DISCUSSION PAPER SERIES

IZA DP No. 1796

Counseling the Unemployed: Does It Lower Unemployment Duration and Recurrence?

Bruno Crépon
Muriel Dejemeppe
Marc Gurgand

October 2005

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

Counseling the Unemployed: Does It Lower Unemployment Duration and Recurrence?

Bruno Crépon
CREST-INSEE, CEPR and IZA Bonn

Muriel Dejemeppe
Catholic University of Louvain

Marc Gurgand
Paris-Jourdan Sciences Economiques, CREST-INSEE and Catholic University of Louvain

Discussion Paper No. 1796
October 2005

IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
Email: iza@iza.org

IZA Discussion Paper No. 1796
October 2005

ABSTRACT

Counseling the Unemployed: Does It Lower Unemployment Duration and Recurrence?*

This article evaluates the effects of intensive counseling schemes that are provided to about 20% of the unemployed since the 2001 French unemployment policy reform (PARE). Several of the schemes are dedicated at improving the quality of assignment of workers to jobs. As a result, it is necessary to assess their impact on unemployment recurrence as well as unemployment duration. Using duration models and a very rich data set, we can identify heterogenous and time-dependent causal effects of the schemes. We find significant favorable effects on both outcomes, but the impact on unemployment recurrence is stronger than on unemployment duration. In particular, the program shifts the incidence of recurrence, one year after employment, from 33 to 26%. This illustrates that labor market policies evaluations that consider unemployment duration alone can be misleading.

JEL Classification: J64, J68

Keywords: unemployment duration, employment duration, active labor market policy, counseling

Corresponding author:

Bruno Crépon
15, boulevard Gabriel Péri
92245 Malakoff cedex
France.
E-mail: crepon@ensae.fr

* This paper is part of a research program founded by ANPE (Agence nationale pour l'emploi). We are very gratefull to Julie Micheau, Fabien Couprie and Laurent Josso from ANPE for providing the data and for numerous discussions. We thank Pierre Cahuc, Bart Cockx, Fabien Postel-Vinay, Laurence Rioux and Muriel Roger for useful advice, as well as participants to seminars at London School of Economics, Oxford University, Hong Kong University, IRES, Crest, Paris-Jourdan Sciences Economiques, ANPE, Direction de la prévision, IZA Workshop on Labor Markets and Institutions, COST Workshop and SOLE-EALE Congress. Muriel Dejemeppe also acknowledges the financial support of the program supported by the Belgian government 'Pôles dAttraction interuniversitaires' P5/21.

1 Introduction

The difficulty to allocate the right workers to the right jobs is an important source of market frictions. Attention to this issue has led to renewed interest for the assignment of heterogenous workers to heterogenous jobs in the recent literature.[1] As better sorting individuals into jobs has economic value, active labor market policies should be used to assist the unemployed workers in order to improve their matches.

Many reforms involving such counseling and job-search assistance have been implemented in OECD countries during the 1980s and 1990s (Martin and Grubb, 2001). But they are usually combined with monitoring of the unemployed and sanctions, so that their assignment function has rarely been analysed per se. Compelling evidence that reemployment services can be efficient in reducing beneficiaries unemployment duration is available in the US, based on randomized experiments (Meyer, 1995), and in the United Kingdom (Dolton and O'Neill, 1996, 2002, Blundell et al., 2004). But, as Meyer puts it, "the experiments have not convincingly separated the effects of requirements and assistance". As a matter of fact, using a set of randomized trials that incorporated *only* the element of work search verification, Ashenfelter et al. (2005) argue that such treatments alone are not effective. This implies, the authors say, that the benefit of reemployment services derives mainly from job-search assistance. In the Netherlands, Gorter and Kalb (1996) find rather limited impact of a monitoring and counseling program, while van den Berg and van der Klaauw (2006) find none, because, they argue, monitoring may induce the unemployed to search through an inefficient channel. The counseling dimension of those policies is therefore potentially more important than monitoring, but evidence is still limited.

The exclusive purpose of several schemes implemented along a 2001 French unemployment policy reform is to influence search strategy so as to improve the adequacy between the worker's own capability and the kind of job she is searching. Others schemes simply aim at enhancing search effort, through individual assistance. The French reform, called *Plan d'Aide au Retour à l'Emploi* (PARE), introduced two main changes. On the one hand, the degressivity of unemployment benefits was suppressed. On the other hand, the public unemployment agency revised its support policy towards unemployed persons. This reform departs from most of the foreign experiences in that very intensive schemes are attributed to a rather modest share of the unemployed (less than 20%), at a high cost (600 million euros a year). More importantly, limited actual monitoring is taking place. Compulsory meetings are not used to control the search activities of the unemployed workers, but mostly serves to decide on their allocation to the job-search assistance programs. In this context, we hope to evaluate counseling actions per se.

