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

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Abstract

This paper reports the results from a randomized experiment designed to evaluate the impact of reinforced job placement assistance on the labor market outcomes of young, educated job seekers in France. In order to identify both the direct (net job creation) and displacement ("queue jumping") effects, we use a two-step design. In the first step, the proportions of job seekers to be assigned to treatment (0%, 25%, 50%, 75% or 100%) were randomly drawn for each of the 235 labor markets (*e.g.* cities) participating in the experiment. Then, in each labor market, eligible job seekers were randomly assigned to the treatment, following this proportion. After eight months, eligible, unemployed youths who were assigned to the program were significantly more likely (2.4 percentage points, or 12%) to find a job with a contract lasting at least six months —the objective of the program— than those who were not assigned. However, among men (though not women), there was a negative impact of being in a treated labor market for eligible youth that were not assigned to the program. This effect was stronger for those who were *ex-ante* identified as seeking jobs in sectors in which a large proportion of other job seekers were eligible for the program. Furthermore, the employment effect of program assignment disappeared after 12 months and there was no impact on wages, suggesting that the gains for those who were treated may be due to short-term displacement effects rather than to significant improvements in the matching technology.

Keywords: job placement, counseling, displacement effects, randomized experiment

JEL: J68, J64, C93.

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1 Introduction

Job placement assistance programs are popular in many industrialized countries[1], and a growing literature attempts to assess their effects. In these programs, an intermediary, often a private firm or a non-profit organization, helps unemployed or underemployed workers write their résumés, find job interviews, and prepare for those interviews. Firms are usually paid in full only when the worker has found a job. Unlike other active labor market policies, which have effects that are globally found to be weak, most studies tend to find a significant and positive impact of this form of counseling, especially for job seekers with a low risk of long-duration unemployment (see reviews in Kluve, 2006; Card, Kluve, and Weber, 2010).

These studies are generally based on comparing the short-run labor market outcomes of counseled and non-counseled job seekers.[2] Experimental studies are still relatively rare, but they also tend to find positive impacts of counseling (Rosholm, 2008; Behaghel, Crépon, and Gurgand, 2010).[3] However, an important criticism leveled against these studies (whether or not they are experimental) is that they do not take into account potential displacement effects: job seekers who benefit from counseling may be more likely to get a job, but at the expense of other unemployed workers. This may be particularly true in the short run, when vacancies do not adjust: the unemployed who do not benefit from the program (the control group, in randomized experiments) could be partially crowded out.

Evaluating the magnitude of such displacement effects is a key policy issue. If all a policy does is to lead to a game of musical chairs among unemployed workers, then the calculated impacts will over-estimate welfare benefits for two reasons. First, the treatment effect will be biased upwards when we compare a treated worker to a non-treated worker in a given area due to the violation of the SUTVA assumption (the employment rate among workers in the control group is lower than it would have been absent the program). At the extreme we could (wrongly) deem a policy successful if it *only* negatively affected the control workers. Second,

[1] They are particularly developed in Northern Europe. For instance they represent 0.19%, 0.24%, 0.34% of GDP in Denmark, Sweden, and Germany, respectively (Source: OECD Labour Market Program database. In France, expenditures on employment placement services represent 0.25% of GDP.

[2] See Blasco and Rosholm (2010) for a paper on long-run outcomes.

[3] Van den Berg and Van der Klaauw (2006) find no impact in the Netherlands, but their intervention has more to do with monitoring than actual counseling.

the negative externalities themselves must also be taken into account when judging the overall welfare impacts of any policy. More generally, learning whether such externalities are present and how they vary based on the environment (for example, whether labor market conditions are strong or weak) can help shed light on how labor markets function.

This issue of displacement effects has long been a concern for economists and policy makers. For example, Johnson (1979), Atkinson (1987), and Meyer (1995) point out that displacement effects would alter the impacts of training or unemployment insurance policies. Heckman, Lochner, and Taber (1999) make a similar argument in the case of a tax and tuition subsidy policy by pointing out that the general equilibrium effects of such a policy would be less positive than the partial equilibrium effects, because the returns to education would be lowered by the policy. Based on a calibrated job search model, Davidson and Woodbury (1993) find that workers who receive a reemployment bonus displace ineligible workers. Improving on this approach, Lise, Seitz, and Smith (2004) develop a framework designed to evaluate the general equilibrium impact of the Canadian Self Sufficiency Program. They compare the results of a randomized experiment designed to evaluate partial equilibrium effects of this program with estimates from a structural model that takes general equilibrium effects into account and is calibrated using the same experimental data. They show that negative externalities may cancel out the partial equilibrium positive effects and could even lead to negative global effects. The authors do not directly measure these externalities, however. Close to our framework, Van der Linden (2005) derives equilibrium effects associated with counseling programs in a matching model of the labor market. Equilibrium effects in this model come from wage bargaining whereby counseling increases a job seeker's reservation wage, leading to a decrease in the demand for labor that partially offsets the initial increase in employment.

There have also been attempts to directly measure externalities. For example, Duflo and Saez (2003) evaluate the diffusion of information on a retirement savings program, with a two-step experimental design similar to the one we use here. Miguel and Kremer (2004) exploit the varying treatment density generated by a one-step experimental design to measure spillover effects of school-level deworming programs. Finkelstein (2007) exploits geographic variation in insurance coverage to compute the effects of Medicare on health consumption, and finds them to be six times larger than those suggested by the RAND health insurance experiments. There

are fewer studies focusing specifically on externalities in the labor market. In their evaluation of the British New Deal for Young Unemployed, Blundell, Dias, Meghir, and Van Reenen (2004) compare ineligible people in the areas affected by the program to those in areas not affected by the program. They do not find significant indirect effects on untreated youth of residing in treated areas. Ferracci, Jolivet, and van den Berg (2010) study how the effect of a training program for young employed workers in France varies with the fraction of treated workers, and do find that the effect diminishes. Pallais (2010) estimates the market equilibrium effect of a short-term employment opportunity given to workers in an online marketplace, and finds little evidence of displacement. In their very recent contribution, Gautier, Muller, van der Klaauw, Rosholm, and Svarer (2011) analyze the results from a Danish randomized experiment involving a job search assistance program organized in 2005 in two counties. Comparing control individuals in experimental counties to job seekers in some similar non-participating counties, they find hints of substantial negative treatment externalities.

One potential issue with most of these studies is that, even when the individual treatment is randomly assigned, or as good as randomly assigned, the number of people who are "treated" within a market is not itself randomly assigned. The comparison across markets may thus lead to biased estimates of the equilibrium effects. To address this issue, we implement a two-step randomized design in evaluating a large-scale job seeker assistance program in France that is targeted at young, educated job seekers. Under the program, private providers are contracted to provide reinforced placement services to young graduates (with at least a two-year college degree) who have been unemployed for at least six months.

The private provider is paid partially on delivery, *i.e.* conditional on the individual finding a job with a contract lasting for at least six months.

One of the main innovations of this experiment is the two-level randomization. In the first step, each of 235 local employment areas (located in ten regions of France, covering about half of the territory) was randomly assigned the proportion P of job seekers to be assigned to treatment: either 0%, 25%, 50%, 75% or 100%. The second level of randomization took place within each treated area: in each area, a fraction P of *all* the eligible job seekers (young, educated, unemployed or under-employed for six months or more) was randomly selected to be assigned to treatment. Those assigned to treatment were offered the opportunity to enroll in the

job placement program (about one-third of those assigned to treatment actually enrolled). For those who were assigned to the control group or refused the treatment, nothing changed: they continued to be followed by the counselors of ANPE (the national agency for the unemployed), and to receive the standard forms of assistance. This was a large-scale experiment: in total, we attempted to collect data on about 30,000 youths involved in the experiment, and we have complete data for 23,000 of them, about half of whom where indeed unemployed at the beginning of the experiment.[4]

This design allows us to test for externalities on untreated workers, by comparing untreated workers in areas with some treated workers to those in areas with no treated workers, and to investigate whether the effect of the treatment on the treated, and on the untreated, varies with the fraction assigned to treatment. A first comparison, between unemployed workers assigned to treatment and those who were not assigned to treatment (in control or treatment areas) suggests, consistent with the prior literature, that the program had positive impacts: after 8 months, unemployed workers assigned to treatment were 2.4 percentage points (12%) more likely to have a fixed-term contract with a length of more than six months.

Within treated areas, those assigned to treatment were 2.8 percentage points more likely to have such a contract than those who were not assigned. These differences are larger for men than for women, and larger when the private provider was a for-profit firm than when it was a not-for-profit organization.

At first glance, the externalities imposed on the unemployed, eligible youths who were not assigned to treatment appear to be small in the full sample: overall, these unassigned youths were no less likely to have a long-term contract in treatment areas than in control areas, and the effect of the treatment did not decline with the fraction assigned to treatment within the eligible pool. However, the pattern is different for men than for women. For men, there were large positive effects for those directly treated (the IV estimates suggest that those treated were 7.4 percentage points, or 43 percent, more likely to find a job than those in pure control labor markets), but there was also a negative effects on those in treatment areas who were

[4]The other half had either (1) found a job and the official records based upon which the eligibility list was constructed had not been updated, or (2) were underemployed, for example working part-time while seeking full-time employment. We focus on the unemployed for most of the paper, although for completeness we will also be presenting results for other workers.

not themselves assigned to treatment (-3.9 percentage points, or -23 percent). For women, the treatment effects are also significant and positive (though somewhat smaller), but we find no negative spillover on untreated, eligible women in treatment areas.

To the extent that the beneficiaries of the program took jobs that other workers (who were, for example, older, less educated, or unemployed for a shorter time) also competed for, the externalities may not have been limited to the eligible youths: in fact, they may have been smaller for eligible youths because they were distributed among a larger group of unemployed workers. To investigate this issue, we split the sample according to whether the type of job sought by an individual was one for which she had to compete with many other educated youths. Each job seeker must indicate the type of job she is looking for when she registers at the ANPE: she selects one or two out of 466 different jobs. We focus on the job that a job seeker selected as her first choice. For each of these jobs, we compute the share of educated youth among all young job seekers (using the universe of job seekers at the national level). We find that, for men, in occupations in which the fraction of educated youth among young job seekers is above the median (for example, for project managers, laboratory technicians, and financial officers), there is clear evidence of larger treatment externalities being imposed upon eligible individuals who were untreated: in those sectors, after 8 months, the unemployed who were eligible but were not assigned to treatment were 4.9 percentage points (25%) less likely to find a job with a long-term contract if they lived in an area where some eligible job seekers were treated. In contrast, in occupations in which the fraction of educated youth among young job seekers is below the median (for example, cooking assistants, construction vehicle drivers, and motorcycle mechanics), eligible individuals who were untreated were only 2.4 percentage points less likely to find a job in treated areas (the corresponding p-value is 0.30).

These estimates imply that, at least for men, the program's benefits would have been greatly overstated in a standard program evaluation with individuals randomly assigned within specific sites (for example, as in Dolton and O'Neill (1996), Van den Berg and Van der Klaauw (2006), etc.). Indeed, the within comparison suggests a treatment effect on the treated of 11.6 percentage points (and, by assumption, rules out negative externalities), while our estimates suggest a positive treatment effect of 7.4 percentage points on the treated, but a negative externality of 3.9 percentage points on all of those who were not treated (this includes those not assigned and

those who were assigned but did not take up the program). Furthermore, there is the additional negative externality imposed on those who were not eligible for the program. Taking account of all of theses impacts, the net number of jobs created by the program appears to be negligible compared to its cost.

These externalities suggest that part of the program effect in the short run was driven by treated workers taking jobs that would otherwise have gone to untreated workers, rather than by short-run labor demand and wage adjustments. Indeed, additional evidence suggests that the main effect of the program may have been to help those treated to find a job slightly faster, but at the expense of others who found one later: After 12 months (and up to 20 months), the program effects (both direct and indirect) on durable employment (or any form of employment) had entirely disappeared, both for men and for women. The program also had no impact on wages, suggesting that it did not lead to different types of job matches. We interpret these results in a context of a model of search proposed by Michaillat (2012) and developed by Landais, Michaillat, and Saez (2010) to analyze the design of unemployment insurance. This is a search model with two realistic features: diminishing returns, and a wage structure that does not fully adjust. The model has the additional prediction that externalities should be stronger given more slack labor market conditions (in a recession, for example). We test this additional prediction by comparing the size of the externalities imposed on cohorts affected by the 2008 Recession relative to unaffected cohorts, and by comparing externalities imposed in labor markets with higher unemployment rates to those imposed in better-performing labor markets. Indeed, we find that externalities are larger during the Recession and in labor markets in which unemployment levels are higher. In a difference-in-differences specification, we find the largest externalities occur in weak labor markets experiencing downturns. These findings provide empirical support for the model of the labor market we employ.

The job placement assistance program and the institutional context are described in the next section. Section 3 gives details regarding the experimental design and the data. Section 4 proposes a simple conceptual framework. Section 5 presents the empirical strategy. Section 6 discusses the results. Section 7 concludes.

2 Institutional Context and Description of the Program

2.1 Background: Placement services in France

Until 2005, the French public agency ANPE (*Agence Nationale Pour l'Emploi*) had, from a legal point of view, a monopoly on job placement services. In particular, employers were legally obligated to list their vacancies with ANPE.[5] In 2005, the Social Cohesion Law broke this virtual monopoly by permitting temporary work agencies to openly market their counseling and placement services to job seekers. The public operator (which was renamed *Pole Emploi* in 2008) has nevertheless remained an important agency because every unemployment insurance (UI) recipient must meet her ANPE caseworker at least once per month and follow her recomendations in order to remain eligible for benefits.

When a private job placement market did not spontaneously emerge, the government and unions decided to increase the number of partnerships between the public operator and private actors. Some specific types of job seekers were targeted, starting with those that the ANPE was known to have more difficulty assisting. The idea of forming partnerships was adapted from the German Hartz reforms (Jacobi and Kluve, 2007), in which each local employment office was required to contract with a "Personal Service Agentur" (PSA), often a temporary work agency. These PSAs were responsible for assisting a certain number of job seekers, and received a lump-sum payment for each job seeker that successfully found employment.

The French experience with private counseling has not been as widespread as the German one. In contrast to their German counterparts, however, French policy makers have set up randomized experiments to evaluate the effects of subcontracting placement services to private providers: one was dedicated to job seekers at risk of long-term unemployment (Behaghel, Crépon, and Gurgand, 2009); another to welfare beneficiaries (Crépon, Gurgand, Kamionka, and Lequien, 2011); and a third experiment was focused on young graduates who had been searching for a job for six months or more. This paper analyzes the third experiment.

French policy makers, like policy makers in the U.S., pay particular attention to the plight

[5]Some subpopulations of the unemployed were assisted by others agencies: for example, APEC (*Agence Pour l'Emploi des Cadres*) specialized in placement for executives and managers and *Missions Locales* assisted unskilled youth.

of recent college graduates, and the outlook for them has been bleak in recent years. In 2007, for instance, three years after one cohort of graduates had completed their studies, only 68%-75% had a stable job. Reports ((Hetzel, 2006)) emphasized the lack of job market experience among young university graduates (internships and summer jobs are rare), and recommended introducing specialized counseling services for these job seekers in order to improve their labor market outcomes. In 2007, the Ministry of Labor decided to experiment with subcontracting job placement services for young graduates who had been unemployed or underemployed for six months or more to private providers. Private providers, and temporary employment agencies in particular, were believed to be more efficient than the ANPE at finding jobs for young graduates as a result of private providers' experience in this particular segment of the market.

2.2 Program description

In each of the ten regions selected for the experiment, an invitation to tender was issued. Private operators were selected on the basis of the services they proposed to provide and the prices they charged. For-profit operators were selected in six regions, and five of the six operators were subsidiaries of temporary employment agencies. In four regions, not-for-profit organizations were selected. One not-for-profit is a social and solidarity-oriented training center, and the others were local agencies that are part of a larger not-for-profit youth guidance organization.[6]

The program can be broken down into two main phases:

- Phase I aims to find work for job seekers. For the first six months of the program, the private employment agency counsels the job seeker and helps her to find a durable job. The job must be on either a "CDI" (indefinite-term contract) or a "CDD" (fixed-term) contract lasting six months or more; CDD contracts are more common in practice.

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

[6]Regions in which for-profit operators were selected are: Haute-Normandie, Lorraine, Ile-de-France, Pays de la Loire, Picardie, Réunion. Regions in which not-for-profits were selected are: Centre, Nord-Pas-de-Calais, Provence-Alpes-Cote d'Azur, Rhône-Alpes.

