DO LABOR MARKET POLICIES HAVE DISPLACEMENT EFFECTS? EVIDENCE FROM A CLUSTERED RANDOMIZED EXPERIMENT*

BRUNO CRÉPON
ESTHER DUFLO
MARC GURGAND
ROLAND RATHELOT
PHILIPPE ZAMORA

This article reports the results from a randomized experiment designed to evaluate the direct and indirect (displacement) impacts of job placement assistance on the labor market outcomes of young, educated job seekers in France. We use a two-step design. In the first step, the proportions of job seekers to be assigned to treatment (0%, 25%, 50%, 75%, or 100%) were randomly drawn for each of the 235 labor markets (e.g., cities) participating in the experiment. Then, in each labor market, eligible job seekers were randomly assigned to the treatment, following this proportion. After eight months, eligible, unemployed youths who were assigned to the program were significantly more likely to have found a stable job than those who were not. But these gains are transitory, and they appear to have come partly at the expense of eligible workers who did not benefit from the program, particularly in labor markets where they compete mainly with other educated workers, and in weak labor markets. Overall, the program seems to have had very little net benefits. *JEL* Codes: J68, J64, C93.

I. INTRODUCTION

Job placement assistance programs are popular in many industrialized countries.[1] In these programs, a private intermediary (such as a temporary work agency or a nonprofit organization) assists unemployed workers in their job search. These intermediaries are usually paid in full only when the worker

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1. They are particularly developed in Northern Europe. For instance, in 2010, according to the OECD Labour Market Program Database, they represented 0.34%, 0.19%, 0.21% of gross domestic product (GDP) in Denmark, Germany, and Sweden, respectively. In France, expenditures on employment placement services represent 0.25% of GDP.

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has found a stable job. Unlike other active labor market policies, whose effects have in general be found to be weak, most studies tend to find a significant and positive impact of this form of counseling, especially for job seekers with a low risk of long-duration unemployment (see reviews in Kluve 2006; Card, Kluve, and Weber 2010).

This article focuses on a large-scale job seeker assistance program targeted at young, educated job seekers in France. Under the program, private agencies are contracted to provide intensive placement services to young graduates (with at least a two-year college degree) who have been unemployed for at least six months. The private provider is paid partially on delivery, that is, conditional on the individual finding a job with a contract of at least six months and staying employed for at least six months.

Previous studies on similar programs are generally based on a comparison between the short-run labor market outcomes of counseled versus noncounseled job seekers.[2] Experimental studies are still relatively rare, but they also tend to find positive impacts of counseling (Rosholm 2008; Behaghel, Crépon, and Gurgand 2012).[3] However, an important criticism leveled against these studies 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 with whom they compete in the labor market. This may be particularly true in the short run, during which vacancies do not adjust: the unemployed who do not benefit from the program could be partially crowded out.

Evaluating the magnitude of such displacement effects is essential to a full understanding of the impact of any labor market policy. If all a policy does is to lead to a game of musical chairs among unemployed workers, then the impacts estimated from a standard randomized or nonrandomized evaluation will overestimate its impact for two reasons. First, the treatment effect will be biased upward when we compare a treated worker to a nontreated worker in a given area. The employment rate among

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

3. An exception is van den Berg and van der Klaauw (2006), which finds no impact in the Netherlands, but the intervention they study had more to do with monitoring than with actual counseling.

workers in the control group is lower than it would have been absent the program, leading to a violation of the stable unit treatment value assumption, or SUTVA (Rubin 1980, 1990). At the extreme, we could (wrongly) deem a policy successful if it *only* negatively affected those in the control group. Second, the negative externalities themselves must also be taken into account when judging the overall welfare impacts and cost effectiveness of any policy.

More generally, learning whether and when such externalities arise can help shed light on how labor markets function. We motivate our study with a simplified version of a search model proposed by Michaillat (2012) and Landais, Michaillat, and Saez (2012). This model has the realistic feature that production technology exhibits diminishing returns to scale. As a result, when an unemployed worker increases her search effort, she imposes negative externalities on other workers. In contrast, standard search models with a flat labor demand (e.g., Pissarides 2000) produce no such externalities. Our model also features the additional prediction that externalities should be stronger when the labor market is slack, which we investigate in the data.

Although the possibility of such externalities has long been recognized,[4] there are few studies focusing specifically on externalities in the labor market, and the evidence is mixed. For instance, in their evaluation of the UK New Deal for Young Unemployed, Blundell et al. (2004) compare ineligible people in the areas affected by the program to those in areas not affected by the program. The authors do not find significant indirect effects on untreated youth of residing in treated areas. Likewise, Pallais (2010) estimates the market equilibrium effect of a short-term employment opportunity given to workers in an online marketplace and finds little evidence of displacement. In contrast, Ferracci, Jolivet, and van den Berg (2010) find that in France, the impact of a training program for young unemployed workers diminishes with the fraction of treated workers in a labor market, which could be a sign of externalities. Gautier et al. (2011) analyze a Danish randomized evaluation of a job search assistance program. Comparing control individuals in experimental

4. See Johnson (1979), Atkinson (1987), Meyer (1995), Davidson and Woodbury (1993), Lise, Saez, and Smith (2004), Van der Linden (2005), and Cahuc and Le Barbanchon (2010) for previous work on the topic.

counties to job seekers in some similar nonparticipating counties, they find hints of substantial negative treatment externalities.[5]

One potential issue with these studies is that even when the individual treatment is randomly assigned, or as good as randomly assigned, the number of people who are "treated" within a market is not itself randomly assigned. The comparison across markets may thus lead to biased estimates of the equilibrium effects. To address this issue, we implement a two-step randomized design, similar to Duflo and Saez (2003).

In the first step, each of 235 local employment areas are randomly assigned a proportion P of job seekers to be assigned to treatment: either 0%, 25%, 50%, 75%, or 100%. In the second step, in each area, a fraction P of *all* the eligible job seekers is randomly selected to be assigned to treatment. Those assigned to treatment are offered the opportunity to enroll in the job placement program (about one-third of those assigned to treatment actually enrolled). For those who were assigned to the control group or refused the treatment, nothing changed: they continued to be followed by the counselors of ANPE (French public employment agency) and receive the standard forms of assistance. This design allows us to test for externalities on untreated workers, by comparing untreated workers in areas where some workers are treated to those in areas with no treated workers.

A first comparison suggests, consistent with the prior literature, that the program has positive impacts: after eight months, unemployed workers assigned to treatment are 1.7 percentage points (11%) more likely to have a fixed-term contract with a length of more than six months than the unassigned workers in all areas, and 2.3 percentage points more likely than the unassigned workers in treatment areas. The results are almost identical for any stable job (1.5 and 2.5 percentage points, respectively). The positive effects appear to be concentrated on men, however.

The evidence on externalities imposed on the unemployed eligible youths who were not assigned to treatment is mixed for the full sample: the untreated workers in a treated area are 1.3 percentage points less likely to find a long-term fixed contract than workers in control areas (insignificant), and 2.1 percentage points less likely to find any kind of stable job (significant at the

5. See also Dahlberg and Forslund (2005) for an early attempt to estimate displacement effects.

10% level). Once again, those effects are entirely due to men: untreated men in treated areas are 3.6 (4.3) percentage points less likely to find a long-term fixed contract (any kind of stable job) than men in control areas (both of these numbers are significant at the 5% level); for women the coefficients are very small (0.1 percentage point and 1 percentage point respectively), and totally insignificant.

Even for men, we cannot reject that the effect on unassigned workers is the same in all treatment areas, irrespective of the fraction of assigned among eligible workers, something we would expect with externalities. In particular the negative externalities are almost as large in areas treated at 25% and in areas treated at 75%. This may reflect a lack of power.

However, to the extent that the beneficiaries of the program took jobs that other workers (who were, for example, less educated, or unemployed for a shorter time) also competed for, the externalities may not have been limited to the eligible youths: in fact, they may have been smaller for eligible youths because they were distributed among a larger group of unemployed workers. To shed light on this issue, we investigate how externalities vary with the nationwide share of graduates among all job seekers searching in the same sector. We find that the externalities on eligible youth tend to be stronger when they compete mainly with other eligible workers. This suggests that externalities affect not only people in our sample, but many others as well, although we do not have data allowing us to estimate externalities for ineligible workers.

Furthermore, consistent with the theoretical framework, the externalities are strongest for those who end up searching for a job in slack labor markets. They also were particularly important in the most depressed areas during the last period of the experiment, when recession sharply affected the labor market.

These estimates imply that the program's benefits would have been overstated in a standard program evaluation with individuals randomly assigned within specific sites (for example, as in Dolton and O'Neill 1996, van den Berg and van der Klaauw 2006). Taking into account the externalities on both eligible and ineligible youth, the net number of jobs created by the program appears to be negligible compared to its cost. These results also challenge the conclusions of traditional equilibrium unemployment models and suggest that it is important to account for the possibility of job rationing when analyzing the impact of labor market policies (like Landais, Michaillat, and Saez 2012 for the design of unemployment insurance).

The job placement assistance program and the institutional context are described in the next section. Section III proposes a conceptual framework which clarifies when and why externalities on untreated workers may be expected. Section IV gives details regarding the experimental design and the data. Section V presents the empirical strategy, Sections VI and VII discusses the results, and Section VIII concludes.

II. Institutional Context and Description of the Program

II.A. Background: Placement Services in France

Until 2005, the French public employment agency ANPE (Agence Nationale Pour l'Emploi) had, from a legal point of view, a monopoly on job placement services. In particular, employers were legally obligated to list their vacancies with ANPE.[6] In 2005, the Social Cohesion Law broke this virtual monopoly by permitting temporary work agencies to openly market their counseling and placement services to job seekers. The public operator (which was renamed Pole Emploi in 2008) has remained an important agency because all unemployment insurance (UI) recipients must meet their ANPE caseworkers at least once a month and follow their recommendations to remain eligible for benefits. Nevertheless, according to a quarterly survey conducted by ANPE with those who left the unemployment rolls (*enquête sortants*), between 2002 and 2006, 16% of those who had found a job reported having done so thanks to a contact obtained by a temp agency, whereas only 12% had found the contact through ANPE.

To help foster a vibrant private job placement market, the government and unions decided to encourage partnerships between the public operator and private actors. Some specific types of job seekers were targeted, starting with those for whom the ANPE was known to have difficulty assisting. The idea of forming partnerships was adapted from the German Hartz reforms (Jacobi and Kluve 2007), in which each local

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

employment office was required to contract with a “Personal Service Agentur” (PSA), often a temporary work agency. PSAs are responsible for assisting a certain number of job seekers and receive a payment for each that finds a job.

Three experiments were launched in France to evaluate the effects of subcontracting placement services to private providers.[7] One was dedicated to job seekers at risk of long-term unemployment (Behaghel, Crépon, and Gurgand 2012); another to welfare beneficiaries (Crépon et al. 2011); and a third to young graduates who had been searching for a job for six months or more. This article analyzes the third experiment.

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

II.B. Program Description

The private providers’ intervention has two parts. Phase I aims to help job seekers find work. For the first six months of the program, the private employment agency counsels the job seeker and helps her find a durable job. The job must be on either a CDI (indefinite-term contract) or a CDD (fixed-term contract) with a length of six months or more. Phase II aims to support the former job seeker in her job. During the first six months of the job, the client continued to be followed and advised by the agency. The aim of this phase is to help the client keep her job or find a new job if she resigns.

