a registered unemployment spell may be due to quite different events. In our data, it is in some cases clear whether the job-seeker has found a job or left the labor force (for about 50% of exits from registered unemployment, the job-seeker returned a form stating his motives or the information was coded directly by the employment services during an interview or a phone contact), but in about 50% of cases, there is no way to know from the administrative records whether the job-seeker had found a job or not. From this single source of information, we cannot tell whether an exit from registered unemployment is due to job placement or to a "discouraged job-seeker" effect. As part of the experimental design, an independent survey company was therefore commissioned to conduct a very short phone survey on a subsample of workers whose destination on leaving unemployment was not identified in the administrative records. The questionnaire was extremely focused so as to copy the form that other job-seekers had filled out upon exiting registered unemployment. It had a maximum of 4 questions. We use the first: "Question 1. During the month of ..., you stopped being registered at the ANPE. What was the reason?" The sampling probabilities for this survey were optimized to partly correct for the imbalance between treatment and control groups. Moreover, 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. Thus combined, the administrative records of registered unemployment spells and the short phone

survey allow us to measure transitions from registered unemployment to employment at various horizons.[33]

Sample attrition is quite limited: transitions to employment at the six-month horizon are observed for about 90% of the sample when job-seekers are eligible for benefits or in the stock, and for about 80% of the sample for job-seekers in the inflow who are not eligible for benefits. Within these sub-populations, attrition rates do not significantly differ between control and treatment groups.[34] Given the low and balanced attrition rates, we treat non-response as random (within strata defined by sub-population × assignment).

Table 10 displays descriptive statistics on the main outcome variables used throughout the paper. Exit rates to employment correspond to the probability that a job-seeker has left unemployment to take a job within 3 months (resp. 6, 9 and 12 months) of randomization. This outcome is thus cumulative and it is analogous to (the complement of) a survival function. Depending on the group we consider, exit rates increase from 10-20% within three months to about 30-40% within twelve months. Overall, this confirms that the population targeted by the two programs has a high probability of long-term unemployment. The other variables are commented along with the results.

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[33]In parallel, the Ministry of Labor followed an alternative collection strategy by commissioning a survey on a sample of about 9,000 job-seekers, including those still registered as unemployed. The survey contains more than 100 questions, providing detailed information on the employment status and search strategy of each worker. However, it suffers from a low response rate (50%) and sample selectivity bias (Behaghel, Crépon, Gurgand and Le Barbanchon, 2009). We use that survey to describe the program contents (see above).

[34]Missing observations are due to the combination of two elements: the job-seeker stopped filling out the monthly form stating her employment status without giving a reason (and was no longer in contact with her caseworker), and the job-seeker has not responded to the phone survey.

Statistical appendix: interpretation of 2SLS in the three-treatment case

This appendix clarifies the causal meaning of our Two Stage Least Square (2SLS) estimates through a direct extension of the LATE framework (Angrist, Imbens and Rubin, 1996) allowing for three treatments (including control), with random assignment to treatment but imperfect compliance.

There are three treatments $T \in \{0, 1, 2\}$ and people can be assigned to three groups $Z \in \{0, 1, 2\}$. Consider a variable of interest Y. There are in this setting nine potential outputs $Y(t, z)$ $(t, z) \in \{0, 1, 2\} \times \{0, 1, 2\}$. As in the LATE framework we introduce program entry decisions under assignment to z: $T(z)$. Two assumptions of the LATE framework directly extend to this setting:

Assumption 1 *Exclusion* $Y(t, z) = Y(t)$ *for all* t, z

and

Assumption 2 *Independence* $Y(0), Y(1), Y(2), T(0), T(1), T(2) \perp Z$

We make a somewhat different monotonicity assumption than in the two treatment cases:

Assumption 3 *Extended monotonicity*

$$(T(2) = 1) = (T(0) = 1) \leq (T(1) = 1)$$

and

$$(T(1) = 2) = (T(0) = 2) \leq (T(2) = 2).$$

Assumption 3 implies that there are no "defiers" (who would enter treatment k when assigned to group $j \neq k$, but not when assigned to group k). It also implies that those who enter treatment

k when assigned to group $j \neq k$ are exactly those who enter treatment k when assigned to the third remaining group. For the assumption to hold, it is enough that the population contains only the following six types:

- never takers, such that: $T(0) = 0, T(1) = 0, T(2) = 0$

- program 1 always takers: $T(0) = 1, T(1) = 1, T(2) = 1$

- program 2 always takers: $T(0) = 2, T(1) = 2, T(2) = 2$

- compliers: $T(0) = 0, T(1) = 1, T(2) = 2$

- program 1 exclusive takers: $T(0) = 0, T(1) = 1, T(2) = 0$

- program 2 exclusive takers: $T(0) = 0, T(1) = 0, T(2) = 1$

The problem is how to interpret the 2SLS estimates of equation

$$Y = a + c_1(T = 1) + c_2(T = 2) + u$$

using $Z_1 = (Z = 1)$ and $Z_2 = (Z = 2)$ as instruments. Or, equivalently, how to interpret c_1 and c_2 as characterized by moment conditions:

$$E[u] = E[Z_1 u] = E[Z_2 u] = 0.$$

Proposition 1 *Under Assumption 1, 2 and 3,*

$$c_1 = E[y(1) - y(0)|(T(1) = 1) - (T(0) = 1) = 1]$$

and

$$c_2 = E[y(2) - y(0)|(T(2) = 2) - (T(0) = 2) = 1].$$

c_1 and c_2 can thus be interpreted as Local Average Treatment Effects on compliers and program 1 exclusive takers (i.e. compliers to program 1), and on compliers and program 2 exclusive takers (i.e. compliers to program 2), respectively.

Proof 1 *We have:*

$$T = T(0) + (T(1) - T(0))Z_1 + (T(2) - T(0))Z_2$$

which leads to

$$(T = k) = (T(0) = k) + ((T(1) = k) - (T(0) = k))Z_1 + ((T(2) = k) - (T(0) = k))Z_2, \quad k = 1, 2$$

Moreover, given Assumption 1, Y can be written as:

$$Y = Y(0) + (Y(1) - Y(0))(T = 1) + (Y(2) - Y(0))(T = 2).$$

Thus:

$$
\begin{aligned}
u \ = \ & Y(0) + (Y(1) - Y(0))(T(0) = 1) + (Y(2) - Y(0))(T(0) = 2) - c_0 + \\
& Z_1 \times [(Y(1) - Y(0) - c_1)((T(1) = 1) - (T(0) = 1)) + (Y(2) - Y(0) - c_2)(T(2) = 1) - (T(0) = 1))] + \\
& Z_2 \times [(Y(1) - Y(0) - c_1)((T(1) = 2) - (T(0) = 2)) + (Y(2) - Y(0) - c_2)(T(2) = 2) - (T(0) = 2))].
\end{aligned}
$$

The 2SLS orthogonality conditions and assumption 2 imply:

$$E[(Y(1) - Y(0) - c_1)((T(1) = 1) - (T(0) = 1)) + (Y(2) - Y(0) - c_2)(T(2) = 1) - (T(0) = 1))] = 0$$

and

$$E[(Y(1) - Y(0) - c_1)((T(1) = 2) - (T(0) = 2)) + (Y(2) - Y(0) - c_2)(T(2) = 2) - (T(0) = 2))] = 0.$$

These two equations can be solved in c_1 and c_2. Without further assumptions, c_k is a linear combination of $E[Y(1) - Y(0)|(T(1) = 1) - (T(0) = 1) = 1]$, $E[Y(1) - Y(0)|(T(1) = 2) - (T(0) =$

$2) = 1]$, $E[Y(2) - Y(0)|(T(2) = 1) - (T(0) = 1) = 1]$, *and* $E[Y(2) - Y(0)|(T(2) = 2) - (T(0) = 2) = 1]$, *which is hard to interpret. However, under assumption 3, the two equations greatly simplify to:*

$$(E[Y(1) - Y(0)|(T(1) = 1) - (T(0) = 1) = 1] - c_1)P((T(1) = 1) - (T(0) = 1) = 1) = 0$$

and

$$(E[Y(2) - Y(0)|(T(2) = 2) - (T(0) = 2) = 1] - c_2)P((T(2) = 2) - (T(0) = 1) = 2) = 0,$$

which yields proposition 1.

