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

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Abstract

This paper reports the results of a randomized experiment designed to evaluate the impact of reinforced job placement assistance on the labor market outcomes of young, educated jobseekers 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 jobseekers to be assigned to treatment (0%, 25%, 50%, 75% or 100%) were randomly drawn for each of the 235 labor market (*e.g.* cities) participating in the experiment. Then, in each labor market, eligible jobseekers were randomly assigned to the treatment, following this proportion. After 8 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 6 months —the objective of the program— than those who were not assigned. However, among men (though not women), there is a negative impact of being in a treated labor market for eligible youth that were not assigned to the program to be in an assigned zones. This effect is stronger for those who were *ex-ante* identified as seeking jobs in sectors with a large proportion of other eligible workers. Furthermore, the employment effect disappears after 12 months and there is no impact on wages, suggesting that the programs gains for those who are 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-search assistance programs are popular in many industrialized countries[1], and a growing literature attempts to assess its effects. In these programs, an intermediary, often a private firm or a non-profit organization helps unemployed or underemployed workers write their resume, find job interviews, and prepare for those interviews. They are usually paid in full only when the worker has found a job. Unlike other Active Labor Market Policies, whose effects are globally considered as weak, most studies tend to find a significant and positive impact of this form of counseling, especially for jobseekers with low risks of long-duration unemployment (see reviews in Kluve, 2006; Card, Kluve, and Weber, 2010).

These studies are generally based on the comparison of short-run labor market outcomes between counseled and non-counseled jobseekers.[2] Experimental studies are still relatively rare but their estimates also tend to be positive (Rosholm, 2008; Behaghel, Crépon, and Gurgand, 2010).[3] However, an important criticism leveled against these studies (experimental or not) 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 likely 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.

The presence and the magnitude of these displacement effects are key policy questions. If all a policy does is to lead to a musical chair game between unemployed workers, then the estimated impacts are over-estimates of its welfare implications, for two reasons. First, the treatment effect is biased upwards when we compare a treated worker to a non-treated worker in the 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) call a policy a success if it *only* negatively affected the control workers. Second,

[1] They are particularly developed in Northern Europe. For instance they represent resp. 0.19%, 0.24%, 0.34% of GDP in Denmark, Sweden, Germany (Source: OECD Labour Market Program database). In France, expenditures in placement employment 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 their pattern (for example, in what type of labor market conditions are they strong or weak) can shed important light on how labor markets function.

This issue has long been a concern for economists and policy makers. For example, Johnson (1979), Atkinson (1987), Meyer (1995) point out that displacement effect would alter effects of training or unemployment insurance policies. Heckman, Lochner, and Taber (1999) make a similar argument in the case of a tax and tuition subsidy policy and point out that the general equilibrium effect of such policies would be lower than the partial equilibrium effects, because it would lower the returns to education. Based on a calibrated job-search model Davidson and Woodbury (1993) find that workers who receive a reemployment bonus displace non-eligible workers. Improving on this approach, Lise, Seitz, and Smith (2004) develop a framework designed to evaluate the potential magnitude of the global impact of the Canadian Self Sufficiency Program. They compare the results of a randomized experiment designed to evaluate partial equilibrium effects of this program with what is obtained in a structural model taking into account equilibrium effects and calibrated on the same experimental data. They show that the externalities may cancel the apparent positive effects and could even lead to negative global effects. They do not directly measure these externalities, however. Close to our framework, Van der Linden (2005) derive equilibrium effect associated to counseling programs in a matching model of the labor market. Equilibrium effects in his model come from wage bargaining. In such a case counseled jobseekers's reservation wage would increase and the demand for labor would decrease outweighing partially 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 spill-over effects of school-level deworming programs. Finkelstein (2007) exploits the geographic variability of insurance coverage to compute the effects of Medicare on health consumption, and find 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 youngsters of 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 on on-line market place, and find very little displacement effect. In their very recent contribution, Gautier, Muller, van der Klaauw, Rosholm, and Svarer (2011) use the results of a Danish randomized experiment of a job search assistance program organized in 2005 in two counties. Comparing control individuals in experimental counties to jobseekers in some similar non-participating counties, they find hints of substantial negative externalities of the treatment.

[Other references Cahuc, Crépon, Guitard, and Gurgand (2008)]

One potential issue with all these studies (except Duflo and Saez, 2003) 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 not lead to unbiased estimates of the equilibrium effects. To address this issue, we implement a two-step randomized design in evaluating a large-scale job seeker assistance programs in France, targeted to young, educated job seekers. Under the program, private providers are contracted to provide reinforced placement services for young graduates (with at least a two-year college degree) who have been unemployed for at least 6 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 6 months.

One of the main innovations of this experiment is the two-level randomization. In the first step, each one of 235 local employment areas (in ten regions of France, covering about half of the territory) was randomly assigned the proportion P of jobseekers 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 jobseekers (young, educated, unemployed or under-employed for 6 months or more) was randomly selected to be assigned to treatment. Those assigned to treatment were offered the opportunity to enroll in the program

(about a third of them 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 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 and 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 are 2.4 percentage points (12%) more likely to have a durable fixed term contract of more than 6 months. Within treated areas, those assigned to treatment were 2.8 percentage points more likely to have such contract than those who were not assigned. These differences are larger for men than for women, and larger when agency that won the contract was a for profit firm than when it was a non-for profit organization.

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

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

To the extent that the beneficiaries of the program took jobs that other workers (older, or less educated, or unemployed for a shorter time) also competed for, the externalities may not be limited to the eligible youths: in fact, they may be low on eligible youths, because they are shared among a larger groups of unemployed workers. To investigate this issue, we split the sample according to whether the type of job sought by an individual is a job where he must compete with many other educated youth. Each jobseeker must indicate the type of job she is looking for when she is registering at the ANPE: they select one or two of 466 different jobs. We focus on the job they selected as their first choice. For each of these jobs, we compute the share of educated youth among all young eligible job seekers (using the universe of job seekers at the national level). We find that, for men, in sectors where the fraction of educated youth is above median among young job seekers (for example, project manager, laboratory technician or financial officer), there are clear evidence of stronger externalities of the treatment on the untreated eligible: in those sectors, after 8 months, the unemployed eligible who were not assigned to treatment are 4.9 percentage points (25%) less likely to find a job with a long term contract if they live in an area where some were treated, while in sectors below median (for example cooking assistant, construction vehicle driver or motorcycle mechanic) the untreated eligible are only 2.4 percentage points (p-value 0.30) less likely to find a job in treated areas.

These estimates imply that, at least for men, the program's benefits would have been greatly overstated in a standard program evaluation, where individuals would have been randomly assigned within specific site (for example as in Dolton and O'Neill (1996),in Van den Berg and Van der Klaauw (2006), etc.): the within comparison suggest a treatment effect on the treated, of 11.6 percentage point (and, by assumption rules out negative externalities) when our estimates suggest a 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 (who include those not assigned, those who were assigned but did not take up the program). Furthermore there is the negative externalities on those who were not eligible. Taking all those effect into account, 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 were due to an improvement in the search ability of some workers, to the detriment of others, implying that neither labor demand nor wages fully adjust in the short run. Indeed other pieces of evidence

suggest that the main effect of the program may have been to help those treated to find a job a little faster, at the expense of others who found one later: After 12 months (and up to 20 months), the program effects (both displacement and externality) on durable employment (or any form of unemployment) had entirely disappeared, both for men and for women. The program 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 developped by Landais, Michaillat, and Saez (2010) to analyze the design of unemployment insurance. This is a search models with two realistic features: diminishing returns, and a wage structure that does not fully adjust. The model has the additional prediction that the externalities should be stronger in more slack labor market conditions (in a recession for example). We test this additional prediction, by comparing the size of the externalities for cohorts affected by the 2008 recessions and others, for labor markets were unemployment levels are higher. Indeed, we find that externalities are stronger in recession, stronger in labor markets where unemployment levels are higher, and, in a difference in differences specification, strongest in weak labor markets in bad times. This provides empirical support for this model of the labor market.