Although the specific content of the intervention can vary locally, it has three basic features. First and foremost, a dedicated caseworker is assigned to the job seeker, who should meet her in

7. See Krug and Stephan (2011) for a German example.

person at least once a week. Second, this caseworker has the responsibility to identify job offers that can fit the profile of the job seekers he works with. Third, job seekers attend workshops on various aspects of the search process. A survey of clients from another private operator–run program—which covered the same period and involved some of the same operators working with precisely the same mission—found that one-third of clients attended a professional assessment program, two-thirds attended workshops on writing vitae and motivation letters, and half attended workshops on job interviews, targeting firms, or searching the Internet for jobs (Gratadour and Le Barbanchon, 2009). This turns out to be similar to the level of access offered by the public employment service program. Thus, rather than in these workshops, the added value of intensive counseling seems to lie in the frequent interviews with the dedicated caseworker and the regular follow-up on search strategy and actions taken. The programs are often organized around an individual action plan, the objective of which is periodically reviewed. Although there is no formal monitoring element built into the program as such, counselors are able to form personal relationships with the job seekers and informally encourage a more vigorous search effort (Divay 2009).

In each of the 10 experimental regions, an invitation to tender was issued. The government chose the providers on the basis of the services they offered and the prices they charged. In six regions, for-profit operators were selected, and five of these six were subsidiaries of temporary employment agencies. In four regions, not-for-profit organizations were selected. One not-for-profit was a social and solidarity-oriented training center, and the others were local agencies that are part of a larger not-for-profit youth guidance organization.

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

- *Enrollment:* when a job seeker is enrolled in the program, the private agency receives the first payment (25% of the maximum payment possible).
- *Finding (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 (or an indefinite job), the second payment occurs (40%).

- *Remaining employed after six months:* six months after the job is found, the third payment is made to the operator if the former job seeker is still employed (35%).

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

III. Conceptual Framework

A model of search with decreasing returns to scale in the production function, which is a simplified version of Landais, Michaillat, and Saez (2012) and Michaillat (2012), helps clarify the conditions under which a job search assistance program like this one might generate externalities. In conventional models of equilibrium unemployment with frictions, if some workers increase their job search effort, this generates additional employment creation. The remaining workers are not displaced from existing jobs because, in the process, the total pool of jobs increases enough to absorb the extra labor supply. In the model we consider here, however, job creation does not adjust fully in equilibrium, so untreated job seekers are at least partly displaced by treated ones.

We consider a model with one sector and one type of workers.[8] Jobs end randomly at rate s. Individuals can be unemployed or employed. Let u and n denote the number of unemployed and employed workers; we normalize the labor force to 1, so that $n + u = 1$.

Unemployed people search for jobs and firms open vacancies to hire them. Denote total job search effort exercised by the unemployed as u_e and total opened vacancies as v. The number of matches resulting from the aggregated search effort and available vacancies is given by the matching function $m(u_e, v)$. Following the standard matching model as in Pissarides (2000), we assume the m function is increasing and concave in both its

8. The model can easily be extended to include skilled and unskilled workers for instance, with varying degrees of substitutability, and to allow different types of workers to search either through the same channel or through separate ones.

arguments and homogenous of degree one. The tightness of the labor market is defined as $\theta = \frac{v}{u_e}$.

Not all workers can find a job, and not all vacancies are filled. The probability that a vacancy is filled is $\frac{m(u_e,v)}{v} = m(\frac{u_e}{v},1) = m(\frac{1}{\theta},1) = q(\theta)$, which is decreasing in θ. The probability that an unemployed worker exercising one unit of search effort finds a job is $\frac{m(u_e,v)}{u_e} = \frac{m(u_e,v)}{v} \times \frac{v}{u_e} = \theta q(\theta) = f(\theta)$, which is both increasing and concave in θ, given the assumptions on the matching function.

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

There are thus two types of unemployed job seekers: the treated, benefiting from the counseling program, and those who are not treated. In steady state, there are u_0 untreated and u_1 treated job seekers. Total search effort is thus $u_e = eu_1 + u_0$. These two groups have different exit rates that are derived from the matching function: counseled individuals account for a share $\frac{eu_1}{u_e}$ of the search effort, so that they receive $\frac{eu_1 m(v,u_e)}{u_e} = eu_1 f(\theta)$ job offers. The exit rate for counseled individuals is thus equal to $ef(\theta)$, and the exit rate for the untreated is $f(\theta)$.

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

At the steady state, the inflows and outflows of treated and untreated individuals must remain constant. Therefore, as the total inflow of unemployed people is sn, we have:

$$u_1 ef(\theta) = \pi sn \tag{1}$$

$$u_0 f(\theta) = (1-\pi)sn \tag{2}$$

9. Search effort can be endogenized as in Landais, Michaillat, and Saez (2012), leading to the same results for our purpose.

Writing $1 - n = u = u_1 + u_0$, we can derive the labor supply curve as a mapping between θ and the employment rate n:[10]

$$n = \frac{f(\theta)}{s(\pi/e + 1 - \pi) + f(\theta)} \tag{3}$$

The resulting, $\theta = \theta_B(n)$ is an increasing function of n. Figure I draws the labor supply curve in the tightness/employment rate space (like figure 1 in Landais, Michaillat, and Saez 2012). This is the equivalent of the Beveridge curve, which is conventionally represented in the unemployment-vacancy space. Note that the curve is fairly flat for low levels of employment (low θ) and steep when employment is high: because the function $f(\theta) = m(\theta, 1)$ is concave due to the constant returns to scale assumption for the matching function and increasing, the function $\theta_B(n)$ is convex.

To find the labor market equilibrium, we now consider the firm's decision. We assume that the production technology exhibits decreasing return to scale. This can be justified by some factor (management, fixed capital, etc.) being fixed in the short run. Consider for example the technology is a simple Cobb-Douglas production function:

$$y = an^{\alpha}, \quad \alpha \in (0, 1).$$

To simplify the argument, assume that the total operating cost for a job is fixed $w = w_0$ (for example, because all entry-level workers are paid a binding minimum or negotiated wage).[11] The firm chooses employment to maximize the value of output, minus operating and hiring costs. Let c be the per period cost of an unfilled vacancy, and r the interest rate. Using the Bellman equations for the value of having a vacancy and a filled job we can derive the following labor demand equation:[12]

$$\alpha a n^{\alpha-1} - w_0 - c\frac{r+s}{q(\theta)} = 0 \tag{4}$$

10. We simply use equations (1) and (2) to express u_1 and u_0 as a function of n, and then plug them into $1 - n = u_1 + u_0$.

11. We make this assumption to keep the exposition simple. Endogenous wages as determined by a bargaining model, for example, would not lead to major changes. See note 13.

12. This equation is derived from: (1) the Bellman equations for the value of having a vacancy J_V and a filled job J_E ($rJ_V = -c + q(\theta)(J_E - J_V)$ and $rJ_E = p - w + s(J_V - J_E)$, where $p = \alpha a n^{\alpha-1}$ is the marginal product related to a new hire; and (2) the entry condition requiring that the value of having a vacancy is 0.

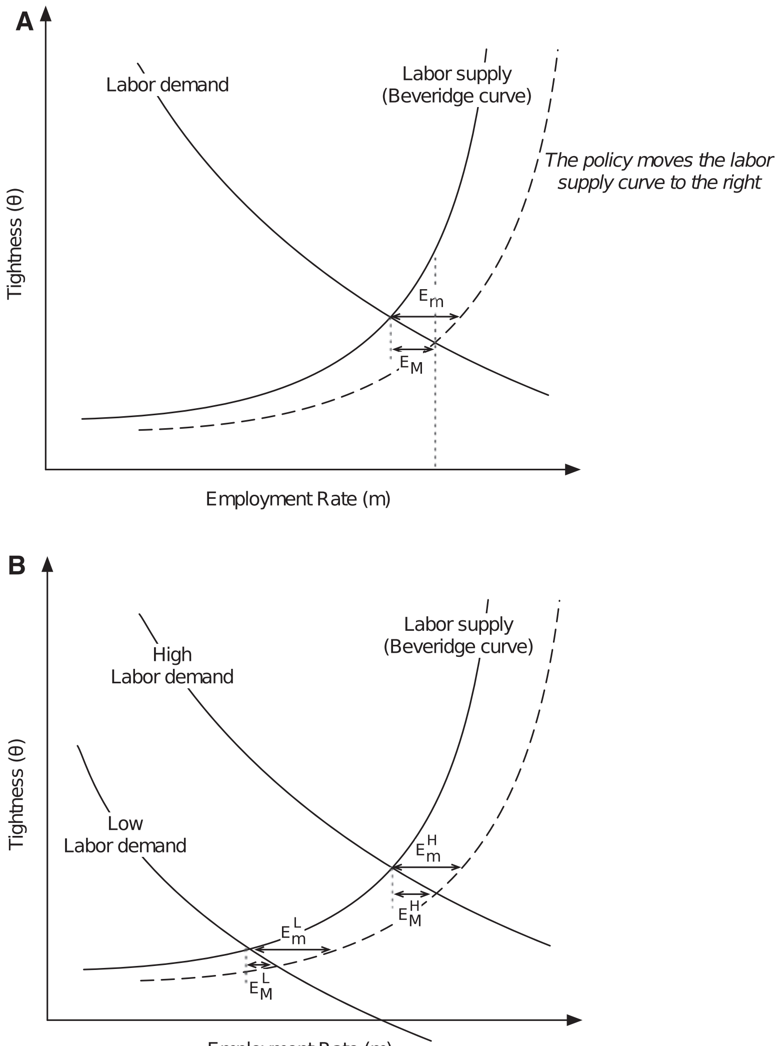

FIGURE I

The Impact of the Policy

Frictions in the labor market can be interpreted as a marginal cost of hiring $\frac{c(r+s)}{q(\theta)}$. This labor demand equation leads to a decreasing relationship between the employment rate and θ: $\theta = \theta_d(n)$. The two equations (3) and (4) together lead to the equilibrium values of θ and n.

The effect of the policy is illustrated in Figure I, Panel A. Starting from an initial situation with $\pi = 0$ and $e = 1$, the policy amounts to providing part of job seekers on that market ($\pi > 0$) with reinforced counseling scheme ($e > 1$). This leads to a decrease in $\left(\frac{\pi}{e} + 1 - \pi\right)$, and thus the Beveridge curve shifts to the right while the labor demand curve remains unchanged. Clearly, this leads to an increase in employment and a decrease in θ in equilibrium. This induces displacement effects, because the exit rate of the untreated, $f(\theta)$, decreases. In the notation used by Landais, Michaillat, and Saez (2012), the size of the externality can be illustrated by the difference between the micro elasticity of employment with respect to the shift in the Beveridge curve (E_m on the graph), which is the effect on one individual and does not take into account the slope of the demand curve, and the macro elasticity (E_M), which represents the net increase in employment.

Notice the key difference between this model and usual matching models such as Pissarides (2000). In such models, where return to scale in the production function is constant, the labor demand equation (4) is horizontal, so θ must remain constant for any value of n. As the ratio of vacancies to unemployment is fixed, new vacancies open as new jobs are filled. Therefore, the shift in the Beveridge curve does not lead to any displacement effects. If there is decreasing return to scale, however, marginal productivity decreases as employment n increases, and θ must adjust.[13] At the other extreme, if the labor demand curve was completely vertical, there would be no aggregate employment effect of a job placement policy (pure rat race model). The gains accruing to beneficiaries would be entirely undone by losses experienced by nonbeneficiaries.

In general, this model predicts that there will be direct employment effects for the beneficiaries, and also externalities on

13. If the wage was made endogenous, for example, if it were the result of a bargaining model, we would obtain a wage equation of the form $w = w(n, \theta)$. In that case substituting $w(n, \theta)$ for w_0 in the labor demand still leads to a decreasing relationship between n and θ (see equation 12 in Michaillat 2012), and there could be employment externalities through this channel. The mechanism would, however, be entirely different: wages would increase due to the improvement in the fallback position of the counseled workers, the deterioration of the untreated situation, and the opening of fewer vacancies. This channel appears to be much less realistic in our context, and we show in the empirical analysis that the program had no impact on wages.

the nonbeneficiaries, as long as the labor demand curve is not completely flat (which will be the case as soon as there is a limiting factor, such as capital or management).