The program also included an incentive scheme for the private job placement operators. Specifically, for each enrolled job seeker, the provider got paid in three stages, with each payment conditional on the fulfillment of a corresponding objective.

- Enrollment: when a job seeker is enrolled in the program, the private agency receives the first payment (25% of the maximum payment possible).
- Obtaining (and accepting) a durable job: when, within six months of entry into the program, a job seeker signs a contract for a job lasting more than six months, the second payment occurs (40%).
- Remaining employed after six months: six months after the job is found, the third payment is made to the counseling company if the former job seeker is still employed (35%).

The maximum possible payment ranged from 1600 to 2100 euros, depending on the firm's initial bid.

The program, like many others of this type, mainly focussed on helping beneficiaries to identify promising job prospects and prepare themselves for interviews. In general, program beneficiaries met job counselors more often. Beneficiaries received assistance in preparing their résumés, communicating with employers, and in assessing their own skills. Lastly, beneficiaries were somewhat more likely to be put in touch with a specific employer (although that difference is not significant).

3 Experimental Design and Data

3.1 Experimental design

As described in the introduction, the randomization took place at both the labor market and individual level.

The experiment took place in the areas covered by 235 public unemployment agencies, scattered across 10 administrative regions (about half of France). Each agency represents a small labor market, within which we may observe treatment externalities. On the other hand, the agencies cover areas that are sufficiently large, and workers in France are sufficiently immobile,

that we can assume that no spillovers take place across areas covered by different agencies. In any case, such migration would lead us to underestimate the magnitude of externalities.

In order to improve precision, we first formed groups of five agencies that covered areas similar in size and with comparable local populations: we obtained 47 such quintuplets. Within each quintuplet, each agency was randomly assigned a share of the population of eligible youths to treat: $P \in \{0, 0.25, 0.50, 0.75, 1\}$.

Job seekers were then randomized into treatment at the individual level, following the proportion drawn in the first stage for each area. Every month from September 2007 to November 2008, youth that had entered the target population (aged below 30, with at least a two-year college degree, and having spent either 12 out of the last 18 months or six months continuously unemployed or underemployed) were identified by each agency. The list of job seekers was then transmitted to us and we independently randomized the assigned proportion into treatment within each agency area. The list of individuals we selected to be potential beneficiaries of the program was then passed on to the contracted counseling firm in the area, which was in charge of contacting the youth and offering them entry into the job placement program. Entry was voluntary, and the youth could elect to continue receiving services from the local public unemployment agency instead. No youth from the control group could be approached by the firm at any time.

In total, we made treatment assignments for fourteen consecutive cohorts of youth, totaling slightly over 57,000 individuals. Approximately 10,000 of the youth lived in areas that were not part of the experiment, and are not included in this study. Out of the remainder, we only collected data on labor market outcomes for cohorts 3 to 12, who are therefore the focus of this paper.[7] In total, 30,343 individuals belonged to the experimental cohorts in regions covered by the study, and 29,636 of them were randomly selected to be surveyed.[8]

[7]Cohorts 1 and 2 were excluded because it took a couple of weeks before the private operators were ready to actually offer the treatment. Cohorts 13 and 14 were not followed because, at the same time they became eligible for the experiment, the Ministry issued a separate, profitable call for tender for job seeker counseling. Anecdotal evidence suggests that private firms were more focused on this second operation and stopped implementing the experimental program; indeed, youths from these cohorts were not enrolled even when they were officially selected for treatment.

[8]The sample size limit was dictated by a fixed budget.

3.2 Data

There are three sources of data for this experiment. First, we have administrative files provided by ANPE to the Ministry of Labor that list eligible job seekers. For each job seeker, these files provide the individual's age, number of months spent unemployed during the current unemployment spell, the public employment agency responsible for the individual, her postal address, and the type of job being sought. These files are not updated in real time, and so are not necessarily current.

A second dataset comes from private counseling firms' administrative files. In order to claim payment, these firms submitted lists of job seekers who actually entered the counseling scheme. Payment was conditional upon a job seeker filling out and signing a form, and copies of the form were reviewed to ensure that firms were not overstating the number of job seekers they were actually counseling. We use this dataset to measure program take-up.

Finally, our primary data source is a series of four endline surveys conducted by the Ministry of Labor. These surveys were necessary due to limitations with the existing administrative data. Administrative data obtained from the unemployment office does not provide a good measure of unemployment: all it records is whether a job seeker is still registered as an official job seeker. However, youth who stop being registered could either have become discouraged, or have found a job. In addition, young job seekers do not have strong incentives to be registered with the ANPE, in particular because they are often not eligible to receive unemployment benefits. Indeed, as we describe in more detail below, the baseline data that we used to select individuals to participate in the experiment proved to be quite inaccurate. Unfortunately, administrative data on employment and wages (from the tax authority or the social security administration) could not be linked to the experimental data for legal reasons related to data confidentiality protections.

The 29,636 job seekers sampled for the surveys were surveyed four times: 8 months, 12 months, 16 months and 20 months after random assignment. The survey was conduced by DARES, the research department at the Ministry of Labor, and was thus an official survey: answering was not mandatory, but response rates to surveys conducted by public agencies tend to be high in France. In order to limit data collection costs and to increase the response rate, the survey was short (10 minutes for the first wave, 5 minutes for the others). Moreover, the

survey combined three collection methods: internet, telephone, and paper questionnaires. As a result, response rates were high: as shown in table 1, 25,904 people (87%) responded to at least one of these surveys and 23,320 (79%) answered at least the first one.

Table 1 also shows the response rates conditional on having been assigned to either the treatment or control group. For each wave, the response rate is above 70%, and the job seekers assigned to treatment are only one percentage point more likely to answer than those assigned to control (this difference remains constant across waves).

The first survey wave took place between August 2008 and May 2009; the last survey wave took place between August 2009 and May 2010. The survey included questions about the respondents' employment situation at the time of the survey (wage, type of contract, part-time or not, occupation) and at the time of program assignment. The respondents' highest degree obtained, family situation (marital status, number of children), and nationality (or parents' nationality) were also obtained. We additionally asked how many times the respondent met a counselor (public, or from the contracted private agency) and what type of help she got during her job search. Finally, individuals assigned to treatment were asked the reasons why they thought they would benefit from entering the program (if they agreed to enter), while those who chose not to participate were asked the reason they did so.

Table 2 presents summary statistics for all surveyed job seekers. These data come from ANPE administrative files. The last two columns provide balancing tests of whether the randomization indeed produced comparable treatment and control samples. Column 4 presents the coefficient on treatment assignment from a regression in which the explained variable is the variable of interest and strata of employment agencies interacted with cohorts dummies are controlled for. At 5%, balancing tests reject that the column 4 coefficient is equal to zero only for gender of job seeker.

Most individuals in the sample are in their twenties. This is not surprising as one of the eligibility conditions was that an individual be aged less than 30. The median age is 26, and the distribution is skewed to the right. Another eligibility condition involved length of unemployment spell; to be eligible, individuals had to have been looking for a job for more than six months or to have been unemployed for more than 12 of the last 18 months. Indeed, individuals who have been unemployed for seven months or more are overrepresented in the sample. Note, however,

that only 9% of the sample has been unemployed for 18 months or more. Because these job seekers are young and have often only had jobs for limited lengths of time, most of them (69%) are not receiving unemployment benefits. Nearly two-thirds of job seekers are women. Finally, one-third of the sample has a vocational two-year college degree ("Bac+2"), and individuals with higher university degrees ("Bac+3" and more) represent another one-third of the sample. In contrast, individuals with degrees from engineering and business schools (which are mostly elite institutions) are scarce: they make less than 2% of the sample.

Table 3 presents summary statistics on employment status at the start of the experiment for those who responded to the first wave of the DARES survey. Importantly, 44% of the sample claimed to have been employed at the time of treatment assignment. There are two possible explanations for this. First, respondents could have recently found a job, and their status may not have been updated in the unemployment agency list that was used to generate the randomization sample. Second, respondents may have been underemployed, *i.e.* holding a part-time job but still looking for full-time employment, and so would have been eligible for treatment (this employment status is known as "activité réduite", or limited activity). In what follows, we will focus primarily on results for the unemployed job seekers because they are more likely to actually receive the treatment, and they are the target of the policy intervention. However, because a significant proportion of individuals claiming to be employed at the time of assignment were still treated (23%), we will also show the impact of the experiment on this subpopulation.

4 Conceptual Framework

A simple model of search with decreasing returns to scale and some rigidity in wages, which is a simplified version of Michaillat (2012) and Landais, Michaillat, and Saez (2010), helps clarify the conditions under which a job search assistance program like the one being studied might generate externalities.

We consider a model with one sector, and one type of worker (the model can easily be extended to include unskilled workers, to allow varying degrees of substitutability between skilled and unskilled workers, and to allow different types of workers to search either through the same

or through separate channels). Jobs end randomly at rate s. Individuals can be unemployed (U) or employed (L), so that the overall labor force is $\bar{L} = L + U$. The employment rate m is $L/\bar{L}$.

Unemployed people search for jobs. Denote total job search effort exercised by the unemployed as U_e; there are V vacancies. As is standard in job search models, denote $M(V, U_e)$ as the matching function, which links the number of matches to search effort, the number of vacancies, and $\theta = V/U_e$, the tightness of the labor market. The probability that a vacancy is filled is then $q(\theta) = M(1, 1/\theta)$, which is a decreasing function in θ. The probability of a match for a worker who exerts search effort 1 is $\theta q(\theta) = M(\theta, 1)$. We maintain the standard assumptions about the matching function, that it is a constant return to scale function that is increasing and concave in both V and U_e (Pissarides, 2000). The job finding probability is thus increasing and concave in θ.

To model the impact of the program, assume for simplicity that everyone exerts search effort 1.[9] When they become unemployed, a fraction of job seekers are assigned at rate π to benefit from reinforced counseling services, which increases the productivity of their search effort to $e > 1$.[10]

There are thus two type of unemployed job seekers: the counseled (indexed by c) and those who are not counseled (indexed by n). These two groups have different exit rates x_c and x_n. We have the following flow equations:

$$x_c U_c = \pi s L$$

$$x_n U_n = (1 - \pi) s L$$

The overall search effort in each group is:

$$U_{ec} = e U_c$$

$$U_{en} = U_n,$$

[9] Search effort can be endogenized –this is done in Landais, Michaillat, and Saez (2010), leading to the same results for our purpose.

[10] The assignment is a function of: (1) eligibility, (2) random assignment, and (3) whether or not an individual chooses to participate.

where U_c (U_n) is the number of unemployed who are counseled (not counseled). Total search effort is $U_e = eU_c + U_n$.

Exit rates are derived from the matching functions: counseled individuals account for a share eU_c/U_e of the search effort, so they receive $eU_cM(V, U_e)/U_e = eU_c\theta q(\theta)$ job offers, which leads to the following exit rate for counseled individuals:

$$x_c = e\theta q(\theta)$$

Similarly, the exit rate for non-counseled individuals is:

$$x_n = \theta q(\theta)$$

These equations make clear that displacement effects will be observed if reinforced counseling services lead to a reduction in the tightness of the labor market. We now examine the conditions under which the reinforced counseling program leads to a change in θ.

Writing $\overline{L} - L = U = U_c + U_n$, we can derive the labor supply curve as a mapping between θ and $m = \frac{L}{\overline{L}}$:

$$m = \frac{\theta q(\theta)}{s\left(\pi/e + 1 - \pi\right) + \theta q(\theta)} \tag{1}$$

The resulting, $\theta = \theta_B(m)$ is an increasing function of m. Figure 1 draws the labor supply curve in the tightness/employment rate space. This is the equivalent of the Beveridge curve (conventionally represented in the unemployment-vacancy space). Note that the curve is fairly flat for low levels of employment (low θ) and steep when employment is high: since the function $\theta q(\theta) = M(\theta, 1)$ is concave due to the (standard) constant returns to scale assumption for the matching function, the function $\theta_B(m)$ is convex.

To obtain the labor market equilibrium, we now consider the firm's decision. Suppose that the production technology exhibits decreasing return to scale. This can be justified by some factor (management, fixed capital, etc.) being fixed in the short run. Then, we have:

$$Y = AL^{\beta}$$

To simplify the argument, assume that the total operating cost for a job is c (for example, because all entry-level workers are paid a binding minimum or negotiated wage).[11] The firm

[11]This is a stark simplification of the Michaillat (2012) and Landais, Michaillat, and Saez (2010) framework:

chooses employment to maximize the value of output, minus operating and hiring costs. Let γ be the per period cost of an unfilled vacancy, and r the interest rate. Using Bellman equations for the value of having a vacancy and a filled job we can derive the following labor demand equation:[12]

$$\beta A L^{\beta-1} - c - \gamma \frac{r+s}{q(\theta)} = 0 \tag{2}$$

Frictions in the labor market can be interpreted as a marginal cost of hiring $\gamma(r+s)/q(\theta)$. This labor demand equation leads to a decreasing relationship between the employment rate and θ: $\theta = \theta_d(m)$. The two equations 1 and 2 together lead to the equilibrium values of θ and m.

The effect of the policy is illustrated in figure 1, panel A. The policy leads to a decrease in $\pi/e + 1 - \pi$. Thus, the Beveridge curve shifts to the right while the labor demand curve remains unchanged. Clearly, this leads to an increase in employment and an increase in θ, and so there will be displacement effects. Notice that if we had a constant return to scale production function instead, then the labor demand equation would be flat, and the shift in the Beveridge curve would therefore not lead to any displacement effects. Likewise, if wages were fully flexible (and determined only by the marginal productivity of labor), the labor demand curve would also be flat. Conversely, in a pure rat-race model, the labor demand curve would be vertical, and there would be no employment effect of a job placement policy. The gains accruing to beneficiaries would be entirely undone by losses experienced by non-beneficiaries. In the notation used by Landais, Michaillat, and Saez (2010), the size of the externality can be illustrated by the difference between the "micro" elasticity of employment with respect to the shift in the Beveridge curve (E_m on the graph), which is the effect on one individual and does not take into account the slope of the demand curve, and the "macro" elasticity (E_M), which represents the net increase in employment.

Thus, the model predicts that there will be direct employment effects for the beneficiaries,

they show that the results below hold as long as there is some rigidity in salary (for example, downward wage rigidity), rather than requiring that wages be fully fixed.

[12]This equation is derived from: (1) the Bellman equations for the the value of having a vacancy J_V and a filled job J_E ($rJ_V = -\gamma + q(\theta)(J_E - J_V)$ and $rJ_E = p - c + s(J_V - J_E)$, where $p = \beta A L^{\beta-1}$ is the marginal product related to a new hire); and (2) from the entry condition requiring that the value of having a vacancy is zero.

but also externalities imposed on the non-beneficiaries, as long as the labor demand curve is not completely flat (which will be the case as soon as there is a limiting factor, such as capital or management). The magnitude of externalities will depend on π, the fraction of workers that are affected by the policy (which will determine the size of the shift in the labor supply curve). In addition, the magnitude of the externality will be increasing in the steepness of the labor demand curve. The model has an additional testable prediction based on the shape of the labor supply curve. This prediction is explored in detail (and proven) in Landais, Michaillat, and Saez (2010) and forms the core of the authors' argument that unemployment insurance should be higher during recessions. This prediction is illustrated in figure 1, panel B. If labor demand is low (left part of the graph), a shift in the labor supply curve will lead to a large gap between the micro and the macro elasticity (i.e. a large externality) since the labor supply curve in this space is almost flat. Employment in this part of the graph is mainly constrained by demand, not by search productivity, so that increasing the productivity of search has very little impact on total employment: the main benefit for the treated workers is that they move ahead in the rat race. If demand is high (right part of the graph), an increase in search productivity has much larger net employment effects (and smaller associated externalities).

5 Empirical Strategy

5.1 Naive estimates: Differences between beneficiaries and non-beneficiaries

A first step is to establish whether or not the program improved the probability that its intended beneficiaries found a job, relative to the probability that non-beneficiaries did so. These estimates are "naive" estimates of the impact of the program because they ignore externalities. Nonetheless, they can give us a sense of whether the program had any effect, and reveal what a traditional evaluation of the program would have concluded.

The randomization took place in two steps, both within strata (area quintuplet), and within each city or employment zone corresponding to one of the 235 public unemployment agencies. A first possible analysis ignores this design and treats all the treatment and control group members symmetrically, regardless of the area from which they were drawn. Hence, we are interested in:

$$y_{ic} = \alpha_1 + \beta_1 T_{ic} + X_{ic}\gamma_1 + \epsilon_{ic} \tag{3}$$

y_{ic} is a labor market outcome for individual i in city c. For this basic analysis, we focus on the main objective of the program, *e.g.* obtaining durable employment. To shed light on the nature of the program, we will also estimate the above equation with measures of the services received by youth while unemployed as the dependent variable. T_{ic} is a dummy equal to 1 if the individual enrolls into the program. X_{ic} is a vector of control variables which includes a set of quintuplet dummies, a dummy for date of entry into the program, and individual-level control variables (age, gender, and education). Standard errors are clustered at the labor market level and robust to heteroskedasticity.