If these policies can have the effect of improving the quality of matches, evaluating their incidence on unemployment duration alone is not sufficient. Indeed, better matches should result in more productive and longer lasting jobs. Generally, unemployment rates or employment levels in the long run involve both unemployment *and* employment du-

[1] Marimon and Zilibotti, 1999, Costrell and Loury, 2004, Teulings and Gautier, 2004, Shimer, 2005, Teulings, 2005, Gautier and Teulings, 2005.

rations. It could be tempting for the public employment service, or any provider, to send workers to short-term or low paid jobs that are quite available, but do not last. This would certainly increase transitions to work, but is not necessarily desirable. It is therefore important to evaluate unemployment *and* employment durations altogether. The major point of this paper is that evaluation of active labor market policies can be misleading when employment duration is neglected. This is either because reduced unemployment duration may come at the cost of increased recurrence or because poor impact on unemployment duration does not imply that the program has failed if employment duration improved.

Yet, most empirical studies do not attempt to evaluate whether reemployment policies increases job duration, although this question has been sometimes investigated for training and employment subsidies (see e.g. Bonnal *et al.*, 1997, Gritz, 1993). Blundell *et al.* (2004) present estimation results which suggest that the New Deal for young people in UK increases by 4.5% points the probability to find a job *lasting at least 13 weeks* (the outcome variable), but the duration of employment is not formally analysed. Dolton and O'Neill (2002) find no effect of the Restart experiment on employment duration, but the program has an important monitoring component which can induce people to accept lower quality jobs. Weber and Hofer (2004) evaluate Austrian schemes similar to ours, but do not consider unemployment recurrence.

In this study, we measure the effectiveness of the four main counseling schemes offered to French unemployed, in raising the transition rate from unemployment to work and lowering recurrence into unemployment. In some countries, controlled experiments are available. In the present context, neither such an experiment nor a quasi-experimental design is available, because the reform applies uniformly to all unemployed. In a comparable situation, Gerfin and Lechner (2002) or Sianesi (2004) have evaluated several training and subsidized jobs programs simultaneously, using matching methods with rich data bases. As such, they have to assume selection on observables. However, semi-parametric identification of causal parameters in the presence of selectivity on unobservables is possible, relying on the timing of events and the proportional hazard specification (Abbring and van den Berg, 2003). This strategy has been successfully implemented in a set of recent papers (Abbring et al., 2000, Lalive et al., 2002a, 2002b, van den Berg et al., 2000, van den Berg et al., 2004 - see also Bonnal et al., 1997, for an early model in that vein). In this paper, we exploit an exceptional administrative database, set up by the French unemployment agency, that contains data on about 400,000 individual unemployment spells and very detailed information on services actually received, since implementation of the reform until June 2004. The size of the data allows flexible estimation of the impact of the four main schemes of the program, including heterogenous and time dependent effects. Because sampling is on individuals, we can observe both unemployment duration and recurrence.

We find that scheme effects on recurrence are strong and systematic, which we interpret as evidence that they improve match quality. All schemes also increase unemployment-employment transitions, but with smaller incidence. Because only a limited fraction of the unemployed receive treatment, the program increases the proportion that has found a job after one year by less than one percentage point. But it decreases the incidence of recurrence one year after a job is found by more than 6 percentage points. Therefore, if unemployment duration alone was considered, treatment effects would be strongly underestimated.

Overall, this is evidence that counseling and job-search assistance are effective, although we do not provide a cost-benefit analysis for lack of data. This is an important complement to the evaluation of labor market policies, which confirms recent insights. It implies that countries that pursue these actions, often with increasing use of private suppliers, are heading in the right direction.

The plan of the paper is as follows. The next section presents the 2001 reform of the French unemployment policy. Section 3 illustrates the economic interpretation of the counseling schemes in the line of assignment models. Section 4 describes the data. Section 5 presents the empirical method, and estimation results are discussed in Sections 6 and 7. Section 8 concludes.