The model has two additional testable predictions that we take to the data.

First, the size of the externality directly depends on π: if very few workers are treated in a particular market, very little changes for the untreated. In turn, π is a function of (1) the fraction of people searching for a job in a particular occupation who are eligible for the program (in our experiment, young, educated, unemployed for more than six months); and (2) the proportion of them assigned to the program. Let κ be the share of eligible unemployed workers among all unemployed workers who are likely close substitutes (in what follows, we compute the share of eligible among those aged under age 30). Assume also that eligible and ineligible individuals are perfectly substitutable. The program varies the share of *eligible* unemployed workers that are assigned to the program, which we denote σ. The share treated in that market is therefore $\pi = \kappa\sigma$.[14] We should thus find larger externalities on other educated workers in labor markets where more workers were assigned to the treatment, and also in professions where educated workers form a larger part of the relevant labor market.

The second prediction is based on the shape of the labor supply curve. This prediction is explored in detail (and proved) in Landais, Michaillat, and Saez (2012) and forms the core of the authors' argument that unemployment insurance should be higher during recessions. This prediction is illustrated in Figure I, Panel B. If labor demand is low (left part of the graph), a shift in the labor supply curve will lead to a large gap between the micro and the macro elasticity (i.e., a large externality) because the labor supply curve in this space is almost flat. Employment in this part of the graph is mainly constrained by demand, not by search productivity, so that increasing the productivity of search has very little impact on total employment: the main benefit for the treated workers is that they move ahead in the rat race. If demand is high (right part of the graph), an

14. If ineligible workers were imperfectly substitutable, it would change this expression, but not the qualitative prediction that the strength of the externalities would depend on the fraction of substituable workers in each occupation.

increase in search productivity has much larger net employment effects (and smaller associated externalities).

IV. Experimental Design and Data

IV.A. Experimental Design

The randomization took place at both the labor market and individual levels. It was organized in the areas covered by 235 public unemployment agencies, scattered across 10 administrative regions (about half of France). Each agency represents a small labor market, within which we may observe treatment externalities. On the other hand, the agencies cover areas that are sufficiently large, and workers in France are sufficiently immobile, that we can assume that no spillovers take place across areas covered by different agencies.[15] Migration or spillover would lead us to underestimate the magnitude of externalities. The results we present are robust to the exclusion of one region (Nord Pas de Calais), which is dominated by a large city (Lille), where treatment and control areas are contiguous.

To improve precision, we first formed groups of five agencies that covered areas similar in size and with comparable local populations; we obtained 47 such quintuplets. Within each of these strata, we randomly selected one permutation assigning the five labor markets to five fractions of treated workers: $P \in \{0, 0.25, 0.50, 0.75, 1\}$.

Every month from September 2007 to October 2008, job seekers who met the criteria for the target population (aged below 30, with at least a two-year college degree, and having spent either 12 out of the last 18 months or 6 months continuously unemployed or underemployed) were identified by the national ANPE office, using the official unemployment registries.

The list of job seekers was transmitted to us, and we randomly selected a fraction of workers following the assigned proportion into treatment within each agency area. The list of individuals that we selected to be potential beneficiaries of the program was then passed on to the contracted counseling firm in the area, which was in charge of contacting the youth and offering them entry into the job placement program. Entry was voluntary,

15. According to the *enquête sortants*, only 17% of eligible youth who found a job in a given quarter had to move to get it.

and the youth could elect to continue receiving services from the local public unemployment agency instead or no service at all. No youth from the control group could be approached by the firm at any time, and none of them were treated.

IV.B. Data

There are three sources of data for this experiment. First, we use the administrative lists of job seekers provided by ANPE to the Ministry of Labor. For each job seeker, these files provide the individual's age, postal address, the number of months spent unemployed during the current unemployment spell, the type of job being sought, and the public employment agency in charge of helping her. These registries are imperfect, because they are not updated in real time; as we will see, a number of workers who were randomized into treatment were in fact already employed at the time of randomization.

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

Our third source of data was four follow-up surveys conducted 8 months, 12 months, 16 months, and 20 months after random assignment. These surveys were necessary because existing administrative data do not provide a good measure of the transition from unemployment to employment; the information recorded reliably is whether someone is still registered as an official job seeker.[16] A youth who stops being registered could either have become discouraged or found a job. In addition, young job seekers do not have strong incentives to be registered with the ANPE, in particular because they are often not eligible to receive unemployment benefits. Unfortunately, administrative data on employment and wages (from the tax authority or the social security administration) cannot be linked to the experimental data for legal reasons related to confidentiality protection.

16. The administrative data on exits from the unemployment registry are affected by both imperfect updating and "unknown exit" for a significant share of unemployment leavers, that is, when a worker leaves the ANPE registry, it can be either because they have found a job or because they have stopped searching for one.

The survey was conducted by DARES, the research department at the Ministry of Labor, and was thus an official survey; answering was not mandatory, but response rates to surveys conducted by public agencies tend to be high in France. To limit data collection costs and increase the response rate, the survey was short (10 minutes for the first wave, 5 minutes for the others). Moreover, the survey combined three collection methods: Internet, telephone, and paper questionnaires. As a result, response rates were high: as shown in Table I, 79% answered the first survey (the one administered after eight months).

Participants were assigned to the experiment in 14 monthly cohorts, starting in September 2007. The study focuses on cohorts 3–11.[17] In these cohorts, 29,636 individuals were randomly selected to be surveyed and 21,431 were found eight months after assignment. Out of them, most of our analysis focuses on the 11,806 who did not declare to be in employment at the time of assignment.

Table I also shows the response rates conditional on having been assigned to either the treatment or control group. The response rate is higher than 70%, and the job seekers assigned to treatment are only 1 percentage point more likely to answer than those assigned to control. In all waves, the response rates remained very high and very similar in treatment and control groups.

The first survey wave took place between August 2008 and May 2009; the last survey wave took place between August 2009

17. We faced a budget constraint that limited the overall size of the sample we could follow, so we made decisions about where to draw the follow up sample from. Cohorts 1 and 2 were not followed because it took a couple of weeks before the private operators were ready to actually offer the treatment. Cohorts 12 to 14 are not used because in July 2008, one month before cohort 12 became eligible for the experiment, the ministry issued a separate, more profitable call for tender for job seeker counseling. Anecdotal evidence and data on the number of beneficiaries from these cohorts suggest that private firms were more focused on this second operation and all but stopped implementing the experimental program. Indeed, youths from these cohorts were not enrolled even when they were officially selected for treatment, and youth in the control groups started being enrolled in this other program, particularly where the private operators were in place. This would have biased our estimates of both direct effects and externalities. In particular, if the private operators targeted the control group for the second program in treatment regions because they had already an office there, this would make our estimates of externalities appear positive. We did not collect data for cohorts 13 and 14; including cohort 12 in the analysis leaves results qualitatively unchanged but somewhat noisier.

TABLE I
RESPONSE RATES TO EIGHT-MONTH SURVEY

Status	(1) Number of responses	(2) All	(3) Control	(4) Treatment	(5) Difference
All	21,431	0.785	0.779	0.789	0.010
					(0.005)
Not employed	11,806	0.713	0.703	0.722	0.019
					(0.007)
Employed	9,625	0.894	0.896	0.893	−0.003
					(0.006)

Notes. Column (1) reports the total number of responses to the survey run eight months after randomization for cohorts 3–11 (total number of individuals sampled for these cohorts is 27,311). Columns (2)–(4) report response rates and column (5) shows the difference between columns (3) and (4) (standard error in parentheses). The second row restricts the sample to job seekers who did not report that they were employed at the time of randomization (either unemployed or undecleared); the third line restricts the sample to those who did.

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

and May 2010. The survey included questions about the current respondent's employment situation (wage, type of contract, part-time or not, occupation). It also elicited some retrospective information about the respondent's situation at the program assignment date, highest degree obtained, family situation (marital status, number of children), and nationality (or parents' nationality). It asked how many times the respondent met a counselor (public, or from the contracted private agency) and what type of help she got during her job search. Finally, individuals assigned to treatment were asked the ways in which they thought they would benefit from entering the program (if they agreed to enter), while those who chose not to participate were asked the reason.

Table II presents summary statistics for job seekers before program assignment (using ANPE administrative file and, for the last row, our own survey), as well as balancing tests.

Most individuals in the sample are in their twenties, which is not surprising given the age requirement. Another eligibility condition involved length of unemployment spell; individuals had to have been looking for a job for more than 6 months or to have been unemployed for more than 12 of the last 18 months. Indeed, individuals who have been unemployed for seven months or longer are overrepresented in the sample. Nineteen percent of the sample has been unemployed for 12 months or more. Because these job seekers are young and have often only had jobs for

TABLE II
SUMMARY STATISTICS

	Proportions			Balancing stats (p-values)			
	All	Treatment	Control	T – C	Across P	Across P (for T)	Across P (for C)
Age	25.567	25.554	25.585	0.592	0.854	0.627	0.767
Seniority in unemployment							
0 to 5 months	0.163	0.163	0.162	0.799	0.503	0.153	0.618
6 months	0.115	0.117	0.112	0.279	0.825	0.462	0.553
7 months	0.301	0.306	0.295	0.211	0.729	0.764	0.434
8 months	0.089	0.089	0.09	0.816	0.805	0.378	0.982
9 to 12 months	0.141	0.139	0.144	0.379	0.673	0.746	0.808
More than 12	0.191	0.186	0.198	0.070	0.251	0.566	0.146
Receives UI	0.328	0.337	0.316	0.028	0.906	0.896	0.274
Male	0.354	0.361	0.347	0.15	0.469	0.851	0.029
Highest degree							
Bac + 5 and more	0.163	0.158	0.169	0.137	0.547	0.448	0.770
Bac + 4	0.11	0.115	0.104	0.042	0.333	0.248	0.709
Bac + 3	0.184	0.183	0.186	0.643	0.501	0.022	0.186
Bac + 2	0.409	0.412	0.404	0.449	0.593	0.519	0.546
Less than Bac + 2	0.134	0.131	0.137	0.275	0.077	0.010	0.480
Not declared	0.001	0.001	0.001	0.392	0.909	0.603	0.891
Employed at randomization							
Employed	0.449	0.445	0.455	0.223	0.543	0.276	0.929
Not employed	0.426	0.430	0.422	0.326	0.274	0.100	0.927
Did not answer	0.125	0.126	0.123	0.651	0.953	0.721	0.999
Number of observations	21,431	12,001	9,430				

Notes. These summary statistics are based on the respondants to the survey, from cohorts 3–11. Columns (1), (2), and (3) report the means of individual characteristics in the full sample, the treatment, and control subsamples, respectively. Columns (4)–(7) report the p-values for the difference between all assigned to treatment and assigned to control (4), joint significance of type of zone dummies on the whole sample (5), among those assigned to treatment (6), and among those assigned to control (7).

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

limited lengths of time, most of them (67%) are not receiving unemployment benefits. Nearly two-thirds of job seekers are women. Finally, 41% of the sample has a two-year college degree (Bac + 2), and individuals with higher university degrees (Bac + 3 and more) represent 46% of the sample.