Entry into the treatment is endogenous, since treatment was voluntary, so treatment is instrumented with a dummy $Z = 1$ if the individual was assigned to the treatment group, 0 otherwise.

The reduced form is thus:

$$y_{ic} = \alpha_2 + \beta_2 Z_{ic} + X_{ic}\gamma_2 + \upsilon_{ic} \tag{4}$$

And the first stage:

$$T_{ic} = \pi_1 + \pi_2 Z_{ic} + X_{ic}\pi_3 + \omega_{ic} \tag{5}$$

A standard active labor market policy experiment, with randomization occurring only within sites and with a sample including only a small number of sites selected for convenience, would typically not include labor markets with all treatment or all control individuals. In such an analysis, each city is a strata, and the strata would be controlled for in the equation of interest:[13]

$$y_{ic} = \alpha_3 + \beta_3 T_{ic} + d_c + X_{ic}\gamma_3 + \epsilon_{ic} \tag{6}$$

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

Comparing the results of estimating equations (3) and (6) can give us a first indication of the importance of externalities: In the absence of externalities, we will not be able to reject the equality of β_1 and β_3, although β_1 will be more precisely estimated (since the 100% sites and 0%

[13]For instance, the experiment in Rosholm (2008) takes place in only two counties in Denmark.

sites do not contribute to the estimation of β_3). However, with negative externalities imposed on the control group in treated areas, one would expect to find $\beta_1 < \beta_3$, since the difference between workers *within* labor markets should be larger than the difference between workers in entirely treated areas and workers in entirely untreated areas.

5.2 Estimating externalities

5.2.1 Unconstrained reduced form

To estimate externalities, we take advantage of the fact that the fraction of treatment job seekers varies by labor market (from 0% to 100%). Negative externalities have two simple implications. First, the probability that eligible youth in the control group find a job should be lower in cities where others were assigned to treatment. Second, the treatment effect should fall as the fraction of workers assigned to the program rises (as the treated workers now compete among themselves for jobs). We estimate a fully unconstrained reduced form model, and test whether the effect of being assigned to treatment or to control varies by assignment probability. The specification we consider is the following:

$$\begin{aligned} y_{ic} &= \beta_{25} Z_{ic} P_{25c} + \beta_{50} Z_{ic} P_{50c} + \beta_{75} Z_{ic} P_{75c} + \beta_{100} Z_{ic} P_{100_c} \\ &+ \delta_{25} (1 - Z_{ic}) P_{25c} + \delta_{50} (1 - Z_{ic}) P_{50c} + \delta_{75} (1 - Z_{ic}) P_{75c} \\ &+ X_{ic}\gamma_4 + u_{ic} \end{aligned} \tag{7}$$

where Z_{ic} is the assignment to treatment variable and P_{xc} is a dummy variable at the area level indicating an assignment rate of x%. ZP_{25} is thus a dummy for being assigned to treatment in a labor market with a rate of 25% assignment. As before, control variables are individual characteristics (gender, education, etc.) and the set of 47 dummy variables for city quintuplets (our randomization strata). Standard errors account for within area correlations between residuals and are robust to heteroskedasticity. The parameter β_x measures the effect of being assigned to treatment in an area where x% of the eligible population was assigned to treatment, compared to being in the "super" control group in which no one was assigned to treatment. Coefficient δ_x measures the effect of being assigned to the control group in an area where x% of the eligible population was assigned to treatment, compared to being in the "super" control group in which

no one was assigned to treatment. Note that there are four parameters β but only three parameters δ as there is no room to estimate the effect on those assigned to the control group when the whole eligible population is assigned to receive the treatment.

There are three tests that can be used to investigate the presence of externalities based on estimates from the regression above: (1) whether all the δ coefficients are jointly zero; (2) whether they are equal to each other; and (3) whether the β coefficients are equal to each other.

5.2.2 Pooled reduced form and interaction with sector

A simpler regression, which just exploits the presence of the "super" control (with zero probability of treatment assignment), pools all those who were assigned to control in an area in which some were treated, and pools all those who were assigned to treatment. This regression does not allow us to estimate the slope of program effects with respect to the share treated, but has more power against the null that there are no externalities.

The reduced form specification is:

$$y_{ic} = \alpha_5 + \beta_5 Z_{ic} P_c + \delta_5 P_c + X_{ic}\gamma_5 + \omega_{ic} \tag{8}$$

where P_c is a dummy for being in any treatment area (*i.e.* an area with positive share treated). In this specification, β_5 is the difference between those assigned to treatment (whether treated or not), and those who are in treatment zones but are not themselves assigned to treatment. δ_5 is the effect of being untreated in a treated zone (compared to being untreated in an untreated zone). The sum $\beta_5 + \delta_5$ is the effect of being assigned to treatment (compared to being in an entirely unaffected labor market).

We also estimate a corresponding IV specification, where program participation (T_{ic}) is instrumented by assignment to the program:

$$y_{ic} = \alpha_6 + \beta_6 T_{ic} P_c + \delta_6 P_c + X\gamma_6 + \nu_{ic} \tag{9}$$

Under the assumption that the externality imposed by a treated worker is the same on all others, regardless of their assignment status, this specification gives us: the average effect of the treatment on the treated compared to others untreated in the treatment zone (β_6), and the effect of being untreated in a treatment zone (δ_6) (see appendix A for a formal derivation).

Under the same assumptions, we can also directly estimate the overall effect of the treatment on the treated, compared to those in the "super" control group, as $\beta_7 = \beta_6 + \delta_6$, by estimating the following IV equation using treatment assignment as an instrument:

$$y_{ic} = \alpha_7 + \beta_7 T_{ic} P_c + \delta_7 P_c(1 - T_{ic}) + X\gamma_7 + \xi_{ic} \tag{10}$$

The model suggests that the size of the externality imposed on any given worker will depend on the fraction of workers in the market that are not assigned to treatment. This fraction unassigned depends on both the fraction of those who are eligible and are assigned, and on the fraction of the overall labor market that is eligible. Indeed, any labor market externalities due to the treatment may affect not only the eligible group, but may also affect workers who are close substitutes for them, although they are not themselves part of the experiment. For example, young, educated individuals who have been unemployed for at least 6 months may be competing for jobs with all young job seekers, or with young, educated job seekers with a slightly shorter duration of unemployment. If this is the case, our experiment may not have the power to capture any externalities imposed on other eligible workers because they represent too small a share of the real pool of workers against whom the treated workers are competing. For instance, our target population represents 25% of all young workers (under 30) who have been unemployed for more than six months (the target population represents 21% of such men and 28% of such women); similarly, the target population represents about 40% of all unemployed, young graduates (the proportion is similar for men and women). Unfortunately, looking for externalities among the rest of the unemployed population would not necessarily be fruitful: the power of the experiment would simply be too low to detect anything if the eligible workers represent only a small share of the overall pool of job seekers.

To investigate the displacement issue, we split the sample according to the probability that eligible workers from this experiment are directly competing with each other. To do so, we focus on education requirements. When they first register at the ANPE, job seekers indicate the category of job they are looking for. There are 466 such categories. Using a nationwide database of job seekers, we compute, for each job, the share of skilled job seekers (with at least 2-year college degrees) among all job seekers under 30 years of age and who have been

unemployed for more than six months. We call this fraction κ. Table 4 lists the 10 categories in which the share of skilled workers is highest (high κ) and the 10 categories in which the share of skilled workers is lowest (low κ), along with the corresponding shares. Low values of κ are found for workers in industrial jobs that require vocational education (often below the college level), such as construction workers (roofers, concrete workers, and sheet fitters). The highest concentration of young job seekers with at least two-year college degrees is found for workers in tertiary occupations, such as lawyers, financial officers, lecturers and psychologists.

In the categories in which the educated are least represented, job seekers are competing with a larger pool of workers (all unskilled, young workers are competing with them). Therefore, in the presence of externalities, we would expect a larger negative effect on the untreated in the job categories with a high share of skilled workers than in those categories with a low share of skilled workers.

We re-estimate equations 8 and 9 separately for eligible job-seekers looking for jobs where κ is below the median, above the median, and in the top quartile. The value of the median and the top quartile are defined with respect to the distribution of κ in our sample of workers. For the median worker in the sample, κ is 54%, and the top quartile cutoff for κ is 80%. Our target workers represent 10% of the young job seekers with past unemployment duration greater then six months in below median κ sectors, 72% in above median κ sectors, and 76% above third quartile.

Finally, the model has the testable implication that externalities should be larger in recessions or in weak labor markets. To test this hypothesis, we run three additional regressions. First, we run:

$$y_{ic} = \alpha_7 + \beta_7^L (T_{ic} P_c * LLD_i) + \beta_7^H (T_{ic} P_c * HLD_i) + \delta_7^L P_c LLD_i + \delta_7^H P_c * HLD_i + X_{ic}\gamma_7 + \nu_{ic} \quad (11)$$

where LLD_i is a dummy equal to 1 for cohorts that face a very weak job market (we assign a value of 1 to the cohorts that became unemployed between October 2007 and July 2008, and hence were looking for employment during the worst of the crisis). HLD_i is a dummy equal to 1 for all other cohorts. Second, we estimate:

$$y_{ic} = \alpha_8 + \beta_8^L (T_{ic} P_c * LLD_c) + \beta_8^H (T_{ic} P_c * HLD_c) + \delta_8^L P_c LLD_c + \delta_8^H P_c * HLD_c + X_{ic}\gamma_8 + \nu_{ic} \quad (12)$$

where LLD_c is a dummy equal to 1 for regions with unemployment rates above the median. HLD_c is a dummy equal to 1 for regions with unemployment rates below the median. Recall that X_{ic} includes a cohort dummies, and therefore the main effect of the labor market conditions is controlled for.

Externalities may vary across cohorts or regions for reasons that are not directly linked to labor market conditions. For example, the effectiveness of the program or the intensity of search efforts may have changed over time (as operators became better at assisting job search, or, on the contrary, lost interest). Alternatively, operators who bid in weak labor markets may be different than those who bid in strong labor markets. The strongest test will thus come from a "difference-in-differences" specification, in which the externality term is interacted with the fully interacted set of weak/strong labor market cohort dummies and weak/strong labor market region dummies:

$$\begin{aligned} y_{ic} = \alpha_9 &+ \beta_9^{LL}(T_{ic}P_c * LLD_i * LLD_c) + \beta_9^{HL}(T_{ic}P_c * HLD_i * LLD_c) \\ &+ \beta_9^{LH}(T_{ic}P_c * LLD_i * HLD_c) + \beta_9^{HH}(T_{ic}P_c * HLD_i * HLD_c) \\ &+ \delta_9^{LL}(P_c * LLD_i * LLD_c) + \delta_9^{HL}(P_c * HLD_i * LLD_c) \\ &+ \delta_9^{LH}(P_c * LLD_i * HLD_c) + \delta_9^{HH}(P_c * HLD_i * HLD_c) + X\gamma_9 + \nu_{ic} \end{aligned} \tag{13}$$

In this specification, the testable implication of the theory is that δ_9^{LL} is significantly different (more negative) than all of the other δ coefficients. The identification assumption is that, to the extent there are difference in externalities across regions, they would not be different in recession and in other periods.

6 Results

6.1 Results: First stage (take-up) and program activities

Panel A in table 5 presents the impact of assignment to treatment on program participation. Not surprisingly, participation in the control group was essentially zero, but take-up in the treatment group was far from universal: it was only 35% for the full sample of workers assigned to treatment. Take up was higher for unemployed workers (44%) than for employed workers (25%). The follow-up survey asked why respondents did not participate (if they did not). 46%

of those assigned to treatment who did not participate reported that they already had or were about to start a job and 11% claimed that they were studying. Only about 17% of respondents answered that they felt that the counseling program was useless or time-consuming.

Appendix tables A.1 and A.2 present the characteristics that predict take-up, based on a probit regression. Some individual characteristics are strongly correlated with take-up: gender –males were more likely to participate–, education –those with higher educational attainment were less likely to participate–, and access to unemployment benefits –those receiving unemployment benefits were more likely to participate. The participation rate was very stable across cohorts, and was similar in areas with varying shares of the eligible population assigned to the treatment.

Panels B and C in table 5 present coefficients β_1 and β_2 for a number of intermediate outcomes, indicating the types of services received by job seekers (according to their self-reports from the endline interview). Overall, as we can see in panel C, participants had 1.6 (50%) more meetings with a job search advisor (over the 8 months after assignment), and received more help preparing their résumés and assessing their skills. Participants were not significantly more likely to have been put in touch with a specific employer, nor did they receive help with transportation to interviews. Overall, the program may have helped participants form more realistic expectations of the jobs they could attain, and helped them navigate the job-hunting process, potentially improving the effectiveness of their search effort.

6.2 Preliminary results: Labor market outcomes

The results of estimating equations 3 and 4, with obtainment of a fixed-term contract with a length of at least six months as the outcome, are presented in table 6.

Panel A1 shows the reduced form "impact", ignoring externalities: all those assigned to treatment are pooled and compared to those assigned to the control group. Panel A2 includes labor market (city) dummies, and thus compares treated and control job seekers within given labor markets. A test of equality examines the hypothesis that the coefficients in panels A1 and A2 are equal.

Overall, job seekers assigned to treatment are one percentage point more likely to have obtained a fixed-term contract with a length of at least six months. The impact is entirely

driven by those who were unemployed at the time of assignment: they were 2.4 percentage point (12%) more likely to find a durable job if they were assigned to treatment than if they were not.

Recall that, depending on the region, the contracts were awarded either to for profit or non-profit operators. The for-profit operators drive the observed difference, and the effect in regions with for-profit operators is about twice that in the sample as a whole: 2.3 percentage points overall in the for-profit subsample, and 4.6 percentage points (22%) for the unemployed subsample in for-profit areas. The differences are systematically larger for men than for women (for example, we find a 7.1 percentage point –or 41%– impact for unemployed men in for-profit regions, versus a 3.5 percentage point impact for unemployed women). As we see in panel B, the differences we estimate translate into relatively large IV estimates: a 5.5 percentage point impact of participation for unemployed workers, a 7.4 percentage point impact for unemployed men, and up to an 19 percentage point (108%) impact for unemployed men in for-profit regions.

As we noted, these estimates are potentially biased estimates of the true effects of the program on participants in the presence of externalities. The comparison between panel A1 and A2 (and B1 and B2) provides a first indication of the presence of externalities. If externalities are important, one would expect the estimates in A2 to be larger than the estimates in A1. In practice, while the estimates in A2 are less precise, they are never significantly different than those in A1. However, for unemployed men the estimated "effects" are about 60% higher when controlling for LEA dummies: this could be an indication that a more precise test will detect significant externalities.

6.3 Externalities

6.3.1 Full sample: Effect on the probability of finding a fixed-term contract with a length of more than six months

Table 7 presents estimates of equation 7. Figure 2 is a graphical representation of the coefficients for unemployed workers (men and women in all areas, and just in areas with for-profit operators; and men only, in all areas, and just in areas with for-profit operators). For all workers (or all unemployed workers pooled together), there is no strong evidence of externalities. We can never reject the hypothesis that all δ coefficients (the coefficients of being in a treated zone for the untreated) are jointly zero. The δ coefficients are also not statistically different from each other,

and there is no evidence that they are decreasing in treatment share, even for subgroups in which the direct effects are large (see figure 2, panel A, for example). Furthermore, the effect of treatment assignment does not appear to decline with the fraction assigned to treatment. If anything, the treatment effect follows a U-shape in assignment rate, with individuals in areas with a 100% assignment rate tending to have the largest treatment effect (see figure 2). These findings mask a difference between men and women, however. For men, the δ coefficients are all negative and jointly significant (two out of three are also individually significant), although they are not increasing in magnitude with the fraction assigned to treatment.

Table 8 presents the estimates of equation (8), which summarizes the previous results by estimating an average externality across areas with varying fractions of job seekers assigned to treatment. In each panel, the first row presents coefficient β_5 (the impact of being assigned to treatment compared to not being assigned to treatment in a treatment zone), and the second row presents coefficient δ_5 (the effect for the untreated of being in a treatment zone). The third row is the sum of the first two, and is can be interpreted as the net effect of being assigned to treatment, compared to being in an untreated zone.