2 The 2001 unemployment policy reform in France

The reform introduced in France in July 2001 was originally influenced by foreign experiences but it ended up with quite distinctive features (Freyssinet, 2002). In particular, sanctions and the definition of acceptable jobs remained based on already existing legal requirements, which are far less compelling than several foreign experiences. In addition, the public unemployment agency (ANPE), that is distinct from the institution providing benefits (UNEDIC), remained in charge of monitoring and counseling for all types of unemployed. UNEDIC did not obtain new power with respect to sanctions.

The reform thus consisted mainly of two distinct elements: a more generous benefit system overall (for entitled unemployed) and significantly stronger individual counseling services offered to the unemployed (whether insured or not), labelled the PAP program (*Projet d'action personnalisé*). Regarding the latter, there were two main changes. First, it was not unusual that an unemployed person would never meet the public unemployment agency caseworkers. A meeting (typically 30 minutes long) is now compulsory for all newly registered unemployed and recurs at least every 6 months. This is a low frequency, but not so far from international practice (Martin and Grubb, 2001). Depending on the person's profile, the caseworker can schedule follow-up interviews between two compulsory meetings, and interviews can be requested at any moment by the unemployed workers themselves.

Second, counseling services that existed before the reform were significantly extended, at the cost of increased budget. Some are provided directly by ANPE, others are subcontracted. Before the reform, these schemes were open only to the long-term (more than a year) unemployed. Training and employment subsidies are also in the range of available measures, but they are not considered in this paper.

During the first compulsory meeting, the unemployed person and the caseworker come together to an agreement about the degree of assistance that the person should receive. This agreement is based on the person's evaluation of her degree of autonomy in job-search and the caseworker's evaluation of her capabilities of finding rapidly a job. The interview concludes with the signing of the PAP contract which determines the degree of assistance and the types of services the person *should* receive. In the first interview, most of the unemployed workers are regarded as self-sufficient in their job search. In this or subsequent interviews, the individuals who are considered to need assistance can be offered a scheme, depending on the availability of slots.

We are interested in 4 categories of schemes that group several variants of each type of service[2]. Several other schemes do exist but they are shorter and have much lower unit costs. These 4 schemes are the most significant aspects of the reform.

- The basic *Skill assessment* ("Evaluation") lasts typically 1 day. The provider helps the individual assess his professional skills, based on testing and simulated work environment.

- Another skill assessment, that we label *Project assessment* ("Bilan de compétences approfondi"), is aimed at individuals with a professional experience who have difficulties finding a job corresponding to their skills. It lasts 20 hours on average, over a maximum period of 42 days. A personal adviser helps the individual analyse her past experience, identify her skills and match them with a new employment project compatible with the state of the labor market.

- *Job-search support* ("Objectif emploi" and similar schemes) is aimed at individuals having a well-defined employment project, but experiencing difficulties in their job search, with the aim of finding rapidly a ("long-term") job. It lasts up to 3 months. In this intensive scheme, the individual is assigned a personal advisor who helps him define the course of actions, teaches on job-search methods (for instance, résumé writing), provides logistic support, proposes job offers or interviews, contacts directly employers and so on. Some actions can be group-based.

- Finally, *Project support* ("Objectif projet" and similar schemes) is aimed at individuals who wish or have to change profession, but need time and help to define a new employment project. It also lasts 3 months during which the unemployed person has frequent contacts with a personal adviser. The objective of this scheme is similar to Project assessment but there are important differences. Project support is intended for lower ability workers who have stronger difficulties with the labor market and need regular and lasting follow-up. In some cases a placement in the workplace, lasting up to several days or weeks, is scheduled.

Only 17% of the unemployment spells in our data are associated with participation to at least one of these four programs over the whole period 2001-2004. This relatively low figure results partly from the fact that some spells end very rapidly (nearly 20% of spells end in the first month of unemployment). A large majority of spells with treatment (80%) receive only one treatment. Among this group, job-search support is by far the

[2]These groups have been defined with the help of the ANPE statistics and research Department and they are homogenous in terms of objectives, targetting and length.

most frequent measure (44%). Figure 1 describes the (monthly) empirical hazards for transition into the schemes. The chances to enter each scheme are typically below 1% per month, with Job-search support being provided more often. This will prove very important for the identification strategy because, with such a low rate, it is unlikely that treatment can be anticipated by potential beneficiaries. Strong peaks are present and they are related to compulsory interviews on the first, 6th and 12th months, but the entry rate remains positive at all dates and does not decline strongly in the long-run.