The last four columns in Table II present balancing tests. The experimental design generates eight experimental groups (untreated workers in control areas, three groups of untreated workers in treated areas, and four groups of treated workers in treated areas). In the analysis that follows, we compare assigned and unassigned workers, unassigned workers across types of areas, and assigned workers across types of areas. We thus present the *p*-values for four types of tests: assigned versus unassigned, joint significance of all the group dummies (with the "super-control" group, in which there is zero probability of treatment assignment, as the omitted category), and joint significance on treated and control groups separately. Eight out of 72 contrasts are significant at the 10% level, which is expected under random assignment.[18]

The last three rows of Table II present summary statistics on employment status at the start of the experiment for those who responded to the first wave of the DARES survey. Importantly, 45% of the sample claimed to have been employed at the time of treatment assignment. There are several possible reasons for this. First, respondents could have recently found a job, and their status may not have been updated in the unemployment agency list used to generate the randomization sample. Second, respondents may have been underemployed, that is, holding a part-time job but still looking for full-time employment, and so would have been eligible for treatment (this employment status is known as *activité réduite*, or limited activity). In what follows, we focus on results for those who did not claim to be employed at baseline (i.e., those who report that they were either unemployed or do not remember their status at baseline), because they were the target of the intervention. Though all those randomized remained eligible, and a few took advantage of the treatment, with better data we would not have included them in the

18. Please refer to the Online Appendix for summary statistics on the sample of those who were initially unemployed (which form the bulk of our analysis in what follows; Online Appendix Table I), for men (Online Appendix Table II), and for women (Online Appendix Table III).

randomization. Furthermore, our model helps us think about externalities of a more effective job search for some on other unemployed workers searching for a job. We have no strong prediction for the impact on those who search on the job.

V. Basic Results: Program Take-up and Difference between Treatment and Control

V.A. *Participation and Services Received*

A first step is to establish what types of help the beneficiaries of the program actually received. We start by estimating the following equation with program take-up and measures of the services received by the youth while unemployed as the dependent variables.

$$y_{ic} = \alpha_1 + \beta_1 Z_{ic} + X_{ic}\gamma_1 + \epsilon_{ic} \tag{5}$$

y_{ic} is take-up of the program (enrolled or not), and measures of the types of help that individual i in city c received. Z_{ic} is a dummy equal to 1 if the individual is assigned to the program. X_{ic} is a vector of control variables that includes a set of quintuplet dummies, a dummy for each cohort of entry into the program, and individual-level control variables (age, gender, education, past duration of unemployment and its square).[19]

Panel A in Table III presents the impact of assignment to treatment on program participation. The randomization was adhered to, and participation in the control group was zero, but take-up in the treatment group was far from universal: it was only 35% for the full sample of workers assigned to treatment. Predictably, take-up was significantly higher for unemployed workers (43%) than for employed workers (25%). The follow-up survey asked why respondents did not participate (if they did not). Forty-six percent of those assigned to treatment who did not participate reported that they already had started or were about to start a job, and 11% claimed that they were studying. Only about 17% of respondents answered that they felt that the counseling program was useless or time-consuming.

Panel B in Table III presents coefficients β_1 for a number of intermediate outcomes, indicating the types of services received

19. More flexible control for past duration of unemployment makes no difference to the result. The results are also not affected by including no control variables.

TABLE III

Take-up and Intermediate Variables

Dependent variable	(1) All workers	(2) Not employed	(3) Employed
Panel A: Program participation			
Program participation	0.350***	0.434***	0.246***
	(0.008)	(0.009)	(0.008)
Panel B: Change in search productivity			
Number of meeting with a counselor	0.551***	0.601***	0.454***
	(0.059)	(0.083)	(0.064)
Control mean	2.497	3.444	1.361
Received help with CV, coaching for interviews, etc.	0.100***	0.113***	0.081***
	(0.007)	(0.009)	(0.009)
Control mean	0.213	0.285	0.126
Help with matching (identify job offers, help with transports)	0.009	0.008	0.010
	(0.006)	(0.008)	(0.006)
Control mean	0.153	0.199	0.099
Panel C: Employment outcomes			
Long-term fixed contract	0.007	0.017***	–0.003
	(0.005)	(0.006)	(0.008)
Control mean	0.2	0.16	0.247
Long-term employment	0.002	0.015	–0.012
	(0.007)	(0.010)	(0.009)
Control mean	0.468	0.365	0.593
Observations	21,431	11,806	9,625

Notes. The table reports ordinary least squares (OLS) regressions of several variables on program assignment, controlling for gender, education, past duration of unemployment and its square, cohort dummies, and 47 dummies for local area quintuplets (see equation (5)). All individuals assigned to treatment and control are pooled, irrespective of their type of area. In Panel C, the dependent variables are employment outcomes when surveyed eight months after the random assignment: long-term fixed contracts are fixed-term contracts with a length of at least six months; long-term employment is either a long-term fixed contract or an indefinite-term contract. Column (2) restricts the sample to job seekers who did not report that they were employed at the time of randomization; column (3) restricts the sample to those who did. Standard errors in parentheses are robust to heteroskedasticity and clustered at the local area level. ***indicate a 1% significance.

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

by job seekers (according to their self-reports from the endline interview). Overall, assigned workers had more meetings with a job search advisor (over the eight months after assignment) and received more help preparing their résumés and assessing their skills. Participants were not significantly more likely to have been put in touch with a specific employer, nor did they receive help with transportation to interviews. Overall, it seems that the program may have helped participants by motivating them to continue searching, rather than directly helping them jump the queue for specific jobs.[20]

V.B. Preliminary Results: Labor Market Outcomes

In a second step, we present naive estimates of the program, comparing assigned and unassigned workers, ignoring externalities. Throughout the article, we consider two labor market outcomes: fixed-term contract of six months or more (LTFC) and any long-term job (fixed-term contract of more than six months or permanent contract, LT). Both measures are potentially interesting: LTFC was the cheapest way for the intermediaries to satisfy their obligations, and hence the measure for which we may expect the largest direct impact. LT is what the government (and the employee) cares about, and to the extent the program led some beneficiaries to get fixed-term contracts instead of indefinite-term contract, it would not be a success.

The results of estimating equation (5) with these two measures of employment outcome are presented in Panel C of Table III. In this specification, all those assigned to treatment are pooled and compared to those assigned to the control group.

Overall, job seekers assigned to treatment are only 0.7 percentage point more likely to have obtained an LTFC and 0.2 percentage point to have an LT, and these estimates are completely insignificant. However, for those who were not employed at the beginning of the study, they were 1.7 percentage points (11%) more likely to have an LTFC and 1.5 percentage points to have an LT (4%) if they were assigned to treatment than if they were not. In Online Appendix Tables IV and V we present the results separately for men and for women. We find stronger effects for men only than in the entire sample: 2.6 percentage points for LTFC and 2.2 percentage points for LT for all workers. On the other hand, the point estimates of the effects for women are actually negative and insignificant for all workers, but positive and insignificant for the unemployed workers.

20. Online Appendix Tables IV and V give the results separately for men and women, with very similar results for both sexes: different take-up of the program thus do not explain the difference by sex that we find later.

VI. Estimating Externalities

As we noted, the estimates in the previous section are potentially biased estimates of the true effects of the program on participants in the presence of externalities. We now turn to examining externalities directly.

VI.A. Unconstrained Reduced Form

To estimate externalities, we take advantage of the fact that the fraction of treated job seekers varies by labor market (from 0% to 100%). In the absence of externalities, the outcomes both for assigned and unassigned workers should be independent of the fraction of workers assigned to the treatment in their areas. In contrast, negative externalities have two simple implications. First, the probability that eligible youth in the control group find a job should be lower in cities where others were assigned to treatment, and the negative impact should increase with π, the fraction of relevant workers who were treated.

Second, the net impact of the treatment (compared to the super-control) should fall as the fraction of workers assigned to the program rises (as the treated workers now compete among themselves for jobs).

We estimate a fully unconstrained reduced-form model and test whether the effect of being assigned to treatment or to control varies by assignment probability. The specification we consider is the following:

$$\begin{aligned} y_{ic} = {} & \beta_{25} Z_{ic} P_{25c} + \beta_{50} Z_{ic} P_{50c} + \beta_{75} Z_{ic} P_{75c} + \beta_{100} Z_{ic} P_{100_c} \\ & + \delta_{25} P_{25c} + \delta_{50} P_{50c} + \delta_{75} P_{75c} \\ & + X_{ic}\gamma_4 + u_{ic} \end{aligned} \tag{6}$$

where Z_{ic} is the assignment to treatment variable and P_{xc} is a dummy variable at the area level indicating an assignment rate of x%. ZP_{25} is thus a dummy for being assigned to treatment in a labor market with a rate of 25% assignment. As before, control variables are individual characteristics (gender, education, etc.) and the set of 47 dummy variables for city quintuplets (our randomization strata). Standard errors are clustered at the local area level. The parameter β_x measures the effect of being assigned to treatment in an area where x% of the eligible population was assigned to treatment, compared to being unassigned in an area of the same type (or, for β_{100}, compared to the super-control).

Coefficient δ_x measures the effect of being assigned to the control group in an area where $x\%$ of the eligible population was assigned to treatment, compared to being in the super-control group in which no one was assigned to treatment. Note that there are four parameters β but only three parameters δ as there is no room to estimate the effect on those assigned to the control group when the whole eligible population is assigned to receive the treatment.

There are two tests that can be used to investigate the presence of externalities based on estimates from the regression: (1) whether all the δ coefficients are jointly zero; (2) whether they are equal to each other (the alternative of interest being that they are declining).

Table IV presents estimates of equation (6), and Figure II is a graphical representation of these results (all the figures are for unemployed job seekers).

Figure II, Panel A shows the average probability that a worker who was unemployed at baseline has obtained a LTFC by the month-eight survey by city-level treatment group (0 to 100), for all workers pooled and separately for assigned and non-assigned workers within each city. We see that the mean for the treatment group is always above that for the control group, suggesting that there is a direct treatment effect. There is no clear evidence for externalities in this sample as a whole; however, the average employment in the control group seems to be unrelated to the fraction of workers assigned to the treatment. Figure II, Panel D shows that there may have been some externalities for men: the control group average appears to be lower in all areas were some workers were exposed to the treatment, and the pattern is generally decreasing.

Table IV presents the coefficients with standard errors, and associated tests for LTFC (columns (1) to (4)) and LT (columns (5) to (8)). Column (2) indeed suggests a positive treatment effect impact of being assigned to treatment compared to not being assigned in a treated area (the β coefficients are jointly significant), and insignificant externalities on untreated workers: the δ coefficients are neither significantly different from zero nor significantly different from each other. For men alone, we do see stronger evidence of externalities: in column (3), the δ coefficients are all negative and jointly significant (two out of three are also individually significant), although we cannot reject that they are constant in magnitude with the fraction assigned to treatment.