Columns 1, 5 and 9 present the results for the full sample, for all men, and for all women, respectively. Not surprisingly, the results are consistent with those in table 7: there is little evidence of any negative effect on the untreated in the sample as a whole. For men, however, there appears to be a negative externality. Those assigned to treatment are 5.1 percentage points more likely to find a job with a durable contract than those who are not assigned to treatment and live in treated areas, but the unassigned in treatment zones are 3.9 percentage point *less* likely to find a job with such a contract than those in untreated areas. Overall, men assigned to treatment are actually not significantly more likely to find a job than those in the "super" control group. Interestingly, since there are no negative effects for women of being in a treatment zone, the net effect for a woman of being assigned to the program (compared to someone in the "super" control group) is actually a 2.4 percentage point increase in the likelihood of finding a job with a durable contract, twice the difference for men.

Table 9 presents the estimates of the parameters of interest from the IV specification (equations 9 and 10). In each panel, the first row is the estimate of the difference in the outcome between treated and untreated workers within the same zone (β_6), the second row is the effect

of being untreated in a treated labor market (δ_6) and the third row is the difference between a treated worker and a worker in an untreated zone (β_7). These results are in line with our reduced from estimates of externalities.

Among men, we find a negative treatment externality, and a large difference between the treated and the untreated in the treatment zones (11 percentage points, which implies that the treated are almost twice as likely to find a long-term job in these markets). The net effect of being treated on the probability of signing a fixed-term contract with a length of more than six months is 7.4 percentage points (43%) for men. This table reveals that the lack of a significant reduced form treatment effect for men is due to the fact that the assigned, but untreated, were hurt by the negative externalities imposed by the treated. For women, there is a smaller difference between the treated and untreated in treatment zones, but no effect of being untreated in a treatment zone (relative to being in the "super" control group). The net effect of treatment is 4.5 percentage points (19%).

6.3.2 Splitting the sample by job type

In tables 8 and 9, columns (2) to (4), (6) to (8) and (10) to (12) split the sample according to κ, the fraction of educated workers among young job seekers in the job category in which the respondent is searching for work. The results confirm the conclusion that externalities appear to be important for men, but not for women. For men, while the effect of being untreated in a treatment zone is -3.9 percentage points for the sample as a whole, it is -4.9 percentage points (26%) for job categories with κ above the median, and -10.6 percentage points (50%) for job categories with κ above the third quartile. Although differences are not very significant, these results suggest that, in sectors in which men are competing mainly with other men in the sample, the externalities are particularly large.

In sectors with lower κ, they *appear* to be smaller, but this is because they are diluted over a larger group competing for the same type of jobs. For women, in contrast, there are no significant negative effects on the untreated of being in a treated zone, even in employment sectors with high values of κ.[14]

This table suggests that, as expected, externalities depend on the effective fraction of workers

[14]Defining κ separately for women and for men produces very similar results.

in the relevant labor market that is treated (this is what we predicted based on our model, but not what we found in table 7). A possible explanation for the lack of impact of the fraction of workers assigned to treatment is a lack of power: assuming that unskilled and skilled workers are substitutes within a particular occupation, the effective fraction assigned to treatment is $\kappa * P * r$, where r is the fraction of the population assigned to treatment that is actually treated. For unemployed men, the take-up rate was 45%, and the median κ is 54%. Thus, increasing the fraction assigned to treatment from 25% to 75% only increased the effective fraction treated in this "market" from 7% to 18%. In contrast, κ is on average equal to 10% below median and to 76% above the third quartile. Thus, for the average treatment assignment of 50%, going from κ below median to κ in the third quartile increases the the fraction treated from 2% to 18%.

Appendix table A.3 present parallel results, focusing on regions where contracts were awarded to for-profit operators (recall that the effects were larger in these regions). The main conclusions are similar, but both the direct effects and the externalities (for men) are stronger (all the point estimates are about twice the size of those found for the sample as a whole). In practice, the program's effects were entirely driven by for-profit operators, with the not-for-profit operators proving essentially ineffective (generating no direct effects and no externalities). The conclusion that there are no negative externalities of being in a treatment zone for women, and the finding of smaller treatment effects for women than for men, remain unchanged.

6.4 Impact of labor market conditions on externalities

Finally, table 10 presents the test of the Landais, Michaillat, and Saez (2010) prediction that the externalities should be larger when labor market conditions are weak. In panel A, we compare cohorts affected by the 2008 Recession to other cohorts. Consistent with the model, we find larger treatment effects, and much larger externalities, in the cohorts most affected by the crisis (in fact, the externalities are negative only for these cohorts). Note that in the affected cohorts, there are negative externalities even for women. In panel B, we compare labor markets with high versus low unemployment rates. The pattern is the same, with statistically significant externalities only in weak labor markets (although the difference in externalities between weak and strong markets is not always significant). Finally, panel C presents results from the difference-in-differences specification: it shows that externalities are indeed the largest

for cohorts from markets with high unemployment rates who entered the experiment during the 2008 Recession, suggesting that differences in patterns of externalities are not likely driven by cohort effects or local characteristics that are unrelated to labor market conditions.

6.5 Other employment outcomes

Until now, we have focused on the main outcome of interest for the program, the type of employment for which firms were rewarded: fixed term contracts for more than 6 months.[15] Fixed-term contract obtainment is not the only variable of interest, however. In fact, two variables may be more directly relevant from a policy point of view. First, did the program increase the probability that individuals obtained any long-term job (with either a fixed-term contract lasting for at least six months or with an indefinite-term contract), or just jobs with fixed-term contracts? Second, did the program increase the probability that participants found any type of job, or did it just increase the stability of the jobs people found?

Panels B and C of tables 8 and 9 present the reduced form and IV estimates of assignment (or treatment) effects and the associated externalities. For all long-term employment (panel B), pooling all workers together, the point estimates are a bit smaller than those for the fixed-term contract outcome (though in the same range): 4.2 percentage points for all workers, with similar effects for men (4.5 percentage points) and women (4.4 percentage points). This point estimate is entirely due to the increase in the number of fixed terms contract (there was no increase in the number of indefinite term contract)[16] The evidence on externalities is consistent with the results from specifications with the fixed-term contract outcome. The point estimates on the dummy for being untreated in a treatment zone are almost the same, although noisier. As before, we do find much higher direct treatment effects for workers seeking jobs with low κ.

For employment of any kind (panel C), the point estimates are somewhat smaller, and usually insignificant (except in low κ sectors), in part because the probability of finding a job is high in the control group (around 63% of workers who were assigned to the control group and

[15] Agencies were paid the same amount for a fixed term contract lasting 6 months or more, or an indefinite term contract, as long as the employment spell was indeed more than 6 months, but indefinite term contract are harder to obtain, and profit maximizing firms would have focused on fixed term contracts.

[16] Results for indefinite term contracts are omitted and available upon request, but they immediately follow from panels A and B taken together.

were initially unemployed have found a job 8 months later). The contrast between the effects estimated when we look at fixed-term contracts versus any type of employment is even starker within areas with for-profit operators (see appendix tables A.3 and A.4): in these areas there are, as we saw, large treatment effects when we look at fixed-term contracts with a length of more than six months (and durable employment), but the effects on any employment are small and insignificant. This suggests that operators were effective at generating exactly the type of employment relationships that would ensure they maximized the payment they received, and essentially converted short-term jobs into slightly longer-term jobs.

6.6 Long-term effects: employment and wages

Another important measure of the program impact is whether any effect persists after eight months. Job placement agencies were contractually incentivized to help beneficiaries find jobs within a maximum of six months, so the direct effects of the program are expected to disappear after 12 months. However, a key rationale for such job placement policies is the idea that a young person's first job serves as a "stepping stone," helping her to find subsequent employment after her first contract ends (or to move from having a six month contract with a firm to a more permanent position). To investigate the persistence of program impacts, we conducted surveys at 12, 16 and 20 months after treatment assignment. Table 11 provides the IV results.[17]

We focus on the "durable employment" outcome in this table but the results on fixed-term employment with a length of more than six months and any employment are the same (see appendix): at 12 months and beyond, there is no difference left whatsoever between the treatment and the control groups ("super" control or unassigned in treatment areas). The probability of finding a durable job steadily increases in the control group (from 48% at 8 months to 62% at 20 months), and the treatment and control groups converge.

Tables 12 and 13 present the impacts of the program on wages for those employed. This is a selected outcome, since there is an employment effect. Appendix tables A.5 and A.6 present the impact on total earnings (including zero values for those who earn nothing, and unemployment benefits for those who receive them), at eight months and beyond. The effects on wages could

[17]For the sake of concision, the reduced form results have been omitted, but are available from the authors upon request.

theoretically have been positive or negative, in the short- or long-run. The impact could have been negative if treated workers were encouraged to quickly accept low-quality jobs, rather than wait for something better. Alternatively, the effect could have been positive if programs helped individuals to find better job matches. Overall, however, there appears to have been no significant treatment effect (or externalities) on wages or earnings, in either the short- or the long-run.

7 Conclusion

This evaluation of an assistance program for young, college-educated job seekers offers a unique opportunity to measure both the direct impact of counseling and the equilibrium effects, in a given market, of providing counseling to a fixed proportion of job seekers. Robust measures are made possible by the two-step randomization design that was implemented on a very large scale.

We find that the reinforced counseling program did indeed have a positive impact on the employment status of young job seekers, compared to untreated job seekers, eight months after assignment into the treatment group. This effect was essentially limited to those in areas with for-profit operators, who may have had better contacts with prospective employers, and to those labor market outcomes which the operators were paid to achieve. Among men, however, these effects came partly at the expense of those who were not treated but were searching for a job in the same labor market (i.e. within the same specific occupation category), which reduced the overall program effect. Although the program benefited those who received counseling, a little less than half of those eligible actually accepted the offer to receive counseling, and so the overall impact of being offered the chance to be treated turns out to be insignificant for men. An open question that remains is why externalities were stronger for men than for women.

Indeed, in our setup, a back of the envelope calculation suggests that the externalities imposed were large enough, and the take-up rate was low enough, that the number of extra jobs obtained by those treated (treatment effect multiplied by eligible population) was only about twice as large as the number of jobs that were lost by eligible workers who were untreated (externality multiplied by the population of untreated workers). Note that this does not take into

account the externalities imposed on ineligible workers, which, as the results disaggregated by κ suggest, are potentially large.

The externalities we estimate suggest that part of the program effects in the short-run were due to an improvement in the search ability of some workers, which reduced the relative job search success of others. This implies that neither labor demand nor wages fully adjusted to the reduction in search frictions (i.e. the movement of the Beveridge curve) in the short run. These results are consistent with a simple search model, which makes an additional prediction that is also verified in the data: externalities are strongest in weak labor markets in which competition for jobs is fiercer.

Furthermore, no long-term, structural, effect of the program can be observed, even in professions in which the treatment affected a significant share of job seekers. Indeed, additional evidence suggests that the main effect of the program was to help those treated to find a job slightly faster, at the expense of others who subsequently took longer to find employment. In particular, after 12 months (and up to 20 months), the program effects (both direct and externality) on durable employment (or any form of employment) had entirely disappeared, both for men and for women.

These results suggest that the current enthusiasm among policy makers in Europe for active labor market policies, such as the one we study, probably needs to be tempered, as most available positive evidence does not measure equilibrium effects. These policies may have mostly redistributive properties, in which case participant targeting should be high on the agenda. Our results also imply that there are potentially important externalities associated with improved job search in the labor market, which has implications for the optimal design of unemployment insurance and other social protection policies (Landais, Michaillat, and Saez, 2010).

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Table 1: Response rates

	(1)	(2)	(3)	(4)	(5)
		Response rate			
Wave	Number of responses	All	Control	Treatment	Difference
8 month survey	23,320	0.787	0.782	0.791	0.009 (0.005)
12 month survey	21,970	0.741	0.735	0.746	0.011 (0.005)
16 month survey	20,791	0.702	0.695	0.707	0.012 (0.005)
20 month survey	21,521	0.726	0.722	0.730	0.008 (0.005)

Source: job seekers' register (ANPE) and follow-up survey (DARES).

Notes: Column 1 reports the total number of responses to surveys after 8, 12, 16 and 20 months (total number of sampled individuals is 29,636). Columns 2, 3 and 4 report response rates in the full sample, the control subsample, and the treatment subsample, respectively. Column 5 reports the difference between columns 4 and 3 (standard error in parenthesis).

Table 2: Summary statistics (1)

Variables	(1)	(2)	(3)	(4)	(5)
	Proportions			Balancing stats	
	All	Control	Treatment	Difference	P-value
Age					
Less than 21	0.050	0.049	0.050	-0.001	0.777
22	0.067	0.061	0.071	0.003	0.404
23	0.091	0.093	0.089	-0.003	0.531
24	0.109	0.108	0.109	-0.003	0.620
25	0.136	0.136	0.136	0.001	0.900
26	0.145	0.144	0.145	-0.001	0.881
27	0.144	0.145	0.143	0.001	0.870
28	0.134	0.138	0.132	-0.001	0.875
29	0.125	0.126	0.125	0.003	0.556
Seniority in unemployment					
0 to 5 months	0.166	0.166	0.166	0.008	0.179
6 months	0.111	0.109	0.113	0.000	0.956
7 months	0.312	0.306	0.317	0.007	0.278
8 months	0.087	0.089	0.084	-0.005	0.250
9 to 12 months	0.122	0.123	0.120	0.002	0.722
12 to 18 months	0.112	0.113	0.111	-0.004	0.423
18 to 24 months	0.037	0.037	0.036	-0.000	0.929
24 to 36 months	0.035	0.037	0.034	-0.006	0.062
more than 36 months	0.018	0.019	0.018	-0.003	0.225
Benefit recepient					
Benefit recipient	0.310	0.301	0.316	0.011	0.124
Non-benefit recipient	0.690	0.699	0.684	-0.011	0.124
Gender					
Female	0.635	0.643	0.628	-0.021	0.007
Male	0.365	0.357	0.372	0.021	0.007
Highest degree					
PhD	0.012	0.013	0.011	-0.001	0.566
Master's degree from a university	0.112	0.114	0.109	0.003	0.617
Engineer, Business School Degree	0.020	0.021	0.019	-0.001	0.764
Maitrise (Bac+4)	0.065	0.064	0.066	0.007	0.069
Other Bac+4/5	0.031	0.028	0.033	0.003	0.231
Bac+3	0.162	0.162	0.161	-0.006	0.329
Bac+2 from a university	0.026	0.025	0.027	0.003	0.215
Technical Bac+2	0.326	0.320	0.331	0.002	0.804
Other Bac+2/3	0.082	0.082	0.082	-0.006	0.176
Less than Bac+2	0.039	0.041	0.037	-0.001	0.660
Not declared	0.127	0.131	0.123	-0.003	0.534
Number of observations	29636	13148	16488		

Source: Job seekers' register (ANPE).

Notes: These summary statistics are based on the individuals sampled for the survey, regardless of whether they responded to the survey. Columns 1, 2 and 3 report the means of individual characteristics in the full sample, the control subsample, and the treatment subsample, respectively. Column 4 reports the difference between columns 2 and 3 and column 5 the p-value of the corresponding test of zero difference.

Table 3: Summary statistics (2)

Variables	(1)	(2)	(3)	(4)
	Proportions			P-value
	All	Control	Treatment	
Employed at randomization				
Employed	0.443	0.447	0.439	0.228
Not employed	0.424	0.419	0.428	0.145
Did not answer	0.134	0.135	0.133	0.674
Number of observations	23320	13042	10278	

Source: follow-up survey (DARES).

Notes: These summary statistics are based on the sample of individuals who responded to the first survey wave, eight months after random assignment. Employed at randomization is measured by the answer to the question "On XX/XX/07, did you have a job?", where XX/XX/07 is the date on which the individual was randomly assigned to treatment or control. Columns 1, 2 and 3 report the means of employment status in the full sample, the control subsample, and the treatment subsample, respectively. Column 4 reports the p-value of the test of zero difference between columns 2 and 3.

Table 4: Jobs with highest and lowest share of skilled job seekers

(1) Job	(2) Share of job seekers who are skilled
Jobs with lowest shares	
Bakery industry worker	0.006
Needlewoman	0.007
Roofer	0.010
Windows/doors/gates fitter	0.010
Concrete worker	0.010
Sheet fitter	0.010
Cooking assistant	0.011
Motorcycle mechanic	0.012
Construction vehicle driver	0.012
Jobs with highest shares	
Psychologist	0.967
Technical planning manager	0.969
Technical production manager	0.977
Lawyer	0.979
Financial officer	0.983
Lecturer/assistant professor	0.986
Research specialist in human sciences	0.987
Executive manager in public sector	0.991
Technical R&D manager	0.994
Actuary	1.000

Source: Job seekers' register (ANPE).