The program and the institutional context are described in the next section. Section 3 gives details of the experimental design and the data. Section 4 proposes a simple conceptual framework. Section 5 presents the empirical strategy. Section 6 is devoted to 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, the monopoly of placement services. In particular, employers were legally obligated to communicate their vacancies to ANPE.[5] In 2005, the Social Cohesion Law broke this virtual monopoly. In particular, temporary work agencies were allowed to openly propose their counseling and placement services to any jobseeker. The public operator (which was renamed

[5]Some segments of unemployed were guided by others networks: *e.g.* APEC (*Agence Pour l'Emploi des Cadres*) specialized in placement for executives and managers or *Missions Locales* for unskilled youth.

Pole Emploi in 2008) has nevertheless retained a prominent role, because every unemployment insurance (UI) recipient must meet her ANPE caseworker at least once per month and follow her prescriptions, in order to remain eligible for benefits.

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

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

Young graduates are, in France like in the US, of particular concern for policy makers. In 2007, three years after they completed their studies, only 68%-75% of college graduates had a stable job. Reports (Commission Hetzel, 2005) emphasized the lack of any job market experience among young graduates from the universities (internships or summer jobs are rare) and recommended introducing specialized counseling services for this segment of job seekers. In 2007, the Ministry of Labor thus decided to experiment with subcontracting private providers of placement services for young graduates that had been unemployed or underemployed for 6 months or more. Private providers, in particular temp agencies, were believed to be more efficient at finding jobs for young graduates than the ANPE, due to their 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 their price. Six regions were delegated to for-profit operators, among which five were subsidiaries of temp agencies. In four regions, not-for-profit organizations were selected. For example, one of them is a social and solidarity-oriented training center. The others are local agencies of a non-for-profit youth guidance organization.[6]

The program can be broken down in two main phases:

- Phase I aims to place jobseekers in employment. 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 contract must be either a "CDI" (illimited) or a "CDD" (fixed term contracts, renewable) of six months or more, CDD are more frequent).

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

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

- Enrollment: when a job seeker is enrolled in the program, the private agency receives the first payment (25% of the total sum)

- 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%)

[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 nonprofits were selected are: Centre, Nord-Pas-de-Calais, Provence-Alpes-Cote d'Azur, Rhône-Alpes.

- Remaining employed after six months: six months after the entry in a durable job, the third payement is given to counseling company if the former jobseeker is still employed (35%).

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

The program, like many others of this type, mainly focused on helping the beneficiaries identify likely job prospect and prepare themselves for interviews. As will be shown in more details below, programs beneficiaries met a counselor more often, were helped to prepare a resume, communicate with employers and assess their skills, and 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 two levels: labor market area, and individuals within market.

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

In order to improve precision, we first formed groups of five agencies that are similar in size and characteristics of the local population: we obtain 47 quintuplets. Within each quintuplets, each agency was randomly assigned a proportion of treated among the population of eligible youths $P = 0, 0.25, 0.50, 0.75, 1$.

Individuals were then randomized at the individual level, following the proportion drawn in the first stage. Every month from September 2007 to November 2008, the youth that had entered the target population (those below 30, with at least a two-years college degree, having spent either or 12 out of the last 18 months, or 6 months continuously unemployed or underemployed)

were identified in each agency. The list was transmitted to us and we randomized the relevant proportion independently in each agency. The list of individuals thus selected to be potential beneficiaries of the program was then passed on to the relevant counseling firm for the area, who was in charge of contacting the youth and offer her entry into the program. Entry was voluntary, and the youth could elect to continue receiving services from the regular local agency instead. No youth from the control group could be approached by the firm at any time.

Fourteen consecutive cohorts of youth were transmitted to us for assignment, covering a little over 57,000 youths in total. About 10,000 of the youth in fact lived in areas that were not part of the experiment, and are not included in this study. Out of remainder, we only collected data on labor market outcomes for cohorts 3 to 12, who are therefore the focus of this paper.[7] Overall, 30,343 individuals were part of the experimental cohorts and regions, out of which which 29,636 were randomly sampled to be surveyed.[8]

3.2 Data

There are three sources of data for this experiment. First, we have administrative files with the list of jobseekers reaching the eligibility conditions were provided by ANPE to the Ministry of Labor. This database contains age, number of months spent unemployed during the current spell, id of the public employment agency, postal address, as well as the kind of job the person is searching. It is not updated in real time and is not necessary completely current.

A second set of data comes from the administrative files transmitted by private counseling firms. In order to claim payment, the firms would send the lists of jobseekers who actually entered the counseling scheme. Acceptance was conditional upon the jobseeker filling and signing a form. Copies of these forms were returned and checked to ensure that firms were not overdeclaring the

[7]Data collection was focused on these cohorts for several reasons. First, cohorts 1 and 2 were given up as it appeared that it took a couple of weeks to the private operators to be ready to actually offer the treatment. Cohorts 13 and 14 were not followed either because, at the same time, the Ministry opened a profitable call for tender for jobseeker counseling. Anecdotal evidence suggested at the time that private firms were more focused on this second operation and stopped implementing the first program; youths were not enrolled even when they were officially selected for treatment.

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

number of jobseekers they were actually counseling. We use this dataset to measure the take-up of the program.

Finally, the main source of information is a series of four endline surveys conducted by the ministry of labor. This survey was necessary due to impossibility of using existing administrative data in this context. Administrative data obtained from the unemployment office is not a good measure of unemployment: all it records is whether the 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. Young job seekers don't have a strong incentives to be registered at the ANPE, in particular because they are often not eligible to unemployment benefits. Indeed, as we describe in more details below, the baseline data that we used to assign people to 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 protection.

The 29,636 jobseekers sampled for the surveys were surveyed four times: 8 months after random assignment, then 12 months, 16 months and 20 months after. The survey was conduced by DARES, the research department of 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 collection costs and to increase the response rate, the survey was short (10 minutes for the first wave, 5 minutes for the others). Moreover, the survey combined three collection modes: internet, telephone, and paper questionnaire. As a result, response rates were high: as shown in table 1, 25,904 people (87%) answered to at least one of these waves and 23,320 (79%) answered at least to the first one.

Table 1 also shows the response rate conditional on having been assigned to treatment or control group. For every wave, the response is above 70%, and the jobseekers assigned to treatment are only 1 percentage point more likely to answer than the ones assigned to control (this difference remains constant across wave).

The first 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 employment situation at the time of the survey (wage, type of contract, part time or not, occupation) and at the time of program assignment, highest degree obtained, family situation, as well individual

nationality or parents' nationality are also obtained. We also asked how many times they met a counselor (regular, or from the private agency) and what kind of help they got during their job search. Finally, individuals assigned to treatment were asked the reasons why they thought they would benefit from entering the program (if they accepted) or for those who refused, why they refused.

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

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

Table 3 presents summary statistics on the employment situation at the time of the survey, according to the report in the wave one survey. Importantly, 44% of the sample declared to have been on employment at the time of assignment to the treatment. There can be two reasons for this. First, they could have recently found a job, and their status may not have been updated in the unemployment agency list that formed the randomization sample. Second, they could be underemployed, *i.e.* hold a part-time job but still be looking for full-time employment, and thus

remain eligible for treatment (a status known as "activité réduite", or reduced activity). In what follows, we will mostly focus on the results for the unemployed workers because they are more likely to actually be treated, and they are the actual target of the policy. However, because a proportion (23%) of the workers reporting to have been employed at the time of assignment were still treated, we will show the impact in this population.