TABLE IV

REDUCED FORM: IMPACT OF PROGRAM ASSIGNMENT AND ASSIGNMENT PROBABILITY

	Labor market outcome: Long term fixed contract				Labor market outcome: Long term employment			
		Not employed				Not employed		
	(1) All workers	(2) All	(3) Men	(4) Women	(5) All workers	(6) All	(7) Men	(8) Women
Assigned to treatment in 25% areas	0.016	0.021	0.037	0.015	0.006	0.024	0.068**	0.002
	(0.012)	(0.014)	(0.027)	(0.016)	(0.016)	(0.021)	(0.032)	(0.027)
Assigned to treatment in 50% areas	0.009	0.013	0.021	0.008	–0.011	–0.005	–0.016	0.001
	(0.012)	(0.013)	(0.021)	(0.020)	(0.016)	(0.022)	(0.038)	(0.028)
Assigned to treatment in 75% areas	–0.015	0.007	0.061**	–0.016	0.025	0.039	0.059	0.026
	(0.016)	(0.019)	(0.030)	(0.021)	(0.020)	(0.028)	(0.046)	(0.035)
Assigned to treatment in 100% areas	0.010	0.025**	0.021	0.028**	0.001	0.020	0.000	0.034*
	(0.009)	(0.010)	(0.014)	(0.014)	(0.011)	(0.014)	(0.023)	(0.018)
25% areas	–0.002	–0.015	–0.041**	–0.001	–0.003	–0.012	–0.063***	0.015
	(0.010)	(0.011)	(0.019)	(0.013)	(0.012)	(0.015)	(0.024)	(0.020)
50% areas	–0.002	–0.014	–0.026	–0.005	–0.011	–0.026	–0.017	–0.032
	(0.010)	(0.013)	(0.018)	(0.017)	(0.013)	(0.018)	(0.027)	(0.023)
75% areas	0.016	–0.006	–0.055**	0.014	–0.026	–0.039	–0.06	–0.027
	(0.016)	(0.020)	(0.027)	(0.024)	(0.019)	(0.025)	(0.041)	(0.032)

(continued)

TABLE IV
(CONTINUED)

	Labor market outcome: Long term fixed contract				Labor market outcome: Long term employment			
		Not employed				Not employed		
	(1) All workers	(2) All	(3) Men	(4) Women	(5) All workers	(6) All	(7) Men	(8) Women
Control mean	0.199	0.167	0.150	0.178	0.473	0.376	0.396	0.364
F-test for $\beta_{25} = \beta_{50} = \beta_{75} = \beta_{100} = 0$	0.34	0.05	0.07	0.22	0.71	0.27	0.19	0.43
F-test for $\delta_{25} = \delta_{50} = \delta_{75} = 0$	0.72	0.48	0.04	0.92	0.53	0.27	0.04	0.19
F-test for $\delta_{25} = \delta_{50} = \delta_{75}$	0.52	0.90	0.59	0.77	0.52	0.51	0.28	0.09
Number of observations	21431	11806	4387	7419	21431	11806	4387	7419

Notes. The table reports OLS regressions of employment outcome variables on program assignment dummies interacted with area assignment probability dummies (see equation (6)). Areas with 0% treatment are the reference. For the list of controls and the definition of the outcomes, see Table III. The dependent variables are measured when surveyed eight months after the random assignment. Columns (2)–(4) and (6)–(7) restrict the sample to job seekers who did not report that they were employed at the time of randomization. Only p-values for the F-tests are reported. Standard errors in parentheses are robust to heteroskedasticity and clustered at the local area level.

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

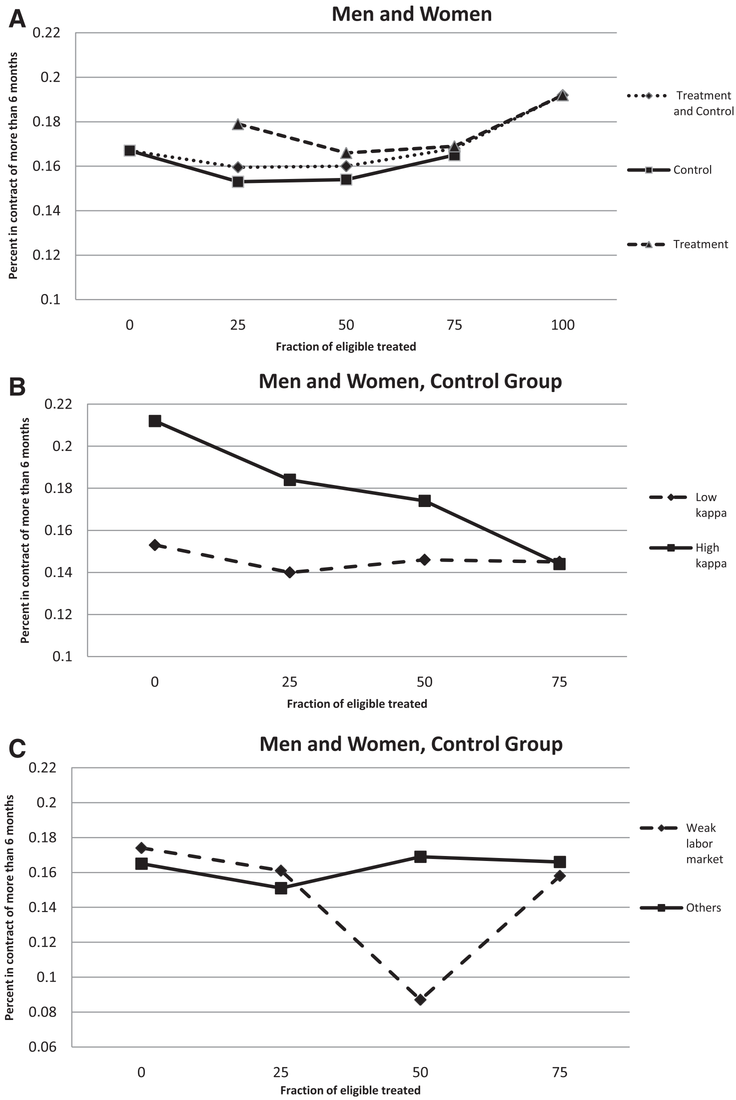

FIGURE II

Average Employment Rate, per Group

These figures plot the (unadjusted) average fraction of workers who were employed in a long-term fixed contract eight months after program assignment in the different groups for several categories of treatment and control groups. In Panels B and C, only data on the unassigned worker are used. In Panel B, the low (high) kappa (κ) is for the occupations where the fraction of eligible workers among job seekers in this occupation is in the bottom (top) quartile (see text for details). In Panel C, "weak labor markets" are later cohorts, in regions with unemployment rate above average for the period.

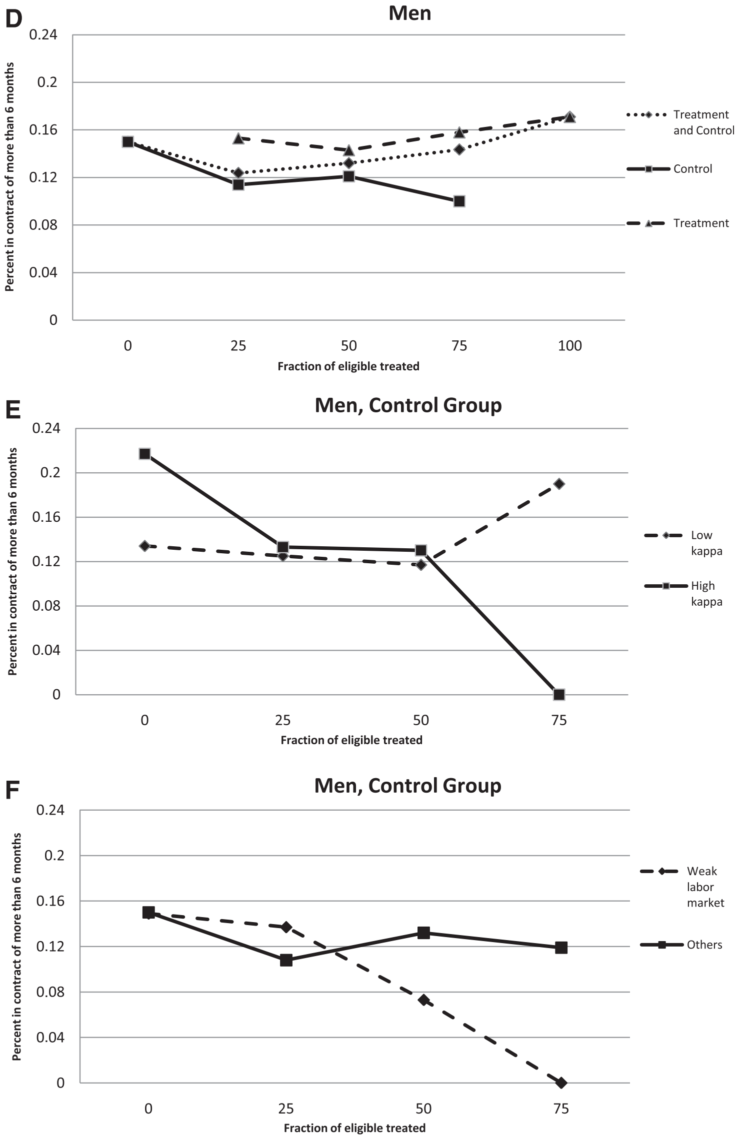

FIGURE II

Continued

This difference between men and women remains throughout the article. It was not something we expected ex ante, and we do not have any solid explanation for it, although we discuss additional analysis we performed to shed light on this phenomenon.

VI.B. Pooled Reduced Form

Due to the relatively low take-up (and hence the relatively small direct reduced-form impact of program assignment on the probability to find a job), and the fact that a sizable fraction of the target sample was in fact already employed when the experiment started, the power of the experiment to detect difference between cities with different assignment is relatively low. Moreover, the average κ (the share of eligible among all young job seekers) is only 19%, which implies that the difference in share of treated between a zone treated at 75% and a zone treated at 25% is only 19*75% – 19*25% = 9.5%. As a result, even for men alone (where we do find a significant negative impact of being in a treated labor market, for example, and where the pattern has generally the right shape) we cannot reject equality between the dummies indicating different assignment rules. Because our next tests involve subsamples, this further affects power.

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

The reduced-form specification is:

$$y_{ic} = \alpha_2 + \beta_2 Z_{ic} P_c + \delta_2 P_c + X_{ic}\gamma_2 + \omega_{ic} \tag{7}$$

where P_c is a dummy for being in any treatment area (i.e., an area with positive share treated). In this specification, β_2 is the difference between those assigned to treatment (whether treated or not) and those who are in treatment zones but are not themselves assigned to treatment. δ_2 is the effect of being untreated in a treated zone (compared to being untreated in an untreated zone). The sum $\beta_2 + \delta_2$ is the effect of being assigned to treatment (compared to being in an entirely unaffected labor market).

Table V presents the estimates of equation (7). In each panel, the first row presents the estimate of β_2, the second row the estimate of δ_2, and the third row the estimate of $\beta_2 + \delta_2$. Columns (1) to (3) present the results for the full sample of those not employed at baseline, for men and for women, respectively. The results are consistent with those in Table IV. For example, column (1) in Panel A finds that those assigned to treatment are 2.3 percentage points more likely to have an LTFC that those assigned to control in the treatment labor markets. This is roughly the average of the coefficients in the first four rows of column (3), Table IV (0.021, 0.013, 0.007, 0.025), with a stronger weight given to the last number because there are more people assigned to treatment in these zones. Those assigned to control in a treated labor market are 1.3 percentage points less likely to have found a fixed-term contract than those assigned to the super-control (insignificant). This number corresponds roughly to the average of the number of the last three rows of Table IV (–0.15, –0.14, –0.006). This time, more weight is given to the first of these numbers because there are more individuals who are untreated in these zones.

Overall, the conclusions from columns (1) to (3) of Table V are similar to those of Table IV, with significant estimates of externalities for men but not for women. The only difference is that we find some limited evidence (significant at 10%) of negative externalities for men and women combined for LT. One striking result in this table is that the net effects of being assigned to treatment are all insignificant, suggesting that when we take externalities into account, the program is actually ineffective *for those assigned to it*.