Notes: In this table, we used the exhaustive, national, anonymous job seekers' register (ANPE) of 2007. Occupations are defined in a nomenclature of 466 jobs. The job a given job seeker is categorized as searching for is the one he declared that he was searching for during his first meeting with an ANPE caseworker. The share of skilled job seekers searching for a job is measured, for each job, as the ratio between the number of job seekers younger than 30 with at least a two-year college degree that reported searching for this job and the total number of job seekers younger than 30 that reported searching for this job. Column 1 reports the ten occupations with lowest and highest shares; column 2 reports the corresponding shares.

Table 5: Take-up and intermediate variables

Dependent variable	(1) All workers All	(2) All workers For-Profit	(3) Unemployed All	(4) Unemployed For-Profit	(5) Employed All	(6) Employed For-Profit	(7) Unemployed men All	(8) Unemployed men For-Profit	(9) Unemployed women All	(10) Unemployed women For-Profit
Panel A: First stage: Program participation										
A1 Without LEA dummies										
Program participation	0.348***	0.304***	0.441***	0.386***	0.245***	0.219***	0.453***	0.389***	0.433***	0.385***
	(0.008)	(0.013)	(0.010)	(0.016)	(0.008)	(0.016)	(0.013)	(0.021)	(0.012)	(0.018)
A2 With LEA dummies										
Program participation	0.345***	0.316***	0.438***	0.401***	0.244***	0.228***	0.440***	0.387***	0.429***	0.401***
	(0.010)	(0.017)	(0.014)	(0.021)	(0.012)	(0.020)	(0.018)	(0.026)	(0.016)	(0.026)
Panel B: Reduced form: Impact of being assigned to treatment										
Number of meeting with a	0.542***	0.498***	0.658***	0.636***	0.443***	0.388***	0.631***	0.554***	0.666***	0.676***
counselor	(0.058)	(0.080)	(0.086)	(0.113)	(0.061)	(0.103)	(0.121)	(0.187)	(0.104)	(0.147)
Control mean	2.514	2.547	2.934	2.989	1.360	1.371	3.004	3.140	2.894	2.904
Received help with CV, coaching	0.097***	0.092***	0.114***	0.109***	0.081***	0.076***	0.122***	0.125***	0.110***	0.101***
for interviews, etc.	(0.007)	(0.011)	(0.010)	(0.016)	(0.008)	(0.014)	(0.015)	(0.022)	(0.013)	(0.023)
Control mean	0.216	0.209	0.260	0.250	0.126	0.122	0.271	0.249	0.254	0.250
Help with matching (identify	0.005	-0.009	0.007	-0.012	0.007	-0.011	0.010	-0.020	0.006	-0.006
job offers, help with transportation)	(0.005)	(0.009)	(0.008)	(0.014)	(0.006)	(0.010)	(0.013)	(0.021)	(0.010)	(0.017)
Control mean	0.157	0.160	0.194	0.199	0.102	0.109	0.217	0.223	0.180	0.186
Panel C: IV regression: Impact of participation										
Number of meetings with a	1.563***	1.638***	1.494***	1.650***	1.817***	1.781***	1.399***	1.437***	1.536***	1.747***
counselor	(0.159)	(0.255)	(0.188)	(0.285)	(0.235)	(0.458)	(0.260)	(0.470)	(0.230)	(0.362)
Control mean	2.514	2.547	2.934	2.989	1.360	1.371	3.004	3.140	2.894	2.904
Received help with CV, coaching	0.279***	0.304***	0.260***	0.284***	0.332***	0.348***	0.271***	0.324***	0.254***	0.262***
for interviews, etc.	(0.017)	(0.034)	(0.021)	(0.039)	(0.031)	(0.058)	(0.031)	(0.057)	(0.028)	(0.055)
Control mean	0.216	0.209	0.260	0.250	0.126	0.122	0.271	0.249	0.254	0.250
Help with matching (identify	0.015	-0.028	0.016	-0.031	0.028	-0.051	0.023	-0.051	0.013	-0.014
job offers, help with transports)	(0.015)	(0.029)	(0.019)	(0.035)	(0.024)	(0.048)	(0.029)	(0.053)	(0.023)	(0.043)
Control mean	0.157	0.160	0.194	0.199	0.102	0.109	0.217	0.223	0.180	0.186
Observations	23320	8756	9890	3678	10317	3933	3716	1378	6174	2300

Source: Job seekers' register (ANPE) and follow-up survey (DARES).

Notes: Panel A1 reports OLS regressions of program participation on program assignment, controlling for gender, unemployment duration (in months), unemployment duration squared and 5 dummies for diplomas (see equation 5). Panel A2 reports the same OLS regressions as Panel A1, with 235 dummies for Local Employment Agencies (LEA) added to the set of covariates. Panel B reports OLS regressions of the row dependent variables on program assignment, controlling for the same covariates (see equation 4). Panel C reports IV regressions of the row dependent variables on program participation, controlling for the same covariates, with program participation instrumented for with program assignment (see equation 5 for the first stage and equation 3 for the second stage). "Unemployed "and "Employed" refer to self-declared status at the date of randomization based on the 8 month survey. Columns (2), (4), (6), (8), and (10) are restricted to regions in which the private operator was for-profit. Control means display means of the dependent variable for the subset of individuals assigned to the control group. Standard errors are robust to heteroskedasticity and clustered at the LEA level.

Table 6: Effect of the program on beneficiaries: Basic results ignoring externalities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Labor market outcome: fixed-term contract with a length of more than six months							
	All workers		Unemployed		Unemployed men		Unemployed women	
	All	For-Profit	All	For-Profit	All	For-Profit	All	For-Profit
Panel A: Reduced form: Estimated impact of being assigned to treatment								
A1 Without LEA dummies								
Assigned to treatment	0.010*	0.023***	0.024***	0.046***	0.033***	0.071***	0.019*	0.035**
	(0.005)	(0.009)	(0.008)	(0.012)	(0.012)	(0.022)	(0.010)	(0.016)
A2 With LEA dummies								
Assigned to treatment	0.007	0.015	0.020*	0.032*	0.051***	0.112***	0.005	-0.006
	(0.008)	(0.012)	(0.011)	(0.018)	(0.019)	(0.034)	(0.015)	(0.022)
Equality tests (p-value)	0.83	0.59	0.77	0.53	0.42	0.31	0.41	0.14
Panel B: IV regression: Estimated impact of participation								
B1 Without LEA dummies								
Treated (enrolled in program)	0.028*	0.077***	0.055***	0.120***	0.074***	0.185***	0.045*	0.090**
	(0.015)	(0.029)	(0.017)	(0.033)	(0.026)	(0.058)	(0.023)	(0.041)
B2 With LEA dummies								
Treated (enrolled in program)	0.021	0.049	0.046*	0.079*	0.116***	0.289***	0.012	-0.014
	(0.022)	(0.037)	(0.025)	(0.044)	(0.042)	(0.088)	(0.035)	(0.052)
Equality tests (p-value)	0.83	0.55	0.77	0.47	0.40	0.34	0.45	0.13
Control mean	0.199	0.194	0.213	0.206	0.172	0.172	0.237	0.225
Observations	23320	8756	9890	3678	3716	1378	6174	2300

Source: Job seekers' register (ANPE) and follow-up survey (DARES).

Notes: The dependent variable is having a job with a fixed-term contract with a length of at least six months, when surveyed 8 months after random assignment. Panel A1 reports OLS regressions of this variable on program assignment, controlling for gender, unemployment duration (in months), unemployment duration squared, 5 dummies for diplomas, and 47 dummy variables for Local Employment Agency (LEA) quintuplets. In Panel A2, the dummy variables for quintuplets are replaced by 235 LEA dummies (see equation 4). Panel B1 reports IV regressions of the row dependent variables on program participation, controlling for the same covariates, with program participation instrumented for with program assignment. In Panel B2, the dummy variables for quintuplets are replaced by 235 LEA dummies (see equation 5 for the first stage and 3 for the second stage). "Unemployed "and "Employed" refers to self-declared status at the date of randomization based on the 8 month survey. Columns (2), (4), (6), and (8) are restricted to regions in which the private operator was for-profit. Control means display means of the dependent variable for the subset of individuals assigned to the control group. For each panel, the p-values of the equality tests between the coefficients from the previous two specifications are presented. Standard errors are robust to heteroskedasticity and clustered at the LEA level.

Table 7: Reduced form: Impact of program assignment and assignment probability

	Labor market outcome: fixed-term contract with a length of more than six months									
	All workers		Unemployed		Employed		Unemployed men		Unemployed women	
	All	For-Profit	All	For-Profit	All	For-Profit	All	For-Profit	All	For-Profit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Assigned to treatment in 25% area	0.020*	0.038**	0.023	0.043*	0.026	0.042*	0.008	0.039	0.032	0.049*
	(0.011)	(0.017)	(0.015)	(0.025)	(0.016)	(0.022)	(0.023)	(0.042)	(0.020)	(0.029)
Assigned to treatment in 50% area	0.010	0.024	0.010	0.020	0.013	0.026	0.011	0.069	0.011	-0.006
	(0.011)	(0.020)	(0.015)	(0.025)	(0.016)	(0.028)	(0.026)	(0.052)	(0.019)	(0.025)
Assigned to treatment in 75% area	0.006	0.023*	0.012	0.033*	0.003	0.021	0.003	0.009	0.016	0.050*
	(0.008)	(0.013)	(0.012)	(0.017)	(0.012)	(0.020)	(0.018)	(0.031)	(0.016)	(0.027)
Assigned to treatment in 100% area	0.012	0.039**	0.028**	0.074***	-0.004	0.015	0.022	0.047	0.033*	0.095***
	(0.008)	(0.016)	(0.012)	(0.022)	(0.012)	(0.020)	(0.015)	(0.030)	(0.017)	(0.028)
Assigned to control in 25% area	-0.002	0.021	-0.021	-0.007	0.014	0.044*	-0.048**	-0.085**	-0.005	0.040
	(0.010)	(0.017)	(0.013)	(0.022)	(0.015)	(0.023)	(0.022)	(0.039)	(0.016)	(0.026)
Assigned to control in 50% area	0.002	0.006	-0.004	-0.005	0.007	0.010	-0.022	-0.017	0.010	0.006
	(0.010)	(0.016)	(0.015)	(0.023)	(0.014)	(0.021)	(0.022)	(0.050)	(0.021)	(0.027)
Assigned to control in 75% area	0.026*	0.016	0.016	0.032	0.040*	0.014	-0.063**	-0.094**	0.046	0.089**
	(0.015)	(0.024)	(0.024)	(0.033)	(0.024)	(0.037)	(0.031)	(0.038)	(0.029)	(0.044)
Control mean	0.199	0.194	0.213	0.206	0.246	0.238	0.172	0.172	0.237	0.225
F-test for equality of all assigned to treatment coefficients	.61	.70	.56	.27	.29	.70	.81	.62	.63	.01**
F-test for equality of all assigned to control coefficients	.21	.71	.28	.54	.40	.38	.47	.37	.22	.17
F-test for all assigned to treatment coefficients equal to zero	.33	.07*	.19	.01**	.41	.41	.71	.38	.28	.00***
F-test for all assigned to control coefficients equal to zero	.34	.60	.32	.74	.34	.30	.05**	.03**	.37	.13
Observations	23320	8756	9890	3678	10317	3933	3716	1378	6174	2300

Source: Job seekers' register (ANPE) and follow-up survey (DARES).

Notes: The dependent variable is having a job with a fixed-term contract with a length of at least six months, when surveyed 8 months after random assignment. The table reports OLS regressions of this variable on program assignment interacted with assignment probability dummies, controlling for gender, unemployment duration (in months), unemployment duration squared, 5 dummies for diplomas and 47 dummy variables for Local Employment Agency (LEA) quintuplets (see equation 7). "Unemployed "and "Employed" refer to self-declared status at the date of randomization based on the 8 month survey. Columns (2), (4), (6), (8), and (10) are restricted to regions in which the private operator was for-profit. Control means display means of the dependent variable for the subset of individuals assigned to the control group. The first test line is for equality of assignment to treatment effects in 25%, 50%, 75% and 100% areas; the second test line is for equality of assignment to control effects in 25%, 50% and 75% areas. The last two test lines are for the joint null of assignment to treatment effects equal to 0 in 25%, 50%, 75% and 100% areas, and for the joint null of assignment to control effects equal to 0 in 25%, 50% and 75% areas. All tests are F-tests and only p-values are reported. Standard errors are robust to heteroskedasticity and clustered at the LEA level.

Table 8: Reduced form: Impact of the program, accounting for externalities

	All unemployed				Men (unemployed)				Women (unemployed)			
	By job type: share of job seekers who are eligible for program											
	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: fixed-term contract with a length of more than six months												
Assigned to program (β)	0.028***	0.032**	0.027**	0.051**	0.051***	0.046**	0.054**	0.089**	0.016	0.027	0.014	0.033
	(0.010)	(0.015)	(0.014)	(0.022)	(0.015)	(0.019)	(0.023)	(0.035)	(0.012)	(0.021)	(0.017)	(0.029)
In a Program area (δ)	-0.009	-0.014	-0.007	-0.049*	-0.039**	-0.024	-0.049*	-0.106**	0.008	-0.005	0.020	-0.018
	(0.011)	(0.018)	(0.015)	(0.026)	(0.016)	(0.023)	(0.025)	(0.043)	(0.015)	(0.024)	(0.020)	(0.033)
Net effect	0.019**	0.019	0.020	0.001	0.012	0.022	0.006	-0.017	0.024*	0.021	0.034**	0.015
of program assignment ($\beta + \delta$)	(0.009)	(0.014)	(0.012)	(0.022)	(0.013)	(0.020)	(0.019)	(0.037)	(0.013)	(0.020)	(0.017)	(0.028)
Control mean	0.213	0.195	0.226	0.252	0.172	0.151	0.192	0.214	0.237	0.221	0.245	0.269
Panel B: Durable employment												
Assigned to program (β)	0.024*	0.047**	0.011	0.016	0.035*	0.031	0.033	0.048	0.021	0.057***	0.000	0.004
	(0.013)	(0.018)	(0.017)	(0.025)	(0.020)	(0.029)	(0.029)	(0.041)	(0.015)	(0.022)	(0.021)	(0.031)
In a Program area (δ)	-0.012	-0.028	-0.013	-0.006	-0.034	-0.018	-0.049	-0.075	-0.003	-0.030	0.010	0.025
	(0.014)	(0.020)	(0.019)	(0.027)	(0.024)	(0.032)	(0.033)	(0.052)	(0.018)	(0.027)	(0.024)	(0.035)
Net effect	0.012	0.019	-0.002	0.010	0.002	0.013	-0.016	-0.027	0.017	0.027	0.010	0.029
of program assignment ($\beta + \delta$)	(0.013)	(0.018)	(0.016)	(0.021)	(0.021)	(0.027)	(0.026)	(0.040)	(0.017)	(0.023)	(0.020)	(0.028)
Control mean	0.480	0.442	0.512	0.531	0.484	0.448	0.520	0.540	0.478	0.439	0.507	0.526
Panel C: Employment												
Assigned to program (β)	0.022*	0.038**	0.009	0.021	0.023	-0.000	0.038	0.074*	0.024	0.062***	-0.003	0.001
	(0.012)	(0.017)	(0.016)	(0.019)	(0.017)	(0.027)	(0.028)	(0.041)	(0.016)	(0.021)	(0.020)	(0.026)
In a Program area (δ)	-0.015	-0.024	-0.016	-0.030	0.000	0.040	-0.024	-0.077	-0.028	-0.059**	-0.013	-0.008
	(0.014)	(0.020)	(0.018)	(0.024)	(0.023)	(0.032)	(0.032)	(0.050)	(0.018)	(0.026)	(0.023)	(0.032)
Net effect	0.007	0.014	-0.007	-0.009	0.024	0.040	0.013	-0.002	-0.004	0.004	-0.016	-0.007
of program assignment ($\beta + \delta$)	(0.012)	(0.017)	(0.014)	(0.021)	(0.021)	(0.027)	(0.026)	(0.039)	(0.016)	(0.023)	(0.020)	(0.029)
Control mean	0.637	0.606	0.663	0.670	0.632	0.607	0.658	0.655	0.639	0.606	0.666	0.677
Observations	9890	4619	5143	2591	3716	1830	1870	843	6174	2789	3273	1748

Source: Job seekers' register (ANPE) and follow-up survey (DARES).