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 in what conditions a job search assistance program like this one may create externalities.

We consider a model with one sector, and one type of workers (the model can easily be extended to include un-skilled workers, which can be more or less substitutable with the skilled workers, and search either on the same or 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 standard in job search models, denote $M(V, U_e)$ the matching function, which links the number of matches to search efforts and 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, and the probability of a match for a worker who exert search effort 1 is $\theta q(\theta) = M(\theta, 1)$. The usual assumptions about the matching function is that it is a constant return to scale function increasing and concave in both V and U_e Pissarides (2000). The job finding probability is thus increasing with θ, and concave.

To model the impact of the program, assume for simplicity that everyone exert search effort 1.[9] When they become unemployed, a fraction of the unemployed are assigned at rate π to a

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

group benefitting 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) the not counselled (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 both groups are:

$$U_{ec} = eU_c$$

$$U_{en} = U_n,$$

where U_c (U_n) is the number of unemployed who are counselled (not counselled). 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 $eU_c/U_e M(V, U_e) = eU_c \theta q(\theta)$ job offers, which leads to the following exit rate for counseled individuals

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

Similarly exit rate of non counseled individuals is

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

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

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

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

[10]The assignment is a function of (1) eligibility (2) randomization and (3) whether or not they chose to participate.

The resulting, $\theta = \theta_B(m)$ is an increasing function of n. Figure 1 draws the labor supply equation in the tightness/employment rate space. This is the equivalent of the Beveridge curve (conventionally represented in the unemployment-vacancy space). Notice that the curve is fairly flat for low level 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 return to scale assumption of 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 because some factor (management, fixed capital, or another input such as unqualified workers) is fixed in the short run.

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

To present the argument in the simplest way, assume that total operating cost for a job c (for example because everybody at entry level is paid a binding minimum or negociated wage). This is a stark simplification of Michaillat (2012) and Landais, Michaillat, and Saez (2010) framework: they show that the results below obtain as long as there is some rigidity in salary, for example downward wage rigidity, not fully fixed wages. The firm chooses employment to maximize the value of output, minus operating costs, minus the cost of hiring. 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 [11]

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

where c is total variable labor cost. 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 mapping 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. The policy leads to a decrease of $\pi/e+1-\pi$. Thus the Beveridge curve shifts to the right while the labor demand curve remains unchanged.

[11] This equation comes from first 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 $p = \beta L^{\beta-1}$ is the marginal product related to a new hire and second from the entry condition that the value of having a vacancy is zero.

Clearly, this leads to an increase in employment and an increase θ, and thus there will be displacement effects. Notice that if we had constant return to scale 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 be flat. Conversely, in a pure "rate" race model, the labor demand curve would be vertical, and there would be no employment effect of a policy, just a tightening of the labor market. The gains for the beneficiaries would be entirely undone by loss for the 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 (ϵ_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 (ϵ_M), which represents the net increase in employment, as represented on the graph.

Thus, the model predicts that there will be both direct employment effect on the beneficiaries, but also externalities on the non-beneficiaries, as long as the labor demand is neither completely flat (which will be the case as soon as there is a limiting factor, such as capital or management). The 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). The steeper the labor demand, the bigger the externality. Furthermore, the model has an additional testable prediction, due to the shape of the labor supply curve, which is explored in details (and proven) in Landais, Michaillat, and Saez (2010) and forms the core of their argument that unemployment insurance should be higher in recession. This prediction is illustrated in figure 1. 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): 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 gains for the treated workers are that they get ahead in the "rate race". If demand is high (right part of the graph), an increase in the productivity of the search effort has much larger net employment effect, and smaller externalities.

5 Empirical strategy

5.1 Naive estimates: difference between beneficiaries and non beneficiaries

A first step is to establish whether or not the program improved the probability that its intended beneficiaries get a job, compared to other who did not benefit. These estimates are "naive" estimates of the impact of the program because they ignore externalities. But they can give us a first sense of whether the program had any effect, and what a traditional estimate of the program would have concluded.

The randomization took place into steps, both within strata (quintuplet) and within each city (or employment zone corresponding to each 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 city they were drawn from.

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

Where 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.* the obtention of a durable job. To help understand the nature of the program, we will also estimate a similar equation for the services received by the youth while unemployed. T is a dummy equal to 1 if the individual enrolls into the program. X_{ic} is a vector of control variables which include a set of quintuplets dummy, a dummy for entry date 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, randomized purely within sites in a few sites selected for convenience would typically not include cities with just treatment or just control variables. Each city is then a strata, and the strata would be controlled for:[12]

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

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

5.2 Estimating Externalities

5.2.1 Unconstrained reduced form

To estimate externalities, we take advantage of the fact that the fraction of treatment workers vary by labor markets (from 0% to 100%). Negative externalities have two simple implications. First, the probability of eligible youth the control group to find a job should be lower in cities where others got assigned. Second, the treatment effect should be lower as the fraction of workers assigned to the program is larger (as the treated workers now reassign jobs between themselves). 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:

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

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

where Z_{ic} is the assignment to treatment variable and P_{xc} is the 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...) and the set of the 47 dummy variables for each quintuplets of cities (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 the super control where no one was assigned to treatment. Coefficient δ_x measures the effect of being assigned to control in an area where x% of the eligible population was assigned to treatment compared to the super control where no one was assigned to treatment. Note that there are four parameters β but only three parameters δ as there is no room to estimate the effect on non assigned when the whole eligible population is assigned to the program.

There are three tests that will indicate the presence of externalities in this regression (1) all the δ coefficients are jointly zero; (2) they are equal to each other; and (3) 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 assignment), pools all those who were assigned to control in a zone were some were treated, and all of those who were assigned to treatment. This regression does not allow us to estimate the slope with respect to share treated, but has more power against the null that there are no externalities.

The reduced form specification is:

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

where P_c is a dummy for any treatment area (*i.e.* with positive proportion of treated). In this specification, β_5 is the difference between those assigned to treatment (treated or not), and those who are in treatment zones but are not assigned themselves. δ_5 is the effect of being untreated in a treated 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 inflicted by a treated worker is the same on any other worker, regardless of 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 estimate the overall effect of the treated on the treated, compared to those in the super-control, as β_7 by estimating the following IV equation using the treatment assignment as an instrument:

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

The model suggests that the size of the externality on any given worker will depend on the fraction of workers in the market that are not assigned. This depends on the fraction of those who are eligible who are assigned, but also on the fraction of the overall labor market that is eligible: Any labor market externalities due to the treatment may not affect only the eligible group, but may also affect workers who are close substitutes to them, although they are not part of the experiment. For example, the young educated 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 on other eligible workers because they represent too small a share of the real pool of workers they are competing with. For instance, our target population represents 25% of all young workers (under 30) with more than 6 months

unemployment (21% among men and 28% among women); similarly, it 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 workers would not necessarily help: the power of the experiment would simply be too low to detect anything if the eligible workers are a small part of the overall pool.

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 the education requirement. When they first register at the ANPE, job-seekers indicate the type 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-seeker (at least 2-year college degree) among all job-seekers under 30 years old and past unemployment duration greater than 6 months. We call this fraction κ. Table 4 gives a list of the 10 categories where the skilled are most numerous (high κ) and the 10 categories where they are the least numerous (low κ) and their share. Low values of κ are found for industrial jobs that require vocational education (often at below college level), such as construction workers (roofer, concrete worker, sheet fitter). The highest concentration of young job-seekers with at least 2-years college degree is found for qualified, mostly tertiary jobs, such as lawyer, financial officer, lecturer or psychologist.

In the categories where the educated are least numerous among those looking for a job, they are competing with a larger pool of workers (all the 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 categories with a high share of skilled workers than in those 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 median, above 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 is 80%. Our target workers represent 10% of the young job seekers with past unemployment duration greater then 6 months in below median κ sectors, 72% in above median κ sectors, and 76% in the third quartile.