As mentioned, the heterogeneity between both the impacts and the externalities for men and women is a surprising result. The literature on labor market interventions (job search assistance or monitoring) for women has not generally found this result: half the papers over the period (1999–2006) find larger impacts for women, and the other half finds little difference (Bergemann and van den Berg, 2008). In a context similar to ours, Dolton and O'Neill (2002) find a larger impact for women than for men. In Online Appendix Table VII, we shed more light on the pattern by disaggregating by education level (less than two years of higher education versus more than two years). Interestingly, in the low education group, the pattern is the same for men and women. It is only in the high education group

TABLE V

REDUCED FORM: IMPACT OF THE PROGRAM, ACCOUNTING FOR EXTERNALITIES (**BY JOB TYPE: SHARE OF JOB SEEKERS WHO ARE ELIGIBLE FOR PROGRAM**)

	Not employed			Not employed, above third quartile		
	(1) All	(2) Men	(3) Women	(4) All	(5) Men	(6) Women
Panel A: Long-term fixed contract						
Assigned to program (β)	0.023***	0.043***	0.013	0.040**	0.072**	0.021
	(0.008)	(0.013)	(0.010)	(0.016)	(0.029)	(0.022)
In a program area (δ)	–0.013	–0.036***	–0.001	–0.040*	–0.086**	–0.013
	(0.009)	(0.013)	(0.012)	(0.021)	(0.035)	(0.027)
Net effect of program assignment ($\beta+\delta$)	0.010	0.007	0.012	0.000	–0.014	0.008
	(0.008)	(0.011)	(0.011)	(0.019)	(0.031)	(0.024)
Control mean	0.16	0.131	0.177	0.19	0.161	0.204
Panel B: Long-term employment						
Assigned to program (β)	0.025**	0.037**	0.019	0.019	0.059	0.000
	(0.012)	(0.018)	(0.014)	(0.021)	(0.039)	(0.028)
In a program area (δ)	–0.021*	–0.043**	–0.010	–0.005	–0.081*	0.033
	(0.013)	(0.020)	(0.018)	(0.023)	(0.047)	(0.032)
Net effect of program assignment ($\beta+\delta$)	0.003	–0.006	0.009	0.014	–0.022	0.033
	(0.011)	(0.018)	(0.016)	(0.019)	(0.037)	(0.026)
Control mean	0.365	0.372	0.36	0.403	0.408	0.401
Observations	11,806	4,387	7,419	3,066	1,016	2,050

Notes. The table reports OLS regressions of employment outcome variables on a dummy for assigned to the program and a dummy for being in a local area with positive assignment probability (see equation (6)). For the list of controls and the definition of the outcomes, see Table III. The dependent variables are measured when surveyed eight months after the random assignment. The sample is restricted to job seekers who did not report that they were employed at the time of randomization. Columns (1)–(3) include job seekers searching for all kinds of jobs; columns (4)–(6) include only job seekers searching for jobs in which the share of skilled job seekers (κ) is above the third quartile in the distribution of jobs. Standard errors in parentheses are robust to heteroskedasticity and clustered at the local area level. ***indicate a 1% significance (**5% and *10%).

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

that the results are different for men and women, with large direct effects and large externality for men and nothing for women. This helps reconcile our results with those in the literature, which are typically not focused on college graduates. This stills leaves open the question of why this difference exists at higher education levels. We explored two channels for the difference in externalities between men and women in the high education group, marital status and type of occupation sought. The results are not different when breaking down by marital status. Online Appendix Table VIII accounts for the different occupations sought by men and women in the high education group by reweighting the observations for each gender and in each occupation by the share of individuals of the other gender who are searching in this occupation. This answers the question: what would be the results for men (women) if their occupational pattern was the same as women (men). This reweighting makes little difference for women. For men, however, the externalities for LT disappear with the reweighting. This gives some hint that the difference may be partly accounted for by different occupations. This is very tentative, however, because the results are unaffected for LTFC and assigning the occupations of men to women does not affect the results for women.

VI.C. Heterogeneity: Sector and Labor Market Conditions

The model suggests that the size of the externality imposed on any given worker depends on the fraction of workers in the market that are not assigned to treatment. This share depends in turn on the fraction of those who are eligible and are assigned and on the fraction of the overall labor market that is eligible. This is because any labor market externalities due to the treatment may affect not only the eligible group but also workers who are close substitutes for them, although they are not themselves part of the experiment. For example, young, educated individuals who have been unemployed for at least six months may be competing for jobs with all young job seekers, or only with young, educated job seekers with a slightly shorter duration of unemployment. Unfortunately, looking for externalities among the rest of the unemployed population is not feasible due to data constraints.[21]

21. For reasons detailed in footnote 8, the unemployment registers are not considered to be a good measure of unemployment, and national statistics on employment use the *enquête emploi*, a survey similar to the Current Population Survey.

However, this implies that the externalities should be larger in sectors where eligible workers form a large part of the relevant labor market. Thus, to investigate the displacement issue, we split the sample according to κ, the share of eligible workers in the sector where they are looking for a job. We compute κ as follows: when they first register at the ANPE, job seekers indicate the occupation in which they are looking for a job. There are 466 such occupation codes. Using a nationwide database of job seekers, we compute the share of skilled job seekers among all job seekers under age 30 searching in this occupation. Column (1) in Online Appendix Table VI lists the 10 categories in which the share of skilled workers is the highest (high κ) and the 10 categories in which the share of skilled workers is the lowest (low κ), along with the corresponding shares. Column (2) presents the same fractions for the same job but keeps the long unemployment duration requirement. Low values of κ are found for workers in industrial jobs that require vocational education (often below the college level), such as construction workers (roofers, concrete workers, and sheet fitters). The highest concentration of young job seekers with at least two-year college degrees is found for workers in tertiary occupations, such as lawyers, financial officers, teachers, and dentists.[22]

Figure II, Panel B (and Panel E for men only) presents the average fraction of control workers with an LTFC in two types of labor markets: those with κ below the first quartile (eligible workers are a small fraction of the job seekers in the job they are looking for) and those with κ above the third quartile. As expected, the slope is steeper for κ above the third quartile (and is completely flat for κ below the first quartile). Thus, for control

However, the sample for *enquête emploi* is too small to have precise number of unemployment at a fine geographic level. Later, when we use the *enquête emploi* to construct indicators of unemployment rate in each labor market, we assign each of our labor market to the larger regions at which the data are representative.

22. It is important to define the groups by the sectors in which those in our sample were initially looking for a job, and to use administrative data from before the experiment, because the experiment could have affected both the decision of where to look and the reporting on what they were looking for. In practice, people tend to revise their expectation downward as they look for a job. Nevertheless, there is a correlation between the κ in the job initially sought and the κ in the job finally found: 22% (34%) of those who were looking for a job with κ above the third quartile ultimately found a job with κ above the third quartile (median). For those who were initially looking for a job with κ in the first quartile, the numbers are, respectively, 1% and 10%.

workers looking for jobs where they compete mainly with eligible workers, the higher the fraction treated, the worst they fare. This is not true for those who compete with a much larger pool.

The last three columns of Table V present the results of the pooled regression, restricting the sample to those looking jobs where κ is above the third quartile. In Panel A (LTFC), the externality is about three times larger for high κ than for the whole sample and is significant in the population as a whole. This is still driven by men, though there is a negative point estimate for women (which is still insignificant). In Panel B (LT), the externalities are twice as large for men above the third quartile of κ than for the sample as a whole, but the number is positive (and insignificant) for women, which leads the point estimate for the whole sample to actually be lower in column (4) (high κ) than in column (1) (all workers).[23]

Finally, the model has the testable implication that externalities should be larger in weak labor markets. We define a weak labor market by the interactions of being in a generally depressed area (labor market area with unemployment rate above the median, as measured in the *enquete emploi*[24]), and being in one of the later cohort (entered program eligibility between April and July 2008) who were looking for employment after the 2008 recession started. The unemployment rate in these markets at the time our survey was conducted (eight months after entry in the program) was 11.0%, versus 8.2% in the rest of the sample.

Panels C (for all the unemployed) and F (for men only) in Figure II plot average unemployment rates for men among those unassigned to treatment by fraction assigned, separating the sample between weak labor market and strong labor market. In normal conditions, there is no evidence of externalities. However, in weak labor markets, the control group means are lower in all the cities that have some workers assigned to the treatment than in super-control. For men, the pattern is clearly declining in both graphs. For all workers together, there is a surprising blip up in the 75% group (note that both for men and for all workers, the sample gets quite small).

We build the regression framework around our pooled estimates, by interacting both the treatment dummy and the dummy

23. The results for κ below the third quartile are all insignificant.

24. As mentioned, the *enquête emploi* only gives us more aggregated regional data, so we assign each labor market to the corresponding region.

that indicates that a labor market was treated with an interaction of a bad labor market dummy (which varies at the regional level) and a bad cohort dummy.

$$y_{ic} = \alpha_3 + \beta_3^{LL}(Z_{ic}P_c * LL_{ic}) + \beta_3(Z_{ic}P_c * (1 - LL_{ic})) + \delta_3^{LL}(P_c * LL_{ic}) + \delta_3(P_c * (1 - LL_{ic})) + X\gamma_3 + \nu_{ic} \quad (8)$$

where Z_{ic} and P_c are defined as before (Z_{ic} is assignment to treatment and P_c indicate that the area is treated), and $LL_{ic} = LC_i * LM_c$ is a dummy equal to 1 in areas with high unemployment rate ($LM_c = 1$) and for the last cohorts ($LC_i = 1$) and 0 otherwise. LL_{ic} is included in the list of control variables X.

Externalities may vary across cohorts or regions for reasons that are not directly linked to labor market conditions. For example, the effectiveness of the program or the intensity of search efforts may have changed over time. Operators may have become better at assisting in job searches or, on the contrary, may have lost interest. Alternatively, they may have transferred their knowledge on ineligible workers. Finally, operators who bid in weak labor markets may be different than those who bid in strong labor markets.[25]

To test this, we estimate externalities separately in all combinations of cohorts and regions.

$$\begin{aligned} y_{ic} = \alpha_4 &+ \beta_4^{LL}(Z_{ic}P_c * LC_i * LM_c) + \beta_4^{HL}(Z_{ic}P_c * HC_i * LM_c) \\ &+ \beta_4^{LH}(Z_{ic}P_c * LC_i * HM_c) + \beta_4^{HH}(Z_{ic}P_c * HC_i * HM_c) \\ &+ \delta_4^{LL}(P_c * LC_i * LM_c) + \delta_4^{HL}(P_c * HC_i * LM_c) \\ &+ \delta_4^{LH}(P_c * LC_i * HM_c) + \delta_4^{HH}(P_c * HC_i * HM_c) + X\gamma_4 + \nu_{ic} \end{aligned} \quad (9)$$

where LC_i and LM_c are defined as before, HC_i is a dummy equal to 1 in strong cohorts ($HC_i = 1 - LC_i$), and HM_c is a dummy equal to 1 in areas with strong labor markets ($HM_c = 1 - LM_c$). In this specification, the implication of the theory is that δ_4^{LL} is more negative than all the other δ_4 coefficients. The identification assumption is that to the extent that there are differences in externalities across regions and periods, the recession is the only reason

25. For example, we find that the for-profit operators appear to be more effective at placing the eligible workers than the not-for-profit workers, and the externalities are correspondingly larger in regions where they won. They may also have been working in different labor markets.

they are particularly high in worst years in depressed regions, compared to other times or places. The full set of interactions of variables P_c and LC_i is included in the list of control variables X.

The results are presented in Table VI. In Panel A, we compare externalities in weak labor markets and in all the others. In Panel B, we separately estimate all the coefficients in all four regions and cohort combinations. Consistent with the model, we observe in Panel A significant externalities in the weakest markets and no externalities elsewhere. In Panel B, we see that the effect of the interaction is not driven by the bad areas only or the bad cohorts only. In both specifications, we can reject equality of the externalities in weak markets with the other coefficients for LT, but not for LTFC. These results make sense, because it seems sensible that the externalities would be present not only to fixed-term employment but to all kinds of long-term contracts (fixed-term and indefinite).