Table 10: Average of outcome variables in the control group

	Inflow with benefits in areas...				Inflow w/o benefits	Stock
	All areas	... with the two programs	... with private program only	... with public program only		
Exit to employment (%)						
Within 3 months	15	12	13	18	20	13
Within 6 months	27	23	23	30	30	22
Within 9 months	35	31	32	39	36	29
Within 12 months	41	37	38	44	40	34
Exit to employment; exit ≥ 6 months (%)						
Within 6 months	20	18	18	22	24	17
Exit for other reasons than employment (%)						
Within 6 months	13	14	12	12	28	18
Number of days unemployed over a year						
Including part-time jobs	253	261	263	246	201	253
Excluding part-time jobs	215	226	231	203	172	214

Table 11: Private program and public program impact on exit to employment: all areas, by length of exit

| Exit length | Private program (Inflow with UB) | | Public program | | | | | |
| | | | Inflow with UB | | Inflow with no UB | | Stock | |
	< 6 months	≥ 6 months	< 6 months	≥ 6 months	< 6 months	≥ 6 months	≥ 6 months	> 6 months
Within 3 months	-0.004	0.020*	0.030**	0.060***	0.021***	0.033**	0.003	0.019
	(0.007)	(0.012)	(0.014)	(0.022)	(0.008)	(0.014)	(0.009)	(0.014)
Within 6 months	-0.006	0.048***	0.027*	0.064**	0.030***	0.045***	0.011	0.025
	(0.009)	(0.016)	(0.016)	(0.026)	(0.009)	(0.016)	(0.011)	(0.017)
Sample size	68,390	68,390	68,390	68,390	63,941	63,941	86,702	86,702

Estimates from separate 2SLS models with program participation instrumented by program assignment. Each cell displays the impact of intensive counseling on a given subgroup within x months of assignment, except for columns (1) and (3) that are estimated jointly on the Inflow with UB population; Same for columns (2) and (4). Observations are weighted by the inverse of the assignment probability. Robust standard errors in parentheses. *, **, ***: significance at 10%, 5% and 1% levels.

Reading: participation in the private program decreases the probability of exit to employment for less than 6 months by 0.4 percentage point, but it increases the probability of more durable exits by 2 percentage points.

Sample: All areas, by program and sub-population.

Table 12: Heterogeneity in program impacts. Exit to employment within 6 months

	Inflow with unemployment benefits, areas with			Inflow without benefits	Stock
	two programs	private program	public program		
Intercept	0.295***	0.299***	0.398***	0.339***	0.287***
	(0.021)	(0.034)	(0.015)	(0.009)	(0.010)
College education	0.061***	0.095***	0.104***	0.125***	0.076***
	(0.020)	(0.036)	(0.015)	(0.009)	(0.009)
High school graduate	-0.007	0.009	0.051***	0.044***	0.038***
	(0.022)	(0.037)	(0.016)	(0.010)	(0.010)
Aged 30-49	-0.056***	-0.088***	-0.121***	-0.100***	-0.099***
	(0.020)	(0.033)	(0.014)	(0.008)	(0.009)
Aged above 50	-0.166***	-0.151***	-0.199***	-0.149***	-0.178***
	(0.023)	(0.040)	(0.019)	(0.013)	(0.011)
Woman	-0.048***	-0.066**	-0.097***	-0.049***	-0.020***
	(0.017)	(0.028)	(0.013)	(0.008)	(0.008)
Treated by private	0.069	0.039			
programm (Tprivate)	(0.058)	(0.073)			
Tprivate × College education	0.043	0.025			
	(0.050)	(0.071)			
Tprivate × High school graduate	0.071	0.055			
	(0.057)	(0.076)			
Tprivate × Aged 30-49	-0.153***	-0.020			
	(0.054)	(0.068)			
Tprivate × Aged above 50	-0.057	-0.058			
	(0.062)	(0.082)			
Tprivate × Woman	0.064	0.017			
	(0.043)	(0.058)			
Treated by public	0.169*		0.036	0.087**	0.084*
programm (Tpublic)	(0.099)		(0.051)	(0.038)	(0.047)
Tpublic × College education	0.003		0.048	0.002	-0.005
	(0.088)		(0.049)	(0.038)	(0.045)
Tpublic × High school graduate	0.079		-0.041	-0.038	0.017
	(0.100)		(0.053)	(0.044)	(0.049)
Tpublic × Aged 30-49	-0.148		0.021	0.011	-0.011
	(0.090)		(0.046)	(0.035)	(0.044)
Tpublic × Aged above 50	-0.044		0.013	-0.078	0.018
	(0.115)		(0.067)	(0.063)	(0.058)
Tpublic × Woman	-0.023		0.020	-0.006	-0.095**
	(0.076)		(0.042)	(0.034)	(0.037)
Observations	43,977	11,145	13,268	63,941	86,702