Finally, the model has the testable implication that externalities should be stronger in re-

cessions or in weak labor markets. To test this hypothesis, we run three additional regressions:

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

where LLD_i is a dummy equal to 1 for a cohort that faces a very weak job market (we pick the cohorts that became unemployed from october 2007 to july 2008, and hence were looking for employment during the worst of the crisis), HLD_i is a dummy equal to 1 for other cohorts.

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

where LLD_c is a dummy equal to 1 in regions with more unemployment, and HLD_c is a dummy equal to 1 in region with less unemployment.

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 the search effort may have changed over time (as operators became better at it, or on the contrary lost interest), or operators who bid in weak labor markets may be different than those who bid in strong labor market. The strongest test will thus come from a "difference in difference" specification, where the externality is interacted with the fully interacted set of weak/strong labor market cohorts and weak/strong labor market regions.

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

In this specification, the testable implication of the theory is that δ_9^{LL} is significantly different (more negative) than all the other δ coefficients.

6 Results

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

Panel A in table 5 presents impact of assignment to treatment on participation to the program. Not surprisingly, the participation the control group was essentially zero, but take up in the

treatment group was far from universal: it was only 35% on average for all workers. Not surprisingly, it is higher for unemployed workers than for employed workers: among the unemployed, it reaches 44% (versus 25% for the employed). The follow up survey asked for reasons not to participate. 46% of those assigned to treatment who did participate reported that they already had or were about to start a job. 11% claimed that they were studying. Only about 17% of them answered that they felt that the counseling program was useless or time consuming.

Appendix tables A.1 and A.2 present the characteristics that are associated to the probability to take up, from a probit regression. Some individual characteristics are strongly correlated with take up: gender –males were more likely to participate–, education – those with education level were less likely to participate–, unemployment benefits – those receiving unemployment benefits were more likely to participate. The participation rate was very stable across cohorts, and was similar in areas were more people were 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 the jobseekers (according to their self report at the endline interview). Overall, as we can see in panel C, participants had 1.6 (50%) more meetings with an job search advisor (over the 8 months after assignment) and received more help preparing their resume and assessing their skills. They were not significantly more likely to have been put in touch with specific employer, nor did they receive help with transport to interviews. Overall, the program may have helped them form more realistic expectation of the jobs they could aspire to, 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 equation 3 and 4, the main labor market outcomes targeted by the program (fixed term contract for at least 6 months) are presented in table 6.

Panel A1 shows the reduced form "impact", ignoring externalities: all those assigned to treatment are compared to all those assigned to control. Panel A2 includes labor market (city) dummies, and thus compares treated and control young jobseekers within a given labor market. A test of equality tests the hypothesis that coefficients in panel A1 and A2 are equal.

Overall, assigned workers are 1 percentage point more likely to have obtained a fixed term contract of at least 6 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 than if they were not. In contrast, and not surprisingly, there is no impact at all for those who were actually employed at the time of assignment (columns 5 and 6). Recall that, depending on the region, the contracts were awarded either to for profit or non-profit operators. The for-profit operators drive most the difference, and the effect in those regions is about twice that in the sample as a whole: 2.3 percentage point in the sample as a whole, 4.6 percentage point (23%) for the unemployed. The differences are systematically larger for men than for women (for example, 7.3 percentage points –or 36%– for unemployed men in for profit regions, versus 3.1 percentage points for women). As we see in panel B, these differences translate into relatively large IV estimates: 5.5 percentage points for unemployed workers, 7.4 percentage points for unemployed men, and up to 18 percentage point (94%) for unemployed men in for profit regions.

As we noted, these estimates are potentially biased estimate of the real 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 estimate in A2 (which combine within and across city comparison) to be larger than the estimate in A1. In practice, while the estimate 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 ALE dummies: this could be an indication that a more precise test will detect significant externalities.

6.3 Externalities

6.3.1 Whole sample: Effect on the probability to find a durable fixed term contract

Table 7 presents estimates of equation 7. Figure 2 is a graphical representation of the coefficients for the unemployed workers (men and women all operators, and just the for profit operators; men only, all operators and just the for profit operators). For all workers (or all unemployed taken 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. They are also not statistically different from each other, and there is no pattern of them decreasing, even for subgroups where the direct effects are large (see figure 2, for example panel A for unemployed workers). Second, the effect of the treatment on those assigned does not appear to decline with the fraction assigned. If anything, the treatment effect follows a U-shape in assignment rate, with the group with 100% assignment rate tends to have the largest treatment effect (see figure 2). This hides 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 estimate of equation (8), which summarizes these results by estimating an average externality, regardless of the fraction of workers assigned. In each panel, the first row present coefficient β_5 (the impact of being assigned compared to not being assigned in a treatment zone, 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 someone in an untreated zone.

Columns 1, 4 and 8 present the results for the whole sample (everyone, men, and women). 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 point more likely to find a job with long term contracts than those who are not assigned and live in treated areas, but the unassigned in treatment zones are 3.9 percentage point *less* likely to find one. Overall, men assigned to treatment are actually not significantly more likely to find a job than those in the super control. Interestingly, since there are no negative effect for women or being in a treatment zone, the next effect for a woman of being assigned to the program (compared to someone in the super control) is actually 2.4 percentage point, twice the difference for men.

Table 9 presents the estimates of the parameters of interest in the IV specification (equations 9 and 10). In each panel, the first row is the estimate of the difference in outcome between treated and untreated workers within the same zone (β_6), the second row is the effect of being untreated in a treatment labor market (δ_6) and the third row is the difference between a treated worker and

a worker in a untreated zone (β_7). This does not change our estimate of externalities compared to the reduced from specification.

Among men, there is a negative externality, and a large difference between those who are in fact treated and the untreated in the treatment zones (11 percentage point, almost a doubling of the probability to find this kind of a job for the untreated in these markets). The net effect of being treated on the probability to find a contract with a durable fixed term is 7.4 percentage points (43%) for men . This tables highlight that the lack of significant effect in the reduced form of being assigned to treatment for men is due to the fact that the assigned, but untreated, were hurt by the negative externalities. For women, there is a smaller difference between the treated and the non treated in treated zones, but no effect of being untreated in the treatment zone. 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) and (7) and (9) and (10) split the sample according to κ, the fraction of educated workers among young job seekers. The results confirm the conclusion that externalities appear to be important for men, but not for women. For men, while the negative effect of being untreated in a treatment zone is 3.9 percentage points for the sample as a whole, -4.9 percentage points (26%) for κ above median, and -10.6% (50%) for κ in the third quartile. These results suggest that, in sectors where men are competing mainly with other men in the sample, the externalities are high. In sectors where they are competing with a smaller pool of men, they *appear* to be smaller, but this is because they are diluted over a larger group. For women, in contrast there are no significant negative effect of being in a treated zone and untreated, even in very sectors with high κ.[13]

This table suggests that, as expected, the externalities depend on the fraction of workers in the relevant labor market who are effectively treated, which is what we expect from the model, but not what we found in table 7. A possible explanation the lack of impact of the fraction of workers assigned is lack of power: assuming that unskilled and skilled workers are substitutes within a particular occupation, the effective fraction assigned is $\kappa * P * r$ where r is the fraction of treated. For unemployed men, the take up was 45%, and the average κ is 54%. Thus, increasing

[13]Defining κ separately for women and for men produce very similar results.

the fraction assigned from 25% to 75% only increased the fraction treated from 7% to 18%. In contrast, κ is 10% below median and 76% in the third quartile. Thus, for the average treatment assignment of 50%, going from κ below median to κ in the third quartile increase the the fraction treated from 2% to 18%.