Notes: The dependent variable in Panel A is having a job with a fixed-term contract with a length of at least six months, when surveyed 8 months after random assignment; in Panel B, it is having a fixed-term contract with a length of more than six months or an indefinite-term contract, when surveyed 8 months after random assignment; in Panel C, it is having any type of job, when surveyed 8 months after random assignment. In each panel, the two first lines report OLS regressions of the dependent variable on a dummy for assigned to program and a dummy for being in a Local Employment Agency (LEA) with positive assignment probability, controlling for gender, unemployment duration (in months), unemployment duration squared, 5 dummies for diplomas and 47 dummy variables for LEA quintuplets (see equation 8). The third line reports the sum of the two previous parameters. All estimates are for individuals who reported in the the 8 month survey that they were unemployed at the date of randomization. Columns (1), (5) and (9) include job seekers searching for all kinds of jobs; columns (2), (6) and (10) include job seekers searching for jobs in which the share of skilled job seekers (κ) is below the median; columns (3), (7), and (11) include job seekers searching for jobs in which the share of skilled job seekers is above the median; columns (4), (8) and (12) include job seekers searching for jobs in which the share of skilled job seekers is above the third quartile. Control means display means of the dependent variable for the subset of individuals assigned to the control group. Standard errors are robust to heteroskedasticity and clustered at the LEA level.

Table 9: Effect of the treatment, accounting for externalities

	All unemployed				Men (unemployed)				Women (unemployed)			
	By job type: share of job seekers who are eligible for program											
	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: fixed-term contract with a length of more than six months												
Program participation (β)	0.063***	0.076**	0.059**	0.118**	0.113***	0.104**	0.117**	0.226***	0.037	0.065	0.031	0.075
	(0.022)	(0.034)	(0.030)	(0.050)	(0.034)	(0.043)	(0.049)	(0.087)	(0.028)	(0.050)	(0.037)	(0.063)
In a program area (δ)	-0.009	-0.014	-0.007	-0.050*	-0.039**	-0.023	-0.050**	-0.106**	0.008	-0.005	0.020	-0.019
	(0.011)	(0.017)	(0.015)	(0.026)	(0.016)	(0.022)	(0.025)	(0.042)	(0.015)	(0.023)	(0.020)	(0.032)
Net effect	0.055***	0.063**	0.052**	0.068*	0.074***	0.080**	0.067*	0.120	0.045*	0.060	0.051*	0.056
of program on participants ($\beta + \delta$)	(0.017)	(0.027)	(0.024)	(0.041)	(0.026)	(0.036)	(0.037)	(0.076)	(0.024)	(0.040)	(0.030)	(0.051)
Control mean	0.213	0.195	0.226	0.252	0.172	0.151	0.192	0.214	0.237	0.221	0.245	0.269
Panel B: Durable employment												
Program participation (β)	0.055*	0.111**	0.023	0.037	0.078*	0.069	0.071	0.121	0.048	0.140***	0.001	0.010
	(0.029)	(0.043)	(0.038)	(0.057)	(0.044)	(0.063)	(0.061)	(0.101)	(0.034)	(0.053)	(0.047)	(0.069)
In a program area (δ)	-0.013	-0.028	-0.013	-0.006	-0.034	-0.018	-0.050	-0.075	-0.003	-0.030	0.010	0.025
	(0.014)	(0.020)	(0.019)	(0.027)	(0.024)	(0.031)	(0.033)	(0.051)	(0.018)	(0.026)	(0.024)	(0.035)
Net effect	0.042*	0.083**	0.010	0.031	0.045	0.051	0.021	0.046	0.044	0.109**	0.010	0.034
of program on participants ($\beta + \delta$)	(0.025)	(0.037)	(0.030)	(0.044)	(0.038)	(0.052)	(0.048)	(0.081)	(0.030)	(0.045)	(0.037)	(0.054)
Control mean	0.480	0.442	0.512	0.531	0.484	0.448	0.520	0.540	0.478	0.439	0.507	0.526
Panel C: Employment												
Program participation (β)	0.050*	0.089**	0.019	0.049	0.052	-0.000	0.081	0.188*	0.056	0.152***	-0.006	0.002
	(0.028)	(0.040)	(0.036)	(0.045)	(0.039)	(0.059)	(0.060)	(0.102)	(0.036)	(0.051)	(0.043)	(0.058)
In a program area (δ)	-0.015	-0.024	-0.016	-0.030	0.000	0.040	-0.025	-0.077	-0.029	-0.059**	-0.013	-0.009
	(0.014)	(0.020)	(0.018)	(0.024)	(0.023)	(0.032)	(0.032)	(0.049)	(0.018)	(0.026)	(0.023)	(0.032)
Net effect	0.035	0.065*	0.003	0.019	0.052	0.040	0.056	0.111	0.027	0.093**	-0.019	-0.006
of program on participants ($\beta + \delta$)	(0.023)	(0.034)	(0.028)	(0.037)	(0.034)	(0.048)	(0.047)	(0.083)	(0.030)	(0.044)	(0.036)	(0.049)
Control mean	0.637	0.606	0.663	0.670	0.632	0.607	0.658	0.655	0.639	0.606	0.666	0.677
Observations	9890	4619	5143	2591	3716	1830	1870	843	6174	2789	3273	1748

Source: Job seekers' register (ANPE) and follow-up survey (DARES).

Notes: The dependent variable in Panel A is having a job with a fixed-term contract with a length of at least six months, when surveyed 8 months after random assignment; in Panel B, it is having a fixed-term contract with a length of more than six months or an indefinite-term contract, when surveyed 8 months after random assignment; in Panel C, it is having any type of job, when surveyed 8 months after random assignment. In each panel, the two first lines report IV regressions of the dependent variable on a dummy for program participation and a dummy for being in a Local Employment Agency (LEA) with positive assignment probability, controlling for gender, unemployment duration (in months), unemployment duration squared, 5 dummies for diplomas and 47 dummy variables for LEA quintuplets. Program participation is instrumented by program assignment (see equation 9). The third line reports the sum of the two previous parameters (in practice, it is computed based on equation 10). All estimates are for individuals who reported in the the 8 month survey that they were unemployed at the date of randomization. Columns (1), (5) and (9) include job seekers searching for all kinds of jobs; columns (2), (6) and (10) include job seekers searching for jobs in which the share of skilled job seekers (κ) is below the median; columns (3), (7), and (11) include job seekers searching for jobs in which the share of skilled job seekers is above the median; columns (4), (8) and (12) include job seekers searching for jobs in which the share of skilled job seekers is above the third quartile. Control means display means of the dependent variable for the subset of individuals assigned to the control group. Standard errors are robust to heteroskedasticity and clustered at the LEA level.

Table 10: Heterogeneity of program effect by area and cohort

	fixed-term contract with a length of more than six months			**Durable employment**		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Men	Women	All	Men	Women
Panel A: Cohort differences						
Program participation (β_7^L)	0.086***	0.110**	0.073*	0.094**	0.091	0.105*
bad cohort	(0.030)	(0.049)	(0.039)	(0.045)	(0.070)	(0.059)
Program participation (β_7^H)	0.042	0.111**	0.006	0.017	0.061	-0.002
good cohort	(0.032)	(0.049)	(0.041)	(0.037)	(0.057)	(0.045)
In a program area (δ_7^L)	-0.061***	-0.075***	-0.052***	-0.103***	-0.105***	-0.105***
bad cohort	(0.013)	(0.020)	(0.017)	(0.018)	(0.032)	(0.024)
In a program area (δ_7^H)	0.027**	-0.014	0.050***	0.050***	0.015	0.067***
good cohort	(0.013)	(0.020)	(0.018)	(0.017)	(0.027)	(0.021)
Mean bad cohort (control)	0.168	0.129	0.189	0.391	0.401	0.385
Mean good cohort (control)	0.243	0.199	0.268	0.537	0.536	0.538
test: $\beta_7^L = \beta_7^H$ (p-value)	0.319	0.988	0.248	0.184	0.740	0.164
test: $\delta_7^L = \delta_7^H$ (p-value)	0.000***	0.010**	0.000***	0.000***	0.001***	0.000***
Panel B: Region differences						
Program participation (β_8^L)	0.067**	0.129***	0.032	0.069*	0.112**	0.044
bad area	(0.029)	(0.043)	(0.038)	(0.038)	(0.056)	(0.042)
Program participation (β_8^H)	0.058*	0.091*	0.043	0.033	0.032	0.049
good area	(0.032)	(0.055)	(0.043)	(0.045)	(0.070)	(0.059)
In a program area (δ_8^L)	-0.016	-0.055***	0.007	-0.031*	-0.058**	-0.016
bad area	(0.014)	(0.020)	(0.019)	(0.017)	(0.028)	(0.021)
In a program area (δ_8^H)	0.001	-0.017	0.009	0.011	-0.001	0.013
good area	(0.014)	(0.023)	(0.018)	(0.019)	(0.033)	(0.025)
Mean bad area (control)	0.212	0.183	0.229	0.475	0.485	0.470
Mean good area (control)	0.215	0.158	0.247	0.486	0.483	0.487
test: $\beta_8^L = \beta_8^H$ (p-value)	0.839	0.582	0.862	0.538	0.377	0.948
test: $\delta_8^L = \delta_8^H$ (p-value)	0.337	0.169	0.951	0.059*	0.125	0.315
Panel C: Region and cohort differences						
Program participation (β_9^{LL})	0.081**	0.146**	0.044	0.099*	0.163*	0.066
bad area, bad cohort	(0.040)	(0.057)	(0.054)	(0.057)	(0.090)	(0.075)
Program participation (β_9^{HL})	0.093**	0.051	0.115*	0.086	-0.023	0.163*
good area, bad cohort	(0.044)	(0.086)	(0.060)	(0.073)	(0.109)	(0.094)
Program participation (β_9^{LH})	0.051	0.106	0.021	0.037	0.057	0.024
bad area, good cohort	(0.040)	(0.067)	(0.053)	(0.047)	(0.079)	(0.050)
Program participation (β_9^{HH})	0.028	0.117	-0.016	-0.014	0.069	-0.044
good area, good cohort	(0.051)	(0.074)	(0.065)	(0.059)	(0.084)	(0.080)
In a program area (δ_9^{LL})	-0.067***	-0.097***	-0.050**	-0.124***	-0.133***	-0.121***
bad area, bad cohort	(0.017)	(0.025)	(0.023)	(0.022)	(0.040)	(0.029)
In a program area (δ_9^{HL})	-0.052***	-0.044	-0.055**	-0.076***	-0.066	-0.084**
good area, bad cohort	(0.017)	(0.030)	(0.023)	(0.025)	(0.044)	(0.034)
In a program area (δ_9^{LH})	0.020	-0.025	0.047**	0.035*	-0.004	0.057**
bad area, good cohort	(0.017)	(0.027)	(0.023)	(0.020)	(0.034)	(0.025)
In a program area (δ_9^{HH})	0.036**	0.000	0.054**	0.070***	0.039	0.082***
good area, good cohort	(0.018)	(0.027)	(0.023)	(0.023)	(0.038)	(0.032)
test: $\delta_9^{LL} = \delta_9^{HL} = \delta_9^{LH} = \delta_9^{HH}$	3.557e-08	.02881608	.00001145	8.944e-14	.00313514	6.063e-10
Control Mean	0.213	0.172	0.237	0.480	0.484	0.478
Observations	9890	3716	6174	9890	3716	6174

Source: Job seekers' register (ANPE) and follow-up survey (DARES).

Notes: The dependent variable in columns (1) to (3) is having a job with a contract with a length of at least six months, when surveyed 8 months after random assignment; in columns (4) to (6), it is having a fixed-term contract with a length of more than six months or an indefinite-term contract, when surveyed 8 months after random assignment. All panels report IV regressions of the dependent variable on a dummy for program participation and a dummy for being in a Local Employment Agency (LEA) with positive assignment probability, controlling for gender, unemployment duration (in months), unemployment duration squared, 5 dummies for diplomas and 47 dummy variables for LEA quintuplets. In panel A, the participation and program area variables are interacted with dummy variables characterizing whether or not the individual became unemployed between October 2007 and July 2008 (and hence was looking for employment during the worst of the economic crisis). In panel B, the participation and program area variables are interacted with whether an individual lives in an LEA with an unemployment rate above or below the median. In panel C, the participation and program area variables are interacted with the cohort-specific measure from panel A and the LEA-specific measure from panel B (see equations 12, 11, and 13, respectively). Program participation is instrumented by program assignment interacted with the relevant labor market condition variables. P-values for equality tests are reported. All estimates are for individuals who reported in the the 8 month survey that they were unemployed at the date of randomization. Control means display means of the dependent variable for the subset of individuals assigned to the control group (in bad/good areas/cohorts when specified). Standard errors are robust to heteroskedasticity and clustered at the LEA level.

Table 11: Long-term impact on durable employment, accounting for externalities

	All unemployed				Men (unemployed)				Women (unemployed)			
	By job type: share of job seekers who are eligible for program											
	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: 12 months												
Program participation (β)	0.018	0.063	-0.014	0.068	0.033	0.078	-0.014	0.024	0.010	0.042	-0.013	0.080
	(0.026)	(0.043)	(0.036)	(0.053)	(0.042)	(0.065)	(0.062)	(0.108)	(0.035)	(0.058)	(0.044)	(0.060)
In a program area (δ)	0.014	0.001	0.013	-0.022	0.011	0.025	-0.001	-0.044	0.013	-0.009	0.023	-0.005
	(0.014)	(0.020)	(0.019)	(0.030)	(0.026)	(0.035)	(0.035)	(0.059)	(0.019)	(0.029)	(0.026)	(0.035)
Net effect	0.031	0.064*	-0.001	0.046	0.044	0.103*	-0.014	-0.019	0.024	0.033	0.010	0.075
of program on participants ($\beta + \delta$)	(0.022)	(0.034)	(0.028)	(0.042)	(0.037)	(0.055)	(0.048)	(0.093)	(0.029)	(0.045)	(0.037)	(0.050)
Control mean	0.530	0.483	0.572	0.582	0.522	0.464	0.579	0.603	0.535	0.494	0.569	0.572
Observations	8632	4005	4511	2300	3216	1576	1627	734	5416	2429	2884	1566
Panel B: 16 months												
Program participation (β)	-0.004	0.014	-0.018	0.040	-0.026	-0.007	-0.070	-0.030	0.009	0.023	0.006	0.052
	(0.029)	(0.045)	(0.039)	(0.061)	(0.044)	(0.064)	(0.063)	(0.109)	(0.039)	(0.059)	(0.049)	(0.068)
In a program area (δ)	0.016	-0.004	0.029	0.020	0.032	0.019	0.054	0.070	0.003	-0.026	0.024	0.005
	(0.017)	(0.024)	(0.022)	(0.031)	(0.027)	(0.040)	(0.034)	(0.053)	(0.022)	(0.031)	(0.030)	(0.040)
Net effect	0.012	0.011	0.011	0.060	0.006	0.012	-0.017	0.040	0.013	-0.003	0.031	0.057
of program on participants ($\beta + \delta$)	(0.024)	(0.037)	(0.031)	(0.046)	(0.037)	(0.055)	(0.050)	(0.090)	(0.033)	(0.048)	(0.043)	(0.055)
Control mean	0.577	0.539	0.608	0.616	0.557	0.501	0.614	0.614	0.588	0.563	0.605	0.617
Observations	8049	3704	4240	2151	2986	1445	1528	694	5063	2259	2712	1457
Panel C: 20 months												
Program participation (β)	-0.050*	-0.008	-0.084**	-0.066	-0.047	-0.001	-0.122**	-0.101	-0.044	-0.006	-0.069	-0.068
	(0.026)	(0.043)	(0.034)	(0.053)	(0.041)	(0.070)	(0.055)	(0.109)	(0.038)	(0.053)	(0.046)	(0.064)
In a program area (δ)	0.024*	-0.001	0.037**	0.045	0.016	-0.008	0.047	0.017	0.024	-0.007	0.039*	0.067*
	(0.014)	(0.021)	(0.018)	(0.027)	(0.024)	(0.038)	(0.032)	(0.053)	(0.018)	(0.023)	(0.024)	(0.035)
Net effect	-0.026	-0.009	-0.047*	-0.021	-0.030	-0.009	-0.075	-0.084	-0.020	-0.013	-0.030	-0.002
of program on participants ($\beta + \delta$)	(0.022)	(0.035)	(0.027)	(0.042)	(0.034)	(0.057)	(0.047)	(0.093)	(0.031)	(0.044)	(0.037)	(0.050)
Control mean	0.624	0.582	0.659	0.674	0.607	0.555	0.659	0.665	0.633	0.599	0.658	0.678
Observations	8217	3798	4306	2203	3051	1477	1562	720	5166	2321	2744	1483

Source: Job seekers' register (ANPE) and waves 2 to 4 of the follow-up survey (DARES).