Estimates from separate 2SLS models with program participation instrumented by program assignment (and interactions). Each column is a different regression. Observations are weighted by the inverse of the assignment probability. *, **, ***: significance at 10%, 5% and 1% levels.
Reading: participation in the private program decreases the probability of exit to employment by 6.9 percentage points among reference individuals (young male high-school dropouts); the impact is 15.3 percentage points lower for those aged 30 to 49.
Sample: All areas, by program and sub-population.

Table 13: Private program and public program impact on exit to employment: inflow with UB, by areas, with controls

	Areas with both programs		Areas with one program	
	Private program	Public program	Private program	Public program
Within 3 months	0.018	0.104***	0.012	0.038**
	(0.017)	(0.032)	(0.023)	(0.018)
Within 6 months	0.039*	0.092**	0.053*	0.067***
	(0.021)	(0.037)	(0.029)	(0.020)
Within 9 months	0.053**	0.107***	0.061**	0.062***
	(0.023)	(0.037)	(0.031)	(0.020)
Within 12 months	0.051**	0.072*	0.049	0.047**
	(0.024)	(0.037)	(0.033)	(0.021)
Sample size	43,977	43,977	11,145	13,268

Estimates from separate 2SLS models with program participation instrumented by program assignment. Each cell displays the impact of intensive counseling on a given subgroup within x months of assignment, except for columns (1) and (2) that are estimated jointly. Controls for: education, age group, gender, marital status, citizenship, cause of job loss, number of children. Observations are weighted by the inverse of the assignment probability. Robust standard errors in parentheses. *, **, ***: significance at 10%, 5% and 1% levels.

Reading: participation in the private program increases exit to employment by 4.5 pp within 6 months, in areas where the public program is also implemented.

Sample: Inflow with UB only, distinguishing between areas with 1 and 2 programs.

Table 14: Private program and public program impact on exit to employment: inflow with UB, by areas, with area fixed effects

	Areas with both program		Areas with one program	
	Private program	Public program	Private program	Public program
Within 3 months	0.029*	0.124***	0.004	0.030
	(0.017)	(0.032)	(0.023)	(0.019)
Within 6 months	0.045**	0.114***	0.036	0.058***
	(0.022)	(0.037)	(0.029)	(0.021)
Within 9 months	0.060**	0.129***	0.042	0.052**
	(0.024)	(0.036)	(0.032)	(0.021)
Within 12 months	0.059**	0.096***	0.033	0.037*
	(0.025)	(0.037)	(0.033)	(0.021)
Sample size	43,977	43,977	11,145	13,268

Estimates from separate 2SLS models with program participation instrumented by program assignment. Each cell displays the impact of intensive counseling on a given subgroup within x months of assignment, except for columns (1) and (2) that are estimated jointly. Fixed effects for areas with same probabilities of assignment to treatment. Observations are weighted by the inverse of the assignment probability. Robust standard errors in parentheses. *, **, ***: significance at 10%, 5% and 1% levels.

Reading: participation in the private program increases exit to employment by 4.5 pp within 6 months, in areas where the public program is also implemented.

Sample: Inflow with UB only, distinguishing between areas with 1 and 2 programs.