Appendix table A.3 present the same results, focusing on regions where the contracts was 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 as for the sample as a whole). In practice, the effects were entirely driven by for profit operators, with the not-for profit operators being essentially ineffective (no direct effect, and no externalities). The conclusion that there are no negative externalities of being in a treatment zone for women, and smaller treatment effects 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 crisis 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 or low unemployment. The pattern is the same, with externalities being significant only in weak labor markets (although the difference in externalities between weak and strong market is not always significant). Finally, panel C presents the difference in difference specification: it shows that externalities are indeed the weakest for the affected cohorts and weak markets, suggesting that the difference in the pattern of externalities are not likely to be driven by cohort effects or labor market conditions that are unrelated to labor market conditions.

6.5 Other labor employment outcomes

Until now, we have focused on the main variable of interest in the program, the obtention of a durable fixed term employment contract (fixed term contracts of 6 months or more), which

was the objective set to the operators, and hence the variable where we should see the largest treatment effect. This is not the only variable of interest, however. In fact, two variables may be more directly relevant from a policy point of view. One the one hand, did the policy led to an increase in the probability to obtain any durable job (fixed term contracts over 6 months or indefinite term contracts), or just fixed term contracts? And on the other hand, did it increase the probability to get any job at all, or just increases the stability of the jobs people got?

Panel B and C of tables 8 and 9 present the reduced form and IV estimates of assignment (or treatment) effects and the externalities. For long term employment (panel B), taking all workers together, the point estimates are a bit smaller (though in the same range) as for durable fixed term contracts: 4.2 percentage point for all workers, similar for men (4.5 percentage points) and women (4.4 percentage points). The evidence on externalities is consistent with the results on durable fixed terms. The point estimates of the dummy for being untreated in a treatment zones are almost the same, although noisier. As before, we do find much higher direct treatment effect estimate in job with low κ, particularly for men.

For employment of any kind (panel C), the point estimates are a little smaller, and usually insignificant (except in low κ sectors), in part because the probability of finding a job is larger in the control group (around 63% of workers who were assigned to the control group and were initially unemployed have found a job 8 months later). The contrast between the effects on durable fixed terms contract and employment is even starker for the for-profit operators (see appendix tables A.3 and A.4): for them there are, as we saw, large treatment effects on durable fixed term contracts (and durable employment), but the effect on employment are small and insignificant. This suggests that operators were effective at tailoring exactly the type of employment relationships that would ensure they get their payment, and essentially converted short term contract into slightly longer term jobs.

6.6 Long term effects: employment and wages

Another important outcome of the program is whether any effect persists past 8 months. The incentive in the contract was to help the beneficiaries find a job within a maximum 6 months, so the direct effect of the program are expected to disappear at 12 months and beyond. But a key rationale for such policies is the idea that the first job provides a "stepping stone", helping

the youth find further employment after the first contract ends (or transforming a 6 months contract at a firm into a more permanent position). To investigate this question, we conducted surveys at 12, 16 and 20 months. Table 11 provides the IV results[14].

We focus on the "durable employment" outcome in this table, but the results on durable fixed term employment 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 zone). The probability to find a durable job steadily increases in the control group (from 48% at 8 months to 64% at 20 months), and the all the groups converge.

Tables 12 present the results for wages for those employed. This is a selected outcome, since there is an employment effect. Appendix table and A.5 present the impact of total earnings (including zero for those who earn nothing, or unemployment benefits for those who get them), at 8 months and beyond. The effects on wages could have been positive or negative, in the short or long run: they could have been negative if the worker was encouraged to hurry to take a less good job, rather than wait for something better. It could have been positive if the programs helped them find a better job match. Overall, however, there appear 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 a job seeker's assistance program for young graduates offers a unique opportunity to analyze both the direct impact and the equilibrium effects of counseling a certain proportion of jobseekers in a given market.

We find that the reinforced counseling program does indeed have a positive impact on the employment situation of young jobseekers 8 months after assignment into treatment. This effect is essentially limited to for profit operators, who may have had better contacts with prospective employers. Among men, however, these effects come partly at the expenses of those who were not treated but are searching for a job in the same labor market, which lowers the overall program effects. Indeed, although the program is beneficial for those treated, because a little

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

less than half of those eligible actually took up the offer (even among the unemployed workers), the overall impact of being offered the chance to be treated turns out to be insignificant for men. Indeed, in our set up, we calculate that the exernalities are large enough, and the take up was low enough, that the of extra jobs obtained by those treated was only about twice as large as the number of jobs that were lost by eligible workers who did not benefit (and this does not take into account the externalities on the ineligible workers). An open question that remains is why the externalities were stronger for men than for women.

These externalities suggest that part of the program effect in the short run were due to an improvement in the search ability of some workers, to the detriment of others, implying that neither labor demand nor wages fully adjust 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: the externalities are strongest in weak labor market. Indeed other pieces of evidence suggest that the main effect of the program may have been to help those treated to find a job a little faster, at the expense of others who found one later. In particular, after 12 months (and up to 20 months), the program effects (both displacement and externality) on durable employment (or any form of unemployment) 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 this one probably need to be tempered. They also imply that there potentially important externalities to search effort 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)
	Number of	Response rate			
Wave	answers	Total	Assigned to control	Assigned to 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: Jobseekers' register (ANPE) and experiment survey (DARES).

Notes: Column 1 reports the total number of answers for the experiment surveys at 8, 12, 16 and 20 months over the total of 29,636 sampled individuals. Columns 2, 3 and 4 report the response rate respectively in the whole sample, the assigned-to-treatment group and the control group. 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 recepient	0.310	0.301	0.316	0.011	0.124
Non benefit recepient	0.690	0.699	0.684	-0.011	0.124
Gender					
Female	0.635	0.643	0.628	-0.021	0.007
Male	0.365	0.357	0.372	0.021	0.007
Highest degree					
PhD	0.012	0.013	0.011	-0.001	0.566
Master'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: Jobseekers' register (ANPE).

Notes: These summary statistics are based on the sample of individuals that were sampled for the survey, whether they responded or not to the experiment survey. Columns 1, 2 and 3 report the means of individual characteristics respectively in the whole sample, the assigned-to-treatment group and the control group. 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

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

Source: Experiment survey (DARES).

Notes: These summary statistics are based on the sample of individuals who responded to the first wave, 8 months after random assignment, of the experiment survey. Occupation at randomization is measured by the answer, in the experiment survey, to the question "On XX/XX/07, did you have a job?", where XX/XX/07 is the date of random assignment of the individual. Columns 1, 2 and 3 report the means of individual characteristics respectively in the whole sample, the assigned-to-treatment group and the control group. Column 4 reports the p-value of the test of zero difference correponding to the difference between columns 2 and 3.

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

(1) Job	(2) Share of skilled job-seekers searching this job
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
Cooker 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: Jobseekers' register (ANPE).

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

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 transports)	(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: Jobseekers' register (ANPE) and experiment survey (DARES).

Notes: Panel A1 reports estimates from the OLS regression of program participation on program assignment, controlling for gender, seniority in unemployment (in months), seniority in unemployment squared and 5 dummies for diplomas. See equation (5) for details. Panel A2 reports estimates from the same OLS regression as Panel A1, when 235 dummies for Local Employment Agencies (LEA) are added to the covariates. Panel B reports estimates from the OLS regression of the dependent variables listed in the rows on program assignment, controlling for the same covariates. See equation (4) for details. Panel C reports estimates from the IV regression of the dependent variables listed in the rows on program participation, controlling for the same covariates, with program participation instrumented by program assignment. See equation (5) for the first stage and (3) for the second stage. Columns (1) and (2) report estimates of regressions performed on the full sample of workers, while columns (3) and (4) are restricted to workers who were unemployed at random assignment, (5) and (6) to workers who were employed at random assignment, (7) and (8) to men who were unemployed at random assignment, and (9) and (10) to women who were unemployed at random assignment. "Unemployed "and "Employed" status refer to the 8-month survey declaration of young people about their employment status at the date of randomization. Columns (2), (4), (6), (8), and (10) are restricted to regions in which the private operator is a for-profit one, while the other columns are based on all regions. Control means display means of the dependent variable computed on the individuals that are part of the selected sample and are assigned to the control group. In all regressions, standard errors are robust to heteroskedasticity and are clustered at the Local Employment Agency level (LEA).