Notes: The dependent variable is having a job with a a fixed-term contract with a length of at least six months or an indefinite-term contract; in Panel A, it is measured 12 months after random assignment; in Panel B, 16 months; in Panel C, 20 months. In each panel, the two first lines report IV regressions of the dependent variable on a dummy for program participation and a dummy for being in a Local Employment Agency (LEA) with positive assignment probability, controlling for gender, unemployment duration (in months), unemployment duration squared, 5 dummies for diplomas and 47 dummy variables for LEA quintuplets. Program participation is instrumented by program assignment (see equation 9). The third line reports the sum of the two previous parameters (in practice, it is computed using equation 10). All estimates are for individuals who reported in the the 8 month survey that they were unemployed at the date of randomization. Columns (1), (5) and (9) include job seekers searching for all kinds of jobs; columns (2), (6) and (10) include job seekers searching for jobs in which the share of skilled job seekers (κ) is below the median; columns (3), (7), and (11) include job seekers searching for jobs in which the share of skilled job seekers is above the median; columns (4), (8) and (12) include job seekers searching for jobs in which the share of skilled job seekers is above the third quartile. Control means display means of the dependent variable for the subset of individuals assigned to the control group. Standard errors are robust to heteroskedasticity and clustered at the LEA level.

Table 12: Long-term impact on wages, accounting for externalities

	All unemployed				Men (unemployed)				Women (unemployed)			
	By job type: share of job seekers who are eligible for program											
	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: 8 months												
Program participation (β)	-10	167	-97	-437	346	420	385	401	-205	26	-291	-672
	(139)	(112)	(232)	(383)	(276)	(288)	(489)	(732)	(193)	(61)	(305)	(551)
In a program area (δ)	69	-25	130	228	-30	-47	-61	-223	117	-18	193	391*
	(60)	(40)	(102)	(155)	(120)	(107)	(235)	(302)	(78)	(29)	(125)	(230)
Net effect	60	142	33	-209	316	373	324	177	-88	8	-98	-281
of program on participants ($\beta + \delta$)	(105)	(111)	(173)	(272)	(227)	(268)	(371)	(638)	(129)	(49)	(203)	(364)
Control mean	1,324	1,16	1,46	1,543	1,482	1,3	1,642	1,679	1,237	1,074	1,363	1,483
Observations	6144	2746	3306	1704	2289	1070	1210	553	3855	1676	2096	1151
Panel B: 12 months												
Program participation (β)	-55	53	-109	31	-32	103	-109	411	-45	52	-90	25
	(146)	(86)	(231)	(409)	(324)	(209)	(530)	(1)	(151)	(70)	(239)	(402)
In a program area (d)	60	-74	163	318**	47	-45	83	269	72	-106	204**	289*
	(71)	(61)	(104)	(162)	(140)	(86)	(246)	(447)	(79)	(80)	(102)	(164)
Net effect	5	-21	53	349	15	58	-26	680	27	-54	115	314
of program on participants ($\beta + \delta$)	(118)	(91)	(177)	(317)	(240)	(215)	(359)	(730)	(122)	(95)	(191)	(352)
Control mean	1,345	1,181	1,479	1,522	1,516	1,302	1,694	1,746	1,252	1,111	1,364	1,418
Observations	5405	2394	2933	1531	1976	927	1044	480	3429	1467	1889	1051

Source: Job seekers' register (ANPE) and waves 2 to 4 of the follow-up survey (DARES) .

Notes: The dependent variable is the wage earned, and is defined for those who are employed; in Panel A, it is measured 8 months after random assignment; in Panel B, 12 months. In each panel, the two first lines report IV regressions of the dependent variable on a dummy for program participation and a dummy for being in a Local Employment Agency (LEA) with positive assignment probability, controlling for gender, unemployment duration (in months), unemployment duration squared, 5 dummies for diplomas and 47 dummy variables for LEA quintuplets. Program participation is instrumented by program assignment (see equation 9). The third line reports the sum of the two previous parameters (in practice, it is computed using equation 10). All estimates are for individuals who reported in the the 8 month survey that they were unemployed at the date of randomization. Columns (1), (5) and (9) include job seekers searching for all kinds of jobs; columns (2), (6) and (10) include job seekers searching for jobs in which the share of skilled job seekers (κ) is below the median; columns (3), (7), and (11) include job seekers searching for jobs in which the share of skilled job seekers is above the median; columns (4), (8) and (12) include job seekers searching for jobs in which the share of skilled job seekers is above the third quartile. Control means display means of the dependent variable for the subset of individuals assigned to the control group. Standard errors are robust to heteroskedasticity and clustered at the LEA level.

Table 13: Long-term impact on wages, accounting for externalities (continued)

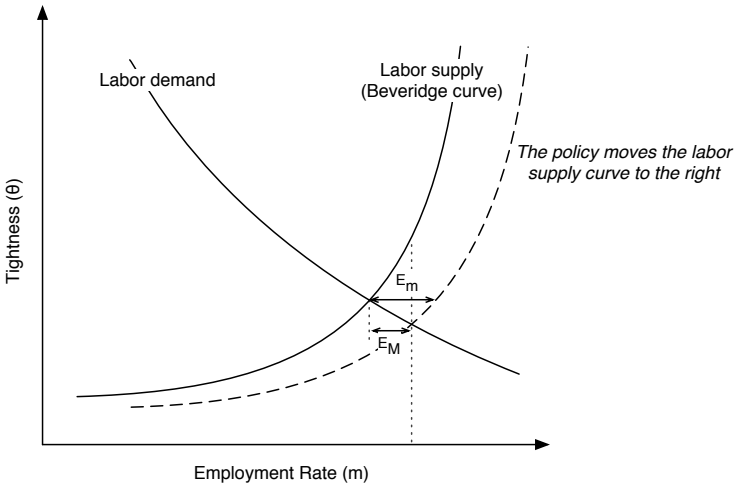
	All unemployed				Men (unemployed)				Women (unemployed)			
	By job type: share of job seekers who are eligible for program											
	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel C: 16 months												
Program participation (β)	-296	-390	-261	-152	-169	586	-792**	-1,300**	-320	-1	49	317
	(242)	(562)	(194)	(289)	(271)	(431)	(379)	(589)	(350)	(996)	(219)	(332)
In a program area (δ)	135	248	60	113	72	-69	153	557**	140	369	30	-86
	(106)	(212)	(109)	(132)	(139)	(167)	(244)	(263)	(130)	(351)	(97)	(142)
Net effect	-162	-142	-201	-38	-97	517*	-639**	-743*	-180	-630	79	232
of program on participants	(164)	(375)	(169)	(239)	(221)	(311)	(317)	(392)	(236)	(671)	(193)	(329)
Control mean	1,421	1,314	1,512	1,536	1,557	1,278	1,794	1,844	1,343	1,336	1,356	1,393
Observations	5257	2330	2853	1476	1894	871	1017	465	3363	1459	1836	1011
Panel D: 20 months												
Program participation (β)	-37	109	-120	-49	113	391	-75	218	-108	1	-131	-141
	(107)	(133)	(161)	(202)	(235)	(447)	(306)	(366)	(113)	(58)	(167)	(228)
In a program area (δ)	-9	-16	-27	-97	-43	-113	8	-157	1	-20	-22	-16
	(57)	(52)	(101)	(145)	(112)	(160)	(191)	(282)	(62)	(32)	(114)	(140)
Net effect	-46	93	-146	-146	70	278	-67	61	-107	-19	-153	-156
of program on participants ($\beta + \delta$)	(91)	(128)	(132)	(188)	(201)	(349)	(245)	(377)	(92)	(48)	(147)	(190)
Control Mean	1,384	1,241	1,509	1,542	1,53	1,415	1,632	1,701	1,305	1,143	1,443	1,469
Observations	5544	2475	2981	1563	1989	934	1047	488	3555	1541	1934	1075

Source: Job seekers' register (ANPE) and waves 2 to 4 of the follow-up survey (DARES) .

Notes: The dependent variable is the wage earned, and is defined for those who are employed; in Panel C, it is measured 16 months after random assignment; in Panel D, 20 months. In each panel, the two first lines report IV regressions of the dependent variable on a dummy for program participation and a dummy for being in a Local Employment Agency (LEA) with positive assignment probability, controlling for gender, unemployment duration (in months), unemployment duration squared, 5 dummies for diplomas and 47 dummy variables for LEA quintuplets. Program participation is instrumented by program assignment (see equation 9). The third line reports the sum of the two previous parameters (in practice, it is computed using equation 10). All estimates are for individuals who reported in the the 8 month survey that they were unemployed at the date of randomization. Columns (1), (5) and (9) include job seekers searching for all kinds of jobs; columns (2), (6) and (10) include job seekers searching for jobs in which the share of skilled job seekers (κ) is below the median; columns (3), (7), and (11) include job seekers searching for jobs in which the share of skilled job seekers is above the median; columns (4), (8) and (12) include job seekers searching for jobs in which the share of skilled job seekers is above the third quartile. Control means display means of the dependent variable for the subset of individuals assigned to the control group. Standard errors are robust to heteroskedasticity and clustered at the LEA level.

Figure 1: The impact of the policy

Panel A



Panel B

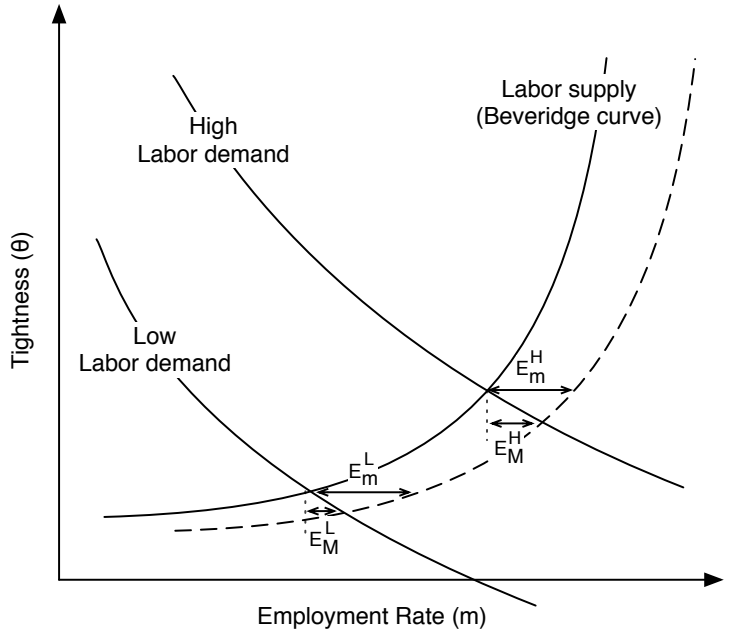
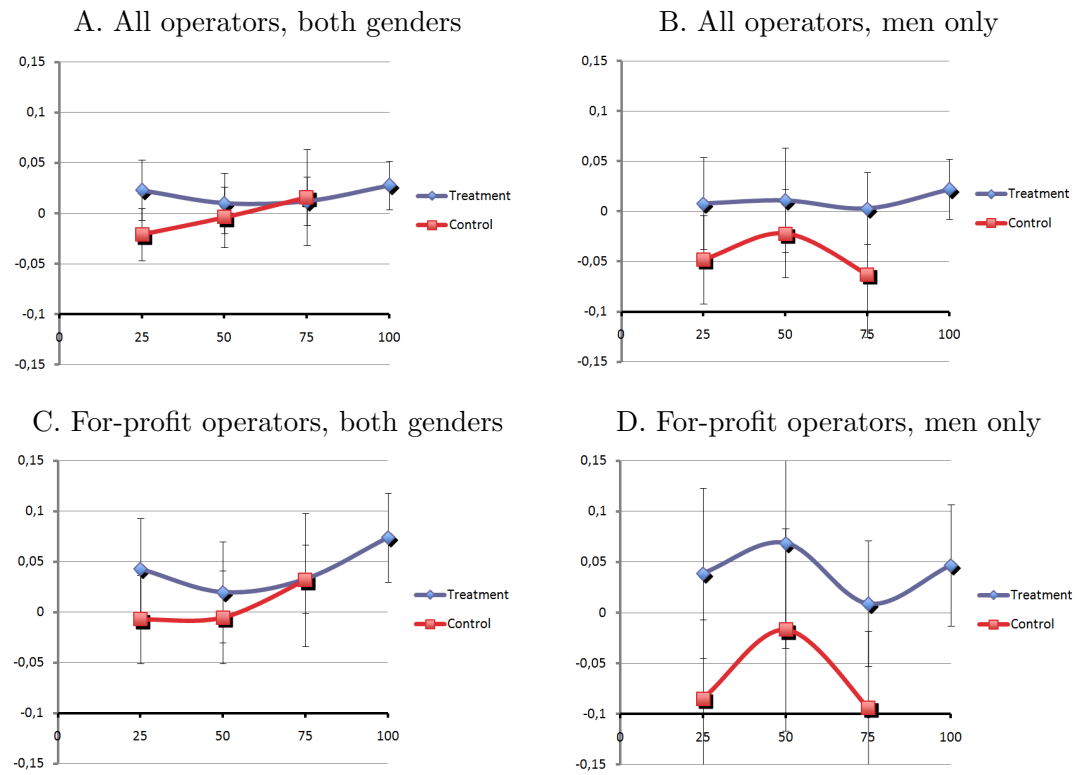


Figure 2: Disaggregated effects of the program
(Sample restricted to initially unemployed workers)



These graphs report the coefficients and standard errors from the regressions corresponding to equation (7). See notes to table (7) for additional details.

Appendix A: Interpretation of the IV estimate with externalities

Consider a simple case in which areas are randomly assigned to a probability of treatment P which is either positive or zero, and individuals in the "treatment" area are randomly assigned to the treatment. Let Z be the individual assignment variable and T the treatment status. Assume for simplicity that individuals assigned to the control group are never treated (so, in the notation of Imbens and Angrist (1994), $T(0) = 0$ and $T = T(1)Z$).

There are three potential outcomes $y(P, T)$: $y(0, 0)$ is the potential outcome when no treatment takes place in the area, $y(1, 0)$ is the potential outcome when untreated in a treatment area, and $y(1, 1)$ the potential outcome when treated.

The observed outcome is then simply:

$$y = y(0,0)(1-P)+y(1,0)P(1-T)+y(1,1)PT = y(0,0)+(y(1,0)-y(0,0))P+(y(1,1)-y(1,0))PT$$

Then, we have:

$$E(y|P, Z) = E(y(0, 0)) + E(y(1, 0) - y(0, 0))P + E(y(1, 1) - y(1, 0)|T = 1)P(T = 1)|Z)PZ.$$

What IV identifies is (1) $AE = E(y(1, 0) - y(0, 0))$, which is the average externality over the population, and (2) the "treated in treated zone" effect, $TNTT = E(y(1, 1) - y(1, 0)|T(1) = 1)$. Simple manipulations show that this parameter can be expressed as the difference between the treatment on the treated parameter (TT) and the externality on the treated (ET):

$$TNTT = E(y(1, 1) - y(0, 0)|T(1) = 1) - E(y(1, 0) - y(0, 0)|T(1) = 1) = TT - ET$$

Meanwhile, the average externality can be expressed as:

$$\begin{aligned} AE &= E(y(1, 0) - y(0, 0)|T(1) = 1)P(T(1) = 1) + E(y(1, 0) - y(0, 0)|T(1) = 0)P(T(1) = 0) \\ &= ET * P(T(1) = 1) + ENT * P(T(1) = 0) \end{aligned}$$

Under the assumption $ENT = ET(= AE)$, the TT parameter is simply the sum of AE and TNTT.

Appendix B: Supplementary tables (not for publication)

Table A.1: Take-up by individual characteristics: probit regression

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

Table A.2: Take-up by individual characteristics: probit regression (continued)

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

Source: Job seekers' register (ANPE).

Notes: The table reports the results from a probit regression of being treated on variables for individual characteristics. The regression is performed on the sample of individuals assigned to treatment.