VI.D. Other Outcomes and Longer Term Results

We have so far focused on the short term and the main outcomes of interest for the program, LT, and LTFC. Job placement agencies were contractually incentivized to help beneficiaries find jobs within a maximum of 6 months, so the direct effects of the program are expected to disappear after 12 months. Because all the gains were in the form of fixed-term contracts, the gains could well have been temporary. However, a key rationale for such job placement policies is the idea that a young person's first job serves as a stepping stone, helping her find subsequent employment after her first contract ends (or move from having a six-month contract with a firm to a more permanent position). To investigate the persistence of program impacts, we conducted surveys at 12, 16, and 20 months after treatment assignment.

Columns (1) to (3) in Table VII show the impact of the program and the externalities on any form of employment (including short-term temporary contract), at 8, 12, and 20 months.[26] Columns (4) to (6) show estimates of equation (7) for long-term employment at 8, 12, and 20 months. In the short term, the program gave a small advantage to those assigned in the probability of finding any job (overall they are 1.9 percentage points—or 4%—more likely to find a job than those who were not assigned). The externalities are negative and of the same magnitude,

26. We omit 16 months because the results are the same as for 12 or 20 months.

TABLE VI

HETEROGENEITY OF PROGRAM EFFECT BY AREA AND COHORT

	LT FC	LT Empl.	LT FC	LT Empl.	LT FC	LT Empl.
	All		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Assigned to program (β) bad area, bad cohort	0.055***	0.066***	0.082***	0.110***	0.036	0.036
	(0.018)	(0.023)	(0.030)	(0.036)	(0.025)	(0.029)
Assigned to program (β) good area or good cohort	0.015*	0.015	0.033**	0.019	0.007	0.014
	(0.008)	(0.013)	(0.015)	(0.021)	(0.011)	(0.015)
In a program area (δ_1) bad area, bad cohort	–0.042*	–0.077**	–0.043	–0.144***	–0.041	–0.035
	(0.024)	(0.030)	(0.032)	(0.044)	(0.031)	(0.041)
In a program area (δ_2) good area or good cohort	–0.009	–0.009	–0.036**	–0.017	0.007	–0.006
	(0.010)	(0.014)	(0.015)	(0.024)	(0.014)	(0.020)
Test ($\delta_1 = \delta_2$)	0.202	0.05	0.867	0.017	0.178	0.533
Panel B						
In a program area (δ_1) bad area, bad cohort	–0.041*	–0.078***	–0.042	–0.146***	–0.039	–0.036
	(0.024)	(0.030)	(0.032)	(0.043)	(0.031)	(0.041)
In a program area (δ_2) good area, bad cohort	–0.024	0.044	–0.047	0.010	–0.008	0.069*
	(0.019)	(0.028)	(0.032)	(0.043)	(0.028)	(0.041)
In a program area (δ_3) bad area, good cohort	0.011	–0.038	–0.027	–0.060	0.032	–0.025
	(0.017)	(0.025)	(0.026)	(0.038)	(0.023)	(0.033)
In a program area (δ_4) good area, good cohort	–0.020	–0.015	–0.039	0.010	–0.011	–0.032
	(0.019)	(0.024)	(0.027)	(0.046)	(0.027)	(0.033)
Test ($\delta_1 = \delta_2 = \delta_3 = \delta_4$)	0.303	0.022	0.965	0.024	0.337	0.185
Mean super-control	0.167	0.376	0.15	0.396	0.178	0.364
Observations	11,806		4,387		7,419	

Notes. The table reports OLS regressions of employment outcome variables on a dummy for assigned to the program and a dummy for being in a local area with positive assignment probability, interacted with bad/good area and bad/good cohort dummies (see equation (9)). In Panel B, estimates related to program assignment are not reported for concision. Bad areas are those with an average unemployment rate during first semester of 2007 that is above the median of our areas. Bad cohorts are cohorts 8–11, that is, entering the experiment in April to July 2008. For the list of controls and the definition of the outcomes, see Table III. The dependent variables are measured when surveyed eight months after the random assignment. The sample is restricted to job seekers who did not report that they were employed at the time of randomization. Only p-values for the equality tests are reported. Standard errors in parentheses are robust to heteroskedasticity and clustered at the local area level. ***indicate a 1% significance (**5%, *10%).

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

TABLE VII

SHORT-TERM VS. MEDIUM-TERM IMPACTS ON EMPLOYMENT OUTCOMES, ACCOUNTING FOR EXTERNALITIES

	Any employment			Long-term employment		
	(1) All	(2) Men	(3) Women	(4) All	(5) Men	(6) Women
Panel A: 8 months						
Assigned to program (β)	0.019*	0.026	0.017	0.025**	0.037**	0.019
	(0.011)	(0.017)	(0.014)	(0.012)	(0.018)	(0.014)
In a program area (δ)	–0.020	–0.013	–0.026	–0.021*	–0.043**	–0.010
	(0.013)	(0.021)	(0.018)	(0.013)	(0.020)	(0.018)
Net effect of program	–0.001	0.014	–0.010	0.003	–0.006	0.009
assignment ($\beta + \delta$)	(0.011)	(0.019)	(0.015)	(0.011)	(0.018)	(0.016)
Control mean	0.487	0.490	0.486	0.365	0.372	0.360
Observations	11,806	4,387	7,419	11,806	4,387	7,419
Panel B: 12 months						
Assigned to program (β)	0.015	0.020	0.012	0.010	0.021	0.004
	(0.012)	(0.020)	(0.014)	(0.012)	(0.019)	(0.015)
In a program area (δ)	–0.025*	–0.007	–0.036**	–0.001	0.006	–0.006
	(0.014)	(0.024)	(0.017)	(0.014)	(0.024)	(0.018)
Net effect of program	–0.009	0.013	–0.024	0.009	0.027	–0.002
assignment ($\beta + \delta$)	(0.013)	(0.021)	(0.015)	(0.012)	(0.021)	(0.015)
Control mean	0.560	0.556	0.563	0.454	0.447	0.458
Observations	10,263	3,792	6,471	10,263	3,792	6,471
Panel C: 20 months						
Assigned to program (β)	–0.010	–0.015	–0.007	–0.014	–0.022	–0.009
	(0.011)	(0.018)	(0.015)	(0.012)	(0.018)	(0.016)
In a program area (δ)	0.009	0.030	–0.002	–0.004	0.007	–0.010
	(0.012)	(0.020)	(0.016)	(0.013)	(0.022)	(0.017)
Net effect of program	–0.001	0.015	–0.009	–0.018*	–0.014	–0.019
assignment ($\beta + \delta$)	(0.010)	(0.017)	(0.013)	(0.011)	(0.019)	(0.013)
Control mean	0.654	0.643	0.660	0.576	0.567	0.580
Observations	9,809	3,619	6,190	9,809	3,619	6,190

Notes. The table reports OLS regressions of employment outcome variables on a dummy for assigned to the program and a dummy for being in a local area with positive assignment probability (see equation (6)). For the list of controls, see Table III. The dependent variable in columns (1)–(3) is any sort of employment (including short-term temporary contract); in columns (4)–(6) it is either a long-term fixed contract or an indefinite-erm contract. In Panel A, outcomes are measured when surveyed 8 months after the random assignment; in Panel B, 12 months; and in Panel C, 20 months. The sample is restricted to job seekers who did not report that they were employed at the time of randomization. Standard errors in parentheses are robust to heteroskedasticity and clustered at the local area level. ***indicate a 1% significance (**5%, *10%)

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

though insignificant, and there is no net positive impact of assignment to the program. Over time, more and more of the control group workers find a job (65.4% by 20 months), and the difference between assigned and nonassigned disappears. Likewise, any difference between assigned and nonassigned in the probability to find stable employment disappears over time.

Table VIII presents the impacts of the program on total earnings (including 0 values for those who earn nothing, and unemployment benefits for those who receive them), at eight months and beyond. The effects on wages could theoretically be positive or negative in the short or long run. Although our model predicts no impacts on wages (and impact on earnings coming only from impacts on wages), the impact could have been negative if treated workers were encouraged to quickly accept low-quality jobs, rather than wait for something better. Alternatively, the effect could have been positive if programs helped individuals find better job matches. Overall, however, there appears to have been no significant treatment effect (or externalities) on wages. There is a positive effect on earnings for just men at eight months (which is attributable to the increase in the probability to be employed), but not beyond. Online Appendix Table IX presents the results for wages, to be taken with some caution since wages is a selected outcome.[27]

VII. Instrumental Variable Estimates of Program Impact

The estimates presented herein are of direct policy interest in our context, because the policy was to *offer* access to the reinforced program, not constrain eligible youth to participate. The reduced-form estimates are also sufficient to estimate externalities. However, the impact of the program on those actually participating is also of interest. In particular, the parameter of interest in the model is the relationship between the direct impact of program participation and the externality.

27. The absence of an effect on wages also helps distinguish the model we propose from an alternative model. In a standard search model, wage bargaining can lead to employment externalities. Cahuc and Le Barbanchon (2010) develop a model in which externalities on the nontreated arise from wage bargaining: a raise in treated labor market prospects increases bargained wages and decreases overall job creation. However, if this mechanism was at work, we would expect to observe impacts on both employment and wages for the treated.

TABLE VIII
SHORT-TERM VS. MEDIUM-TERM IMPACTS ON EARNINGS, ACCOUNTING FOR EXTERNALITIES

	(1) All	(2) Men	(3) Women
Panel A: 8 months			
Assigned to program (β)	20.354	132.289**	–37.278
	(30.465)	(66.149)	(42.585)
In a program area (δ)	7.509	–48.023	34.313
	(34.640)	(66.342)	(42.490)
Net effect of program assignment ($\beta + \delta$)	27.862	84.265	–2.965
	(29.957)	(71.066)	(21.326)
Control mean	914	947	896
Observations	11,806	4,387	7,419
Panel B: 12 months			
Assigned to program (β)	–13.493	–37.477	4.130
	(42.149)	(88.484)	(42.995)
In a program area (δ)	10.810	41.019	–3.929
	(46.747)	(97.210)	(43.737)
Net effect of program assignment ($\beta + \delta$)	–2.683	3.542	0.200
	(37.912)	(74.709)	(38.280)
Control mean	996	1073	952
Observations	10,263	3,792	6,471
Panel C: 20 months			
Assigned to program (β)	–12.070	13.060	–30.028
	(31.494)	(62.019)	(34.207)
In a program area (δ)	–28.488	53.386	–69.507
	(45.083)	(71.864)	(57.588)
Net effect of program assignment ($\beta + \delta$)	–40.557	66.446	–99.535*
	(42.335)	(73.690)	(52.295)
Control mean	1072	1103	1054
Observations	9,809	3,619	6,190

Notes. The table reports OLS regressions of earnings on a dummy for assigned to the program and a dummy for being in a local area with positive assignment probability (see equation (6)). Earnings are as declared in the survey, including income from transfers. For the list of controls, see Table III. In Panel A, outcome is measured when surveyed 8 months after the random assignment; in Panel B, 12 months; and in Panel C, 20 months. The sample is restricted to job seekers who did not report that they were employed at the time of randomization. Standard errors in parentheses are robust to heteroskedasticity and clustered at the local area level. ***indicate a 1% significance (**5%, **10%).

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

Because participation was endogenous, a natural strategy is to instrument program participation with assignment to the program, that is, to estimate the following equation where program participation (T_{ic}) is instrumented by assignment to the program

(Z_{ic}) and all the other variables are treated as exogenous and included in the instruments set:

$$y_{ic} = \alpha_5 + \beta_5 T_{ic} P_c + \delta_5 P_c + X\gamma_5 + \nu_{ic} \tag{10}$$

where β_5 compares treated to other untreated eligible workers in treated areas.