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Labor market outcome: Durable Fixed-Term Contract							
	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: Jobseekers' register (ANPE) and experiment survey (DARES).

Notes: The dependent variable is measured as having a job with a durable contract (at least 6 months), 8 months after random assignment. Panel A reports, in the first line ("Without LEA dummies"), estimates from the OLS regression of the outcome on program assignment, controlling for gender, seniority in unemployment (in months), seniority in unemployment squared, 5 dummies for diplomas and 47 dummy variables for quintuplets of LEA. In the second line ("With LEA dummies"), the dummy variables for quintuplets are replaced by 235 dummies for Local Employment Agencies (LEA). See equation (4) for details. Panel B reports, in the first line ("Without LEA dummies"), estimates from the IV regression of the outcome on program participation, controlling for the same covariates, with program participation instrumented by program assignment. In the second line ("With LEA dummies"), the dummy variables for quintuplets are replaced by 235 dummies for Local Employment Agencies (LEA). See equation (5) for the first stage and (3) for the second stage. In all regressions, standard errors are robust to heteroskedasticity and are clustered at the Local Employment Agency level (LEA). Columns (1) and (2) report estimates of regressions performed on the full sample of workers, while columns (3) and (4) are restricted to workers who were unemployed at random assignment, (5) and (6) to men who were unemployed at random assignment, and (7) and (8) to women who were unemployed at random assignment. "Unemployed " status refer to the 8-month survey declaration of young people about their employment status at the date of randomization. Columns (2), (4), (6), (8), and (10) are restricted to regions in which the private operator is a for-profit one, while the other columns are based on all regions. For each panel, the p-values of the equality tests between the coefficients of the first two lines are presented. Control means display means of the dependent variable computed on the individuals that are part of the selected sample and are assigned to the control group.

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

	Labor market outcome: Durable Fixed-Term Contract									
	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% areas	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% areas	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% areas	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% areas	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% areas	-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% areas	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% areas	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 equality of all assigned to treatment coefficients to zero	.33	.07*	.19	.01**	.41	.41	.71	.38	.28	.00***
F-test for equality of all assigned to control coefficients to zero	.34	.60	.32	.74	.34	.30	.05**	.03**	.37	.13
Observations	23320	8756	9890	3678	10317	3933	3716	1378	6174	2300

Source: Jobseekers' register (ANPE) and experiment survey (DARES).

Notes: The dependent variable is measured as having a job with a durable contract (at least 6 months), 8 months after random assignment. The table reports estimates from the OLS regression of the outcome on program assignment interacted with the assignment probability in the Local Employment Agency (LEA), controlling for gender, seniority in unemployment (in months), seniority in unemployment squared, 5 dummies for diplomas and 47 dummy variables for quintuplets of LEA. See equation (7) for details. In all regressions, standard errors are robust to heteroskedasticity and are clustered at the Local Employment Agency level (LEA). Columns (1) and (2) report estimates of regressions performed on the full sample of workers, while columns (3) and (4) are restricted to workers who were unemployed at random assignment, (5) and (6) to workers who were employed at random assignment, (7) and (8) to men who were unemployed at random assignment, and (9) and (10) to women who were unemployed at random assignment. Columns (2), (4), (6), (8), and (10) are restricted to regions in which the private operator is a for-profit one, while the other columns are based on all regions. The first test line is for equality of treatment effects in 25%,

Table 8: Reduced form: impact of program accounting for externalities

	All unemployed				Men (unemployed)				Women (unemployed)			
	By job types: share of youth in eligible in the sector											
	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: Durable Fixed Term contract												
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: Jobseekers' register (ANPE) and experiment survey (DARES).

Notes: The dependent variable is measured in panel A as having a job with a durable fixed-tern contract (at least 6 months), in panel B as having a durable fixed-term contract or an indefinite-term contract, in panel C as having a job, 8 months after random assignment. In each panel, the two first lines report estimates from the OLS regression of the outcome on program assignment and being in a Local Employment Agency (LEA) where the probability of assigned is not zero, controlling for gender, seniority in unemployment (in months), seniority in unemployment squared, 5 dummies for diplomas and 47 dummy variables for quintuplets of LEA. The third line reports estimates from the OLS regression of the outcome on program assignment minus being in LEA where the probability of assigned is not zero, controlling for being in LEA where the probability of assigned is not zero, and usual covariates. See equation (8) for details. In all regressions, standard errors are robust to heteroskedasticity and are clustered at the Local Employment Agency level (LEA). Regressions are performed only on individuals who declared, in the first wave of survey, having been unemployed at the assignment date. Columns (1) to (4) report results for all initially unemployed individuals. Columns (5) to (8) focus on men and columns (9) to (12) on women. Columns (1), (5) and (9) include jobseekers searching all kinds of jobs, while columns (2), (6) and (10) focus on those searching jobs where the share of skilled jobseekers is lower than the median. Columns (3), (7), and (11) focus on jobseekers searching jobs where the share of skilled jobseekers is higher than the median and columns (4), (8)s and (12) on jobseekers searching jobs where the share of skilled jobseekers is higher than the third quartile. Control means display means of the dependent variable computed on the individuals that are part of the selected sample and are assigned to the control group.

Table 9: Effect of the treatment, accounting for externalities

	All unemployed				Men (unemployed)				Women (unemployed)			
		By job types: share of youth in eligible in the sector										
	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: Durable Fixed Term contract												
Program participation	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	-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	(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	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	-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	(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	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	-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	(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: Jobseekers' register (ANPE) and experiment survey (DARES).

Notes: The dependent variable is measured in panel A as having a job with a durable fixed-term contract (at least 6 months), in panel B as having a durable fixed-term contract or an indefinite-term contract, in panel C as having a job, 8 months after random assignment. In each panel, the two first lines report estimates from the second stage of the IV regression of the outcome on program participation and being in a Local Employment Agency (LEA) where the probability of assigned is not zero, controlling for gender, seniority in unemployment (in months), seniority in unemployment squared, 5 dummies for diplomas and 47 dummy variables for quintuplets of LEA, where program participation is instrumented by program assignment. The third line reports estimates from the second stage of the IV regression of the outcome on program participation minus being in LEA where the probability of assigned is not zero, controlling for being in LEA where the probability of assigned is not zero, and usual covariates, where program participation is instrumented by program assignment. See equation (9) for details. In all regressions, standard errors are robust to heteroskedasticity and are clustered at the Local Employment Agency level (LEA). Regressions are performed only on individuals who declared, in the first wave of survey, having been unemployed at the assignment date. Columns (1) to (4) report results for all initially unemployed individuals. Columns (5) to (8) focus on men and columns (9) to (12) on women. Columns (1), (5) and (9) include jobseekers searching all kinds of jobs, while columns (2), (6) and (10) focus on those searching jobs where the share of skilled jobseekers is lower than the median. Columns (3), (7), and (11) focus on jobseekers searching jobs where the share of skilled jobseekers is higher than the median and columns (4), (8)s and (12) on jobseekers searching jobs where the share of skilled jobseekers is higher