Table A.3: Reduced form, accounting for externalities, in for-profit regions only

	All unemployed				Men (unemployed)				Women (unemployed)			
	By job type: share of job seekers who are eligible for program											
	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: fixed-term contract with a length of more than six months												
Assigned to program (β)	0.046***	0.052**	0.045**	0.117***	0.100***	0.113***	0.084*	0.176***	0.020	0.017	0.025	0.095**
	(0.016)	(0.023)	(0.021)	(0.030)	(0.029)	(0.032)	(0.044)	(0.052)	(0.018)	(0.033)	(0.025)	(0.046)
In a Program area (δ)	0.001	-0.001	-0.004	-0.077*	-0.063**	-0.060	-0.059	-0.116*	0.036	0.035	0.032	-0.060
	(0.017)	(0.028)	(0.023)	(0.041)	(0.030)	(0.039)	(0.046)	(0.065)	(0.022)	(0.038)	(0.030)	(0.057)
Net effect	0.047***	0.050**	0.041**	0.039	0.038	0.053	0.026	0.060	0.055***	0.052	0.057**	0.036
of program assignment ($\beta + \delta$)	(0.014)	(0.024)	(0.019)	(0.037)	(0.023)	(0.034)	(0.034)	(0.058)	(0.020)	(0.032)	(0.025)	(0.049)
Control mean	0.206	0.186	0.220	0.232	0.172	0.146	0.195	0.185	0.225	0.212	0.232	0.252
Panel B: Durable employment												
Assigned to program (β)	0.037*	0.071**	0.016	0.048	0.059*	0.057	0.050	0.072	0.032	0.087**	0.004	0.043
	(0.022)	(0.036)	(0.027)	(0.038)	(0.032)	(0.053)	(0.047)	(0.061)	(0.024)	(0.040)	(0.032)	(0.049)
In a Program area (δ)	0.017	0.014	-0.004	-0.024	-0.020	-0.000	-0.044	-0.057	0.027	0.018	0.016	-0.012
	(0.025)	(0.040)	(0.034)	(0.044)	(0.037)	(0.055)	(0.052)	(0.085)	(0.029)	(0.049)	(0.040)	(0.058)
Net effect	0.054**	0.084**	0.012	0.024	0.039	0.057	0.005	0.015	0.060**	0.106**	0.019	0.032
of program assignment ($\beta + \delta$)	(0.023)	(0.034)	(0.029)	(0.034)	(0.033)	(0.042)	(0.043)	(0.069)	(0.027)	(0.042)	(0.035)	(0.046)
Control mean	0.471	0.421	0.511	0.519	0.472	0.429	0.514	0.529	0.471	0.416	0.510	0.515
Panel C: Employment												
Assigned to program (β)	0.007	0.018	-0.002	0.013	0.029	-0.024	0.066	0.055	0.003	0.054	-0.027	0.002
	(0.020)	(0.028)	(0.027)	(0.029)	(0.028)	(0.045)	(0.044)	(0.062)	(0.024)	(0.033)	(0.030)	(0.040)
In a Program area (δ)	0.015	-0.001	0.013	-0.015	-0.019	0.024	-0.043	-0.073	0.022	-0.013	0.033	-0.001
	(0.022)	(0.032)	(0.029)	(0.036)	(0.037)	(0.053)	(0.049)	(0.076)	(0.030)	(0.045)	(0.039)	(0.052)
Net effect	0.022	0.018	0.011	-0.002	0.010	0.000	0.023	-0.018	0.026	0.041	0.006	0.001
of program assignment ($\beta + \delta$)	(0.019)	(0.028)	(0.023)	(0.030)	(0.033)	(0.042)	(0.042)	(0.064)	(0.028)	(0.040)	(0.035)	(0.046)
Control mean	0.634	0.604	0.657	0.672	0.626	0.620	0.631	0.672	0.638	0.594	0.670	0.672
Observations	3678	1664	1974	953	1378	669	704	315	2300	995	1270	638

Source: Job seekers' register (ANPE) and follow-up survey (DARES).

Notes: The dependent variable in Panel A is having a job with a fixed-term contract with a length of at least six months, when surveyed 8 months after random assignment; in Panel B, it is having a fixed-term contract with a length of more than six months or an indefinite-term contract, when surveyed 8 months after random assignment; in Panel C, it is having any type of job, when surveyed 8 months after random assignment. In each panel, the two first lines report OLS regressions of the dependent variable on a dummy for assigned to program and a dummy for being in a Local Employment Agency (LEA) with positive assignment probability, controlling for gender, unemployment duration (in months), unemployment duration squared, 5 dummies for diplomas and 47 dummy variables for LEA quintuplets (see equation 8). The third line reports the sum of the two previous parameters. All estimates are for individuals who reported in the the 8 month survey that they were unemployed at the date of randomization, and are restricted to regions where selected private providers are for-profit. Columns (1), (5) and (9) include job seekers searching for all kinds of jobs; columns (2), (6) and (10) include job seekers searching for jobs in which the share of skilled job seekers (κ) is below the median; columns (3), (7), and (11) include job seekers searching for jobs in which the share of skilled job seekers is above the median; columns (4), (8) and (12) include job seekers searching for jobs in which the share of skilled job seekers is above the third quartile. Control means display means of the dependent variable for the subset of individuals assigned to the control group. Standard errors are robust to heteroskedasticity and clustered at the LEA level.

Table A.4: Effect of the treatment, accounting for externalities, in for-profit regions only

	All unemployed				Men (unemployed)				Women (unemployed)			
	By job type: share of job seekers who are eligible for program											
	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: fixed-term contract with a length of more than six months												
Program participation (β)	0.120***	0.137**	0.114**	0.334***	0.262***	0.296***	0.213*	0.607***	0.051	0.044	0.064	0.256**
	(0.041)	(0.060)	(0.052)	(0.089)	(0.078)	(0.084)	(0.110)	(0.215)	(0.047)	(0.085)	(0.062)	(0.123)
In a program area (δ)	0.000	-0.002	-0.004	-0.077*	-0.064**	-0.062	-0.060	-0.106	0.035	0.035	0.031	-0.064
	(0.017)	(0.028)	(0.023)	(0.040)	(0.030)	(0.038)	(0.045)	(0.065)	(0.022)	(0.038)	(0.029)	(0.057)
Net effect	0.120***	0.135***	0.110**	0.256***	0.198***	0.234***	0.153*	0.502**	0.086**	0.079	0.096*	0.192*
of program participation ($\beta + \delta$)	(0.034)	(0.051)	(0.043)	(0.078)	(0.061)	(0.072)	(0.086)	(0.197)	(0.041)	(0.071)	(0.051)	(0.103)
Control mean	0.206	0.186	0.220	0.232	0.172	0.146	0.195	0.185	0.225	0.212	0.232	0.252
Panel B: Durable employment												
Program participation (β)	0.097*	0.187**	0.042	0.137	0.155*	0.150	0.125	0.250	0.084	0.231**	0.009	0.116
	(0.056)	(0.092)	(0.069)	(0.106)	(0.081)	(0.133)	(0.117)	(0.197)	(0.062)	(0.102)	(0.081)	(0.129)
In a program area (δ)	0.016	0.013	-0.004	-0.024	-0.021	-0.001	-0.045	-0.053	0.027	0.019	0.016	-0.014
	(0.025)	(0.039)	(0.034)	(0.044)	(0.037)	(0.054)	(0.051)	(0.080)	(0.029)	(0.049)	(0.040)	(0.058)
Net effect	0.113**	0.200**	0.037	0.113	0.134*	0.149	0.080	0.197	0.111**	0.250***	0.025	0.103
of program participation ($\beta + \delta$)	(0.049)	(0.077)	(0.058)	(0.086)	(0.069)	(0.105)	(0.096)	(0.177)	(0.054)	(0.086)	(0.068)	(0.104)
Control mean	0.471	0.421	0.511	0.519	0.472	0.429	0.514	0.529	0.471	0.416	0.510	0.515
Panel C: Employment												
Program participation (β)	0.018	0.048	-0.004	0.038	0.077	-0.062	0.166	0.191	0.009	0.143*	-0.069	0.005
	(0.052)	(0.073)	(0.068)	(0.082)	(0.070)	(0.116)	(0.109)	(0.205)	(0.062)	(0.087)	(0.075)	(0.105)
In a program area (δ)	0.015	-0.001	0.013	-0.015	-0.020	0.024	-0.044	-0.070	0.022	-0.012	0.033	-0.001
	(0.022)	(0.032)	(0.029)	(0.036)	(0.037)	(0.052)	(0.048)	(0.070)	(0.029)	(0.045)	(0.038)	(0.052)
Net effect	0.033	0.048	0.009	0.023	0.057	-0.038	0.121	0.121	0.031	0.130*	-0.035	0.004
of program participation ($\beta + \delta$)	(0.044)	(0.061)	(0.054)	(0.068)	(0.061)	(0.093)	(0.091)	(0.185)	(0.055)	(0.075)	(0.065)	(0.090)
Control mean	0.634	0.604	0.657	0.672	0.626	0.620	0.631	0.672	0.638	0.594	0.670	0.672
Observations	3678	1664	1974	953	1378	669	704	315	2300	995	1270	638

Source: Job seekers' register (ANPE) and follow-up survey (DARES).

Notes: The dependent variable in Panel A is having a job with a fixed-term contract with a length of at least six months, when surveyed 8 months after random assignment; in Panel B, it is having a fixed-term contract with a length of more than six months or an indefinite-term contract, when surveyed 8 months after random assignment; in Panel C, it is having any type of job, when surveyed 8 months after random assignment. In each panel, the two first lines report IV regressions of the dependent variable on a dummy for program participation and a dummy for being in a Local Employment Agency (LEA) with positive assignment probability, controlling for gender, unemployment duration (in months), unemployment duration squared, 5 dummies for diplomas and 47 dummy variables for LEA quintuplets. Program participation is instrumented by program assignment (see equation 9). The third line reports the sum of the two previous parameters (in practice, it is computed based on equation 10). All estimates are for individuals who reported in the the 8 month survey that they were unemployed at the date of randomization, and are restricted to regions where selected private providers are for-profit. Columns (1), (5) and (9) include job seekers searching for all kinds of jobs; columns (2), (6) and (10) include job seekers searching for jobs in which the share of skilled job seekers (κ) is below the median; columns (3), (7), and (11) include job seekers searching for jobs in which the share of skilled job seekers is above the median; columns (4), (8) and (12) include job seekers searching for jobs in which the share of skilled job seekers is above the third quartile. Control means display means of the dependent variable for the subset of individuals assigned to the control group. Standard errors are robust to heteroskedasticity and clustered at the LEA level.

Table A.5: Long-term impact on total earnings, accounting for externalities

	All unemployed				Men (unemployed)				Women (unemployed)			
	By job type: share of job seekers who are eligible for program											
	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: 8 months												
Program participation (β)	57	198**	-35	-251	286	285	326	341	-60	178***	-199	-461
	(90)	(83)	(151)	(259)	(188)	(226)	(323)	(508)	(130)	(58)	(207)	(378)
In a program area (δ)	25	-46	71	171	-31	-16	-55	-129	38	-88***	119	304*
	(43)	(35)	(70)	(114)	(80)	(87)	(155)	(206)	(54)	(30)	(85)	(169)
Net effect	82	151*	36	-80	254*	269	271	212	-22	90*	-80	-156
of program on participants ($\beta + \delta$)	(69)	(78)	(114)	(180)	(154)	(186)	(250)	(429)	(88)	(49)	(138)	(242)
Control mean	1,044	919	1,156	1,22	1,104	961	1,242	1,274	1,011	893	1,11	1,195
Observations	9890	4619	5143	2591	3716	1830	1870	843	6174	2789	3273	1748
Panel B: 12 months												
Program participation (β)	10	86	-45	-8	-23	47	-66	4	43	135*	-12	82
	(95)	(66)	(153)	(283)	(184)	(135)	(320)	(729)	(112)	(69)	(174)	(294)
In a program area (δ)	12	-50	48	172	13	-8	-15	149	8	-86*	77	135
	(49)	(45)	(74)	(115)	(90)	(64)	(165)	(278)	(52)	(52)	(74)	(119)
Net effect	22	36	3	164	-10	40	-81	153	51	49	65	217
of program on participants ($\beta + \delta$)	(79)	(66)	(116)	(212)	(144)	(130)	(218)	(514)	(89)	(66)	(137)	(245)
Control mean	1,073	942	1,19	1,225	1,155	993	1,308	1,366	1,027	911	1,127	1,16
Observations	8632	4005	4511	2300	3216	1576	1627	734	5416	2429	2884	1566

Source: Job seekers' register (ANPE) and waves 1 to 4 of the follow-up survey (DARES).

Notes: The dependent variable is the total earnings declared by all survey respondents, and includes income from transfers; in Panel A, it is measured 8 months after random assignment; in Panel B, 12 months. In each panel, the two first lines report IV regressions of the dependent variable on a dummy for program participation and a dummy for being in a Local Employment Agency (LEA) with positive assignment probability, controlling for gender, unemployment duration (in months), unemployment duration squared, 5 dummies for diplomas and 47 dummy variables for LEA quintuplets. Program participation is instrumented by program assignment (see equation 9). The third line reports the sum of the two previous parameters (in practice, it is computed based on equation 10). All estimates are for individuals who reported in the the 8 month survey that they were unemployed at the date of randomization. Columns (1), (5) and (9) include job seekers searching for all kinds of jobs; columns (2), (6) and (10) include job seekers searching for jobs in which the share of skilled job seekers (κ) is below the median; columns (3), (7), and (11) include job seekers searching for jobs in which the share of skilled job seekers is above the median; columns (4), (8) and (12) include job seekers searching for jobs in which the share of skilled job seekers is above the third quartile. Control means display means of the dependent variable for the subset of individuals assigned to the control group. Standard errors are robust to heteroskedasticity and clustered at the LEA level.

Table A.6: Long-term impact on total earnings, accounting for externalities (continued)

	All unemployed				Men (unemployed)				Women (unemployed)			
	By job type: share of job seekers who are eligible for program											
	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile	All	Below median	Above median	Above third quartile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel C: 16 months												
Program participation (β)	-185	-198	-191	-173	-251	238	-699**	-1,237**	-144	-525	84	258
	(163)	(365)	(136)	(220)	(198)	(281)	(297)	(498)	(235)	(641)	(146)	(241)
In a program area (δ)	104	173	43	96	116	25	176	539**	85	227	-2	-92
	(78)	(147)	(78)	(108)	(101)	(111)	(184)	(234)	(98)	(232)	(69)	(115)
Net effect	-81	-26	-148	-77	-135	263	-523**	-698**	-58	-297	82	166
of program on participants ($\beta + \delta$)	(107)	(236)	(118)	(171)	(157)	(205)	(229)	(323)	(156)	(427)	(135)	(237)
Control mean	1,134	1,045	1,215	1,243	1,21	1,003	1,412	1,453	1,091	1,07	1,109	1,147
Observations	8049	3704	4240	2151	2986	1445	1528	694	5063	2259	2712	1457
Panel D: 20 months												
Program participation (β)	-60	2	-94	-14	-28	151	-216	-88	-80	-50	-59	-70
	(77)	(92)	(122)	(159)	(154)	(260)	(211)	(246)	(87)	(60)	(133)	(173)
In a program area (δ)	32	19	18	-64	81	18	152	27	-2	-16	-30	-37
	(43)	(40)	(75)	(104)	(79)	(102)	(129)	(193)	(50)	(31)	(89)	(109)
Net effect	-28	22	-76	-78	54	169	-65	-61	-81	-66	-88	-108
of program on participants ($\beta + \delta$)	(66)	(86)	(100)	(143)	(138)	(202)	(176)	(264)	(69)	(50)	(114)	(136)
Control mean	1,14	1,038	1,229	1,273	1,192	1,102	1,279	1,334	1,11	999	1,202	1,244
Observations	8217	3798	4306	2203	3051	1477	1562	720	5166	2321	2744	1483

Source: Job seekers' register (ANPE) and waves 1 to 4 of the follow-up survey (DARES).

Notes: The dependent variable is the total earnings declared by all survey respondents, and includes income from transfers; in Panel C, it is measured 16 months after random assignment; in Panel D, 20 months. In each panel, the two first lines report IV regressions of the dependent variable on a dummy for program participation and a dummy for being in a Local Employment Agency (LEA) with positive assignment probability, controlling for gender, unemployment duration (in months), unemployment duration squared, 5 dummies for diplomas and 47 dummy variables for LEA quintuplets. Program participation is instrumented by program assignment (see equation 9). The third line reports the sum of the two previous parameters (in practice, it is computed based on equation 10). All estimates are for individuals who reported in the the 8 month survey that they were unemployed at the date of randomization. Columns (1), (5) and (9) include job seekers searching for all kinds of jobs; columns (2), (6) and (10) include job seekers searching for jobs in which the share of skilled job seekers (κ) is below the median; columns (3), (7), and (11) include job seekers searching for jobs in which the share of skilled job seekers is above the median; columns (4), (8) and (12) include job seekers searching for jobs in which the share of skilled job seekers is above the third quartile. Control means display means of the dependent variable for the subset of individuals assigned to the control group. Standard errors are robust to heteroskedasticity and clustered at the LEA level.