To estimate the overall effect of the treatment on the treated, the treated are compared to those in the super-control group. We estimate the following instrumental variables (IV) equation using treatment assignment as an instrument for program participation, and the other variables as instruments for themselves:

$$y_{ic} = \alpha_6 + \beta_6 T_{ic} P_c + \delta_6 P_c(1 - T_{ic}) + X\gamma_6 + \xi_{ic} \tag{11}$$

The presence of externalities, however, complicates the interpretation of β_5 and β_6. Specifically, we now show that on top of the usual monotonicity and independence assumptions (there is no direct effect of program assignment on eligible youths' behavior, other through any externality they may suffer and the fact that being assigned makes them more likely to receive the treatment), another necessary assumption in this context is that the externality on an untreated worker is independent of his treatment status. In other words, we need to assume that the potential outcomes when untreated are on average the same for the compliers and the noncompliers of our experiments (the potential outcomes when treated are, of course, allowed to vary).

To see this, consider the simple case in which areas are randomly assigned to a probability of treatment P that is either positive or 0, and individuals in the "treatment" area are randomly assigned to the treatment. Assume for simplicity, and as is the case in our context, that individuals assigned to the control group are never treated (so, in the notation of Imbens and Angrist (1994), $T(0) = 0$ and $T = T(1)Z$).

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

The observed outcome is then simply:

$$\begin{aligned} y &= y(0,0)(1-P) + y(1,0)P(1-T) + y(1,1)PT \\ &= y(0,0) + (y(1,0) - y(0,0))P + (y(1,1) - y(1,0))PT \end{aligned}$$

Then we have:

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

What IV identifies is (1) $AE = E(y(1,0) - y(0,0))$, which is the average externality over the population, and (2) the "treated in treated zone" effect, $TTZ = E(y(1,1) - y(1,0)|T(1) = 1)$.[28] 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):

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

Meanwhile, the average externality can be expressed as:

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

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

A natural question is whether the assumption that the externality is the same for everybody is reasonable. It could be violated if, for example, the compliers in the experiment chose to be treated because they worried about externalities, or if compliers are the type of people who search harder for a job and would thus have suffered more from the externalities if not treated (since externalities are multiplicative in the search effort).

These caveats notwithstanding, Table IX presents the results, which suggest fairly large net impacts of the program for people who actually take it up. Compared to the super-control, the estimate suggest that it increased the probability that they get an LTFC by 4 percentage points (25%), and LT by 3.6

28. Note that the treatment effect here subsumes any externality that the treated workers impose on each others. The externality is defined as the externality imposed on a nontreated worker in a treated area.

TABLE IX

EFFECT OF THE TREATMENT, ACCOUNTING FOR EXTERNALITIES (BY JOB TYPE: SHARE OF JOB SEEKERS WHO ARE ELIGIBLE FOR THE PROGRAM)

	Not employed			Not employed, above third quartile		
	(1) All	(2) Men	(3) Women	(4) All	(5) Men	(6) Women
Panel A: Long-term fixed contract						
Program participation (β)	0.054***	0.095***	0.030	0.094**	0.176**	0.048
	(0.018)	(0.030)	(0.023)	(0.039)	(0.074)	(0.052)
In a program area (δ)	–0.014	–0.036***	–0.001	–0.041*	–0.088**	–0.014
	(0.009)	(0.014)	(0.012)	(0.021)	(0.036)	(0.028)
Net effect of program participation ($\beta+\delta$)	0.040***	0.060***	0.029	0.053	0.088	0.035
	(0.014)	(0.023)	(0.019)	(0.033)	(0.062)	(0.042)
Control mean	0.16	0.131	0.177	0.19	0.161	0.204
Panel B: Long-term employment						
Program participation (β)	0.057**	0.083**	0.044	0.044	0.145	0.000
	(0.027)	(0.041)	(0.034)	(0.049)	(0.096)	(0.064)
In a program area (δ)	–0.021*	–0.043**	–0.010	–0.005	–0.083*	0.033
	(0.013)	(0.020)	(0.018)	(0.024)	(0.048)	(0.033)
Net effect of program participation ($\beta+\delta$)	0.036	0.040	0.034	0.039	0.063	0.033
	(0.022)	(0.034)	(0.029)	(0.038)	(0.075)	(0.049)
Control mean	0.365	0.372	0.36	0.403	0.408	0.401
Observations	11,806	4,387	7,419	3,066	1,016	2,050

Notes. The table reports IV regressions of employment outcome variables on a dummy for participation into the program and a dummy for being in a local area with positive assignment probability (see equation (10)). Participation into the program is instrumented by assignment to the program. For the list of controls and the definition of the outcomes, see Table III. The sample is restricted to job seekers who did not report that they were employed at the time of randomization. Columns (1)–(3) include job seekers searching for all kinds of jobs; columns (4)–(6) include only job seekers searching for jobs in which the share of skilled job seekers (κ) is above the third quartile in the distribution of jobs. Standard errors in parentheses are robust to heteroskedasticity and clustered at the local area level. ***indicate a 1% significance (**5%, *10%).

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

percentage points (9.8%). Compared to others in the same labor markets, the program increases the chance that participants get an LTFC by 5.4 percentage points and that of getting an LT by 5.7 percentage points. The estimates of externalities are of course unaffected, so the treatment effects are now quite a bit larger than the externalities, which is exactly what we would expect.

The reason the net impact of program assignments are 0 while the net treatment effect is positive is because the take-up is fairly low: those who are assigned but do not take up suffer the same externalities as those who are not assigned. The impact on them is sufficiently negative to compensate for the positive impact on those who are treated.

The difference between the IV and reduced-form results has important implications: a study in which compliance rates were high due to intense follow-up would greatly overestimate the effect of a policy where the program is offered, but compliance is not enforced for the target group if one would then just scale down the effect by the expected take-up under a voluntary program. This is because it would not take into account the potential negative externalities on the never-takers in such a program.

Table X presents the results of the estimation of externalities and the direct program effects in weak labor markets compared to other times. As in Table VI, we find larger externalities and larger treatment effect in weak labor markets. In weak labor market, the effect of participation is 12 percentage points for LTFC and 14.5 percentage points for LT, and the externalities, as before, are 4.2 percentage and 7.7 percentage points, respectively. In normal times, the externalities are not significantly different from zero, and the treatment effects are 3.5 and 3.6 percentage points, respectively.

VIII. Conclusion

This evaluation of an assistance program for young, college-educated job seekers offers a unique opportunity to measure both the direct effect of counseling and the equilibrium effects, in a given market, of providing counseling to a proportion of the eligible population.

TABLE X

HETEROGENEITY OF THE EFFECT OF PROGRAM PARTICIPATION, BY AREA AND COHORT

	All		Men		Women	
	LT FC	LT Empl.	LT FC	LT Empl.	LT FC	LT Empl.
	(1)	(2)	(3)	(4)	(5)	(6)
Program participation (β_1) bad area, bad cohort	0.123***	0.145***	0.177***	0.239***	0.081	0.081
	(0.041)	(0.050)	(0.068)	(0.081)	(0.056)	(0.066)
Program participation (β_2) good area or good cohort	0.036*	0.035	0.073**	0.043	0.017	0.034
	(0.019)	(0.030)	(0.034)	(0.047)	(0.025)	(0.037)
In a program area (δ_1) bad area, bad cohort	–0.042*	–0.076**	–0.044	–0.146***	–0.041	–0.034
	(0.024)	(0.030)	(0.033)	(0.045)	(0.031)	(0.040)
In a program area (δ_2) good area or good cohort	–0.009	–0.009	–0.036**	–0.017	0.007	–0.006
	(0.010)	(0.014)	(0.015)	(0.024)	(0.014)	(0.021)
Test ($\delta_1 = \delta_2$)	0.205	0.05	0.844	0.017	0.18	0.541
Mean super-control	0.167	0.376	0.15	0.396	0.178	0.364
Observations	11,806		4,387		7,419	

Notes. The table reports IV regressions of employment outcome variables on a dummy for participation to the program and a dummy for being in a local area with positive assignment probability, interacted with bad/good area and bad/good cohort dummies. Participation into the program is instrumented by assignment to the program. Bad areas are those with an average unemployment rate during first semester of 2007 that is above the median of our areas. Bad cohorts are cohorts 8–11, that is, entering the experiment in April to July 2008. For the list of controls and the definition of the outcomes, see Table III. The dependent variables are measured when surveyed eight months after the random assignment. The sample is restricted to job seekers who did not report that they were employed at the time of randomization. Only p-values for the equality tests are reported. Standard errors in parentheses are robust to heteroskedasticity and clustered at the local area level. ***indicate a 1% significance (**5%, *10%).

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

We find that the reinforced counseling program did have a positive impact on the employment status of young job seekers eight months after assignment into the treatment group, compared to untreated job seekers. However, this effect came partly at the expense of other workers, especially in weak labor markets.

The externalities we estimate suggest that part of the program effects in the short run were due to an improvement in the search ability of some workers, which reduced the relative job search success of others. These results challenge conventional theories of equilibrium unemployment (Hall 1979; Pissarides 2000) but they are consistent with a search model that takes rationing into account (Landais, Michaillat, and Saez 2012; Michaillat 2012). This model has an additional prediction that is also verified in the data: externalities are stronger in weaker labor markets where competition for jobs is fiercer. This prediction allows us to distinguish it from a pure rat race model, where treatment places the worker at the front of the queue for a fixed supply of jobs.

Additional evidence suggests that the main effect of the program was to help those treated find a job slightly faster, at the expense of others who subsequently took longer to find employment. In particular, after 12 months (and up to 20 months), the program effects on employment had entirely disappeared. In other words, there is no stepping stone effect, in which a fixed-term job can lead to a permanent position.

This has important consequences for estimating the welfare implication and cost benefit of this program. Indeed, in our setting, a back-of-the-envelope calculation suggests that taking externalities into account radically changes the conclusion. Our IV point estimates imply that for 1,000 people who were effectively treated by the program, 36 have found long-term jobs within eight months because of it. However, for 1,000 treated workers, there were on average 2,300 non-treated in the same regions, and the externalities estimates imply that out of these 2,300, 48 were displaced. In other words, there were in fact *more* jobs lost than found. Although this negative point estimate should not be taken too seriously (0 is definitely part of the confidence interval), we cannot reject that the program had, on net, no positive effect. If we had ignored externalities and taken as point estimate the IV within treatment zone, we would have found that out of 1,000 treated workers, 57 found long-term jobs within eight months because of the program. The cost of the program was on average 1,160 euros per worker, 585 euros higher than the cost of the regular employment services for the same duration.[29] Ignoring externalities, we would have thus concluded, for example, that 100,000 euros invested in the program would lead 9.7 extra people to find a job within eight months. Because the effect disappears by 12 months, this already appears to be quite expensive, at about 10,000 euros for a job found on average four months earlier. But at least it is not counterproductive. With externalities, investing 100,000 euros leads to no improvement at all.[30]

These results suggest that the current enthusiasm among policy makers in Europe for active labor market programs should be tempered because most available evidence in their favor does not take equilibrium effects into account. Micro treatment effect estimates of these policies cannot be directly used to draw macroeconomic conclusions. More broadly, our results are suggestive of externalities which should be taken into account when evaluating any labor market policy.[31]

Centre de Recherche en Économie et Statistique (CREST)
Massachussetts Institute of Technology
Paris School of Economics
Centre de Recherche en Économie et Statistique (CREST)
Centre de Recherche en Économie et Statistique (CREST)

29. Detailed calculations on the cost-benefit analysis are available on request.

30. The program led to a net positive impact on the number of fixed-term contracts of six months or more, but for welfare, we really need to consider durable jobs.

31. For example, Landais, Michaillat, and Saez (2012) use the estimates in this article (Tables IX and X) to calibrate their model. Notice, however, that our estimates are obtained on the specific population of young unemployed workers.

SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at QJE online (qje.oxfordjournals.org).

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