Table 10: Heterogeneity of Program Effect by Area and Cohort

	(1)	(2)	(3)	(4)	(5)	(6)
	Durable Fixed Term contract			**Durable Employment**		
	All employment types			All employment types		
	All	Men	Women	All	Men	Women
PANEL A: COHORT DIFFERENCES						
Program participation (b1)	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 (b2)	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 (d1)	-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 (d2)	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 good cohort (control)	0.243	0.199	0.268	0.537	0.536	0.538
Mean bad cohort (control)	0.168	0.129	0.189	0.391	0.401	0.385
test: $\beta_8^L = \beta_8^H$ (p-value)	0.319	0.988	0.248	0.184	0.740	0.164
test: $\delta_8^L = \delta_8^H$ (p-value)	0.000***	0.010**	0.000***	0.000***	0.001***	0.000***
PANEL B: REGION DIFFERENCES						
Program participation (b1)	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 (b2)	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 (d1)	-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 (d2)	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: $\beta_7^L = \beta_7^H$ (p-value)	0.839	0.582	0.862	0.538	0.377	0.948
test: $\delta_7^L = \delta_7^H$ (p-value)	0.337	0.169	0.951	0.059*	0.125	0.315
PANEL C: REGION AND COHORT DIFFERENCES						
Program participation (b)	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 (b)	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 (b)	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 (b)	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 (d1)	-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 (d2)	-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 (d3)	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 (d4)	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: Jobseekers' register (ANPE) and experiment survey (DARES).

Notes: The dependent variable is measured in columns (1) to (4) as having a job with a durable fixed-term contract (at least 6 months), and in columns (5) to (8) as having a durable fixed-term contract or an indefinite-term contract. Panel A reports estimates from the second stage of the IV regression of the outcome on program participation and being in a treated Local Employment Agency (LEA) interacted with belonging to a cohort in which job opportunities were scarcer, controlling for gender, seniority in unemployment (in months), seniority in unemployment squared, 5 dummies for diplomas and 47 dummy variables for quintuplets of LEA, where program participation is instrumented by program assignment. Panel B reports estimates

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

		All unemployed				Men (unemployed)				Women (unemployed)			
			By job types: share of youth in eligible in the sector										
		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 (b)		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 (d)		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		(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 (b)		-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 (d)	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		(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 (b)		-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 (d)		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		(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: Jobseekers' register (ANPE) and waves 2 to 4 of the experiment survey (DARES).

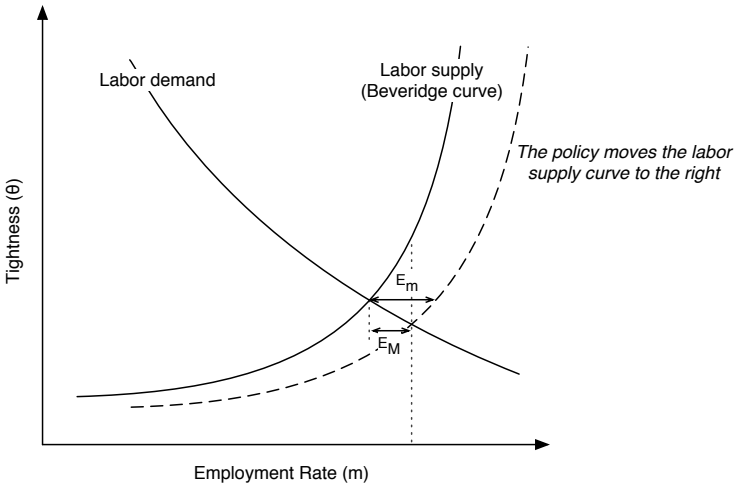
Notes: The dependent variable is measured as having a job with a durable contract (open-ended or fixed-term with at least 6 months duration). In each panel, the two first lines report estimates from the second stage of the IV regression of the outcome on program participation and being in a Local Employment Agency (LEA) where the probability of assigned is not zero, controlling for gender, seniority in unemployment (in months), seniority in unemployment squared, 5 dummies for diplomas and 47 dummy variables for quintuplets of LEA, where program participation is instrumented by program assignment. The third line reports estimates from the second stage of the IV regression of the outcome on program participation minus being in LEA where the probability of assigned is not zero, controlling for being in LEA where the probability of assigned is not zero and usual covariates, where program participation is instrumented by program assignment. See equation (9) for details. Panel A report the results of regressions applied to the second wave of the survey (12 months after the enter into experiment), Panel B to the third wave (16 months after) and Panel C to the fourth one (20 months after). Regressions are performed on individuals who declared, in the first wave of survey, having been unemployed at the assignment date. Column (1) reports results for all initially unemployed individuals. Column (2) focuses on jobseekers searching jobs where the share of skilled jobseekers is higher than the median. Column (3) focuses on jobseekers searching jobs where the share of skilled jobseekers is higher than the median. Column (4) above the third quartile. Column (5) to (8) restrict the same regressions to men and columns (9) to (12) to women. In all

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

	All unemployed				Men (unemployed)				Women (unemployed)			
	By job types: share of youth in eligible in the sector											
	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 (b)	-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 (d)	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	(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 (b)	-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	(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
Panel C: 16 months												
Program participation (b)	-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 (d)	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 (b)	-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 (d)	-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	(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

Figure 1: The impact of the policy

Panel A



Panel B

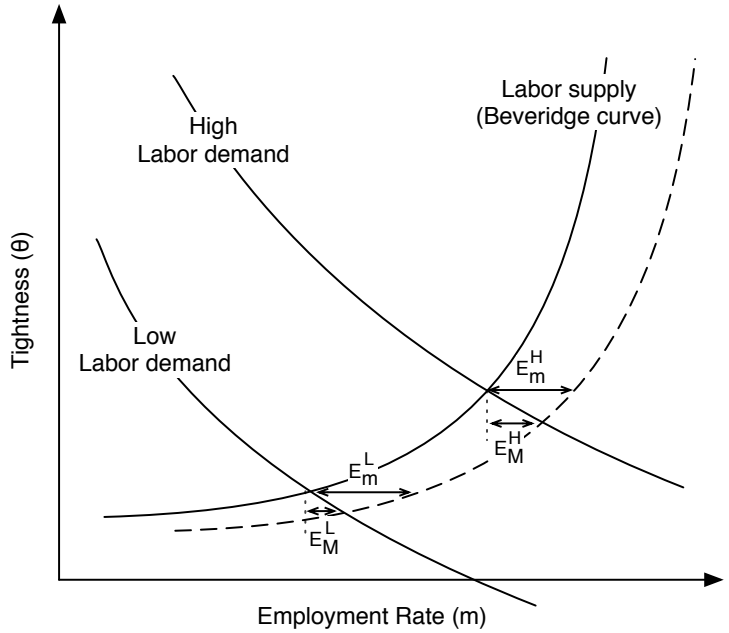
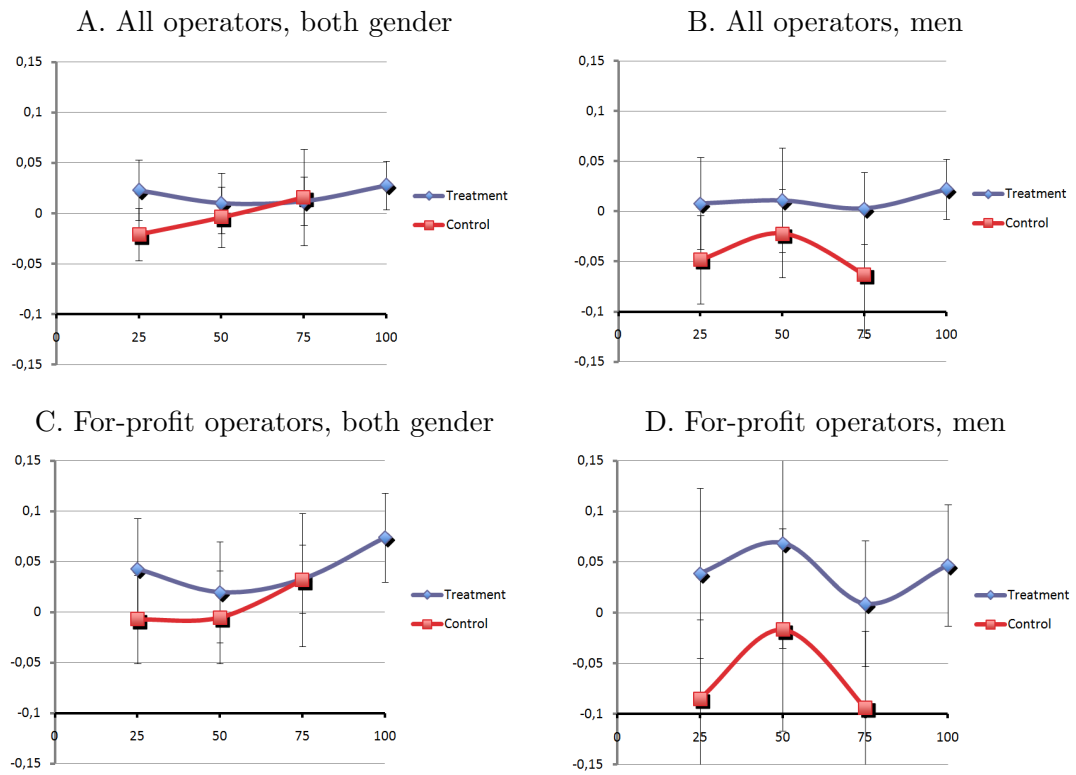


Figure 2: Disaggregated effect of the program

Initially unemployed Workers only



These graphs report the estimates and standard-errors from the regressions corresponding to equation (7). See note of the table (7)

Appendix A: Interpretation of the IV estimate with externalities

Consider a simple case where 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 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 outcome variables $y(P, T)$: $y(0, 0)$ is the potential outcome when no treatment takes place in the area, $y(1, 0)$ is the potential outcome when non treated but in a treatment area, and $y(1, 1)$ the potential outcome when treated.

The observed outcome is then simply:

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

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 $AE = E(y(1, 0) - y(0, 0))$ which is the average externality over the population and 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	(Ref: Technical Bac+2)	
PhD		-0.249^{**} (0.104)
Master from a university		-0.266^{***} (0.059)
Engineer, Business School Degree		-0.652^{***} (0.048)
Maitrise (Bac+4)		0.101^{***} (0.037)
Other Bac+4/5		-0.031 (0.077)
Bac+3		0.039 (0.045)
Bac+2 from a university		0.036 (0.060)
Other Bac+2/3		0.001 (0.032)
Less than Bac+2		-0.020 (0.065)
Not declared		-0.030 (0.041)
Seniority in unemployment	(Ref: 7 months)	
0 month		0.116^{***} (0.042)
3 months		0.044 (0.064)
4 months		0.102 (0.065)
5 months		0.190^{***} (0.067)
6 months		-0.005 (0.037)
8 months		0.052 (0.041)
9 to 12 months		0.071^{*} (0.037)
12 to 18 months		0.102^{***} (0.039)
18 to 24 months		0.116^{*} (0.060)
24 to 36 months		0.040 (0.063)
More than 36 months		0.022 (0.084)
Benefit recipient	(Ref: Recipient)	
Non benefit recipient		-0.128^{***} (0.024)
Employed at the time of assignment	(Ref: Not employed)	
Employed		-0.542^{***} (0.024)
Undeclared		-0.117^{***} (0.035)

Table A.2: Take-up by individual characteristics: probit regression (cont'd)

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

Source: Jobseekers' register (ANPE)

Notes: The table reports the results of a probit regression of being treated on some variables : age, cohort, region and assignment probability. The regression is performed on people assigned to treatment.

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

	All unemployed				Men (unemployed)				Women (unemployed)			
		By job types: share of youth in eligible in the sector										
	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: Durable Fixed Term contract												
Assigned to program (b)	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 (d)	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 (b+d)	(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 (b)	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 (d)	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 (b+d)	(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 (b)	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 (d)	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 (b+d)	(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: Jobseekers' register (ANPE) and experiment survey (DARES).

Notes: The dependent variable is measured in panel A as having a job with a durable fixed-term contract (at least 6 months), in panel B as having a durable fixed-term contract or an indefinite-term contract, in panel C as having a job, 8 months after random assignment. The table reports estimates from the OLS regression of the outcome on program assignment and the assignment probability in the Local Employment Agency (LEA), controlling for gender, seniority in unemployment (in months), seniority in unemployment squared, 5 dummies for diplomas and 47 dummy variables for quintuplets of LEA. See equation (**??**) for details. Regressions are here restricted to **regions where the counseling program was awarded to for profit operators**. They are performed only on individuals who declared, in the first wave of survey, having been unemployed at the assignment date. Columns (1) to (4) report results for all initially unemployed individuals. Columns (5) to (8) focus on men and columns (9) to (12) on women. Columns (1), (5) and (9) include jobseekers searching all kinds of jobs, while columns (2), (6) and (10) focus on those searching jobs where the share of skilled jobseekers is lower than the median. Columns (3), (7), and (11) focus on jobseekers searching jobs where the share of skilled jobseekers is higher than the median and columns (4), (8)s and (12) on jobseekers searching jobs where the share of skilled jobseekers is higher than the third quartile.In all regressions, standard errors are robust to heteroskedasticity and are clustered at the Local Employment Agency level (LEA).

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

	All unemployed				Men (unemployed)				Women (unemployed)			
		By job types: share of youth in eligible in the sector										
	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: Durable Fixed Term contract												
Program participation (b)	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 (d)	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 (b+d)	(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 (b)	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 (d)	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 (b+d)	(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 (b)	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 (d)	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 (b+d)	(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: Jobseekers' register (ANPE) and experiment survey (DARES).

Notes: This table is equivalent to table **??** but **restricted to regions where the counseling program was awarded to for profit operators**. The dependent variable is measured in panel A as having a job with a durable fixed-term contract (at least 6 months), in panel B as having a durable fixed-term contract or an indefinite-term contract, in panel C as having a job, 8 months after random assignment. In each panel, the two first lines report estimates from the second stage of the IV regression of the outcome on program participation and being in a Local Employment Agency (LEA) where the probability of assigned is not zero, controlling for gender, seniority in unemployment (in months), seniority in unemployment squared, 5 dummies for diplomas and 47 dummy variables for quintuplets of LEA, where program participation is instrumented by program assignment. The third line reports estimates from the second stage of the IV regression of the outcome on program participation minus being in LEA where the probability of assigned is not zero, controlling for being in LEA where the probability of assigned is not zero, gender, seniority in unemployment (in months), seniority in unemployment squared, 5 dummies for diplomas and 47 dummy variables for quintuplets of LEA, where program participation is instrumented by program assignment. See equation (9) for details. In all regressions, standard errors are robust to heteroskedasticity and are clustered at the Local Employment Agency level (LEA). Regressions are performed only on individuals who declared, in the first wave of survey, having been unemployed at the assignment date. Columns (1) to (4) report results for all initially unemployed individuals. Columns (5) to (8) focus on men and columns (9) to (12) on women. Columns (1), (5) and (9) include jobseekers searching all kinds of jobs, while columns (2), (6) and (10) focus on those searching jobs where the share

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

	All unemployed				Men (unemployed)				Women (unemployed)			
	By job types: share of youth in eligible in the sector											
	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 (b)	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 (d)	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	(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 (b)	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 (d)	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	(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
Panel C: 16 months												
Program participation (b)	-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 (d)	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	(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 (b)	-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 (d)	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	(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