Although it benefits a limited number of persons, the burden of this program is significant because of high unit costs. The figures in Table 1 relate to the marginal costs of the services. The compulsory meetings with a caseworker have small unit cost, but they are very numerous. Workshop is a light service that is frequently provided and is not evaluated in this paper (it is aimed at individuals who experience only selective problems with their job-search and lasts 1/2 day each).

Skill assessment has a small weight, whereas Project assessment costs almost 900 euros and takes a large share of the budget, although its access is limited. The unit cost of Job-search and Project support is between 300 and 700 euros and they are more often provided. In 2003, ANPE has spent about 440 million euros on these two support schemes, which is the major share of the budget.

Overall, the schemes that are evaluated in this paper thus amount to 600 million euros, approximately 0.04% of GDP, which is a considerable amount for a program that benefits less than a fifth of the unemployed. This is about 20% of the total cost of public employment services and administration.[3] It is limited however, compared to the cost of the whole active labor market policy, which amounts to 1.25% of GDP (OECD, 2004).

3 Economic interpretation of treatments

Three out of the four treatments (Skill assessment, Project assessment and Project support) aim at improving the adequacy between the worker's own capability and the kind of job she is searching. For instance, the official objectives of Project support include: "Know oneself better", "Know one's labor market environment better", "Arbitrage between one's own aspiration and capabilities and the state of the labor market". The assignment literature provides the relevant theoretical background to conceptualize such policies, as it is attentive to worker and job heterogeneity and to the possibility that some workers have a comparative advantage on some jobs (Sattinger, 1993). In such models, better sorting individuals into jobs has economic value and several recent papers have reconsidered unemployment benefits as a search subsidy that helps the worker get a suitable job (Marimon and Zilibotti, 1999, Teulings and Gautier, 2004). Much of the program considered here can be understood analogously. Some unemployed workers may have an imperfect notion of the content of jobs and the fit with their own capabilities: caseworkers help them find out this valuable information.

[3] According OECD, this category includes activities of job placement, counseling and vocational guidance, administrating unemployment benefits, and refeering job-seekers to availbale slots on labour market programs.

The Job-search support scheme simply improves the efficiency of search. This is traditionally understood as affecting job offer arrival rates, but this has an impact on the quality of accepted jobs (van den Berg and van der Klaauw, 2006, Fougère, Pradel and Roger, 2004).

In that section, we illustrate the important fact that evaluating such schemes based on unemployment duration alone can be very misleading. To fix ideas, consider a simple model where workers are characterized by specific capabilities s and job characteristics are measured by c. There is some distribution of both variables in the economy. The quality of a match between a type s worker and a type c job is measured by some function $m(s,c)$. Following Moen (1997), assume that the unemployed concentrate their search in a submarket. The value of search in submarket c for worker type s depends on the arrival rate of job offers $p(m(s,c),c)$, the (unique) wage $w(m(s,c),c)$ and the job destruction rate, $q(m(s,c),c)$. In partial equilibrium, all three functions are assumed exogenous. They depend on $m(s,c)$ because more adequate workers may be offered a job more often, at a higher wage and with lower risk of destruction. They are also direct functions of c because some jobs or some sectors may have different values of p, w and q for any match quality. This leaves open the possibility of arbitrage between high wage-low job offers submarkets, high job offers-high destruction rates submarkets, and so on. Finally, we note λ an efficiency search parameter that represents the Job-search support policy. With r the discount factor, b unemployment benefit, $V_U(s,c)$ and $V_E(s,c)$ the values of search and employment respectively, we have:

$$\begin{aligned} rV_U(s,c) &= b + \lambda p(m(s,c),c)\left[V_E(s,c) - V_U(s,c)\right] \\ rV_E(s,c) &= w(m(s,c),c) + q(m(s,c),c)\left[V_U(s,c) - V_E(s,c)\right] \end{aligned}$$

The value of search is then given by:

$$rV_U(s,c) = b + \alpha(m(s,c),c)\left[w(m(s,c),c) - b\right]$$

with:

$$\alpha(m(s,c),c) = \frac{\lambda p(m(s,c),c)}{r + q(m(s,c),c) + \lambda p(m(s,c),c)}$$

Type s workers chooses submarket c so as to maximize $V_U(s,c)$. This determines her transition probabilities and wage.

Now, assume that some workers have an imperfect view of their own fitness with jobs: their decisions will be based on some inadequate function $\widetilde{m}(s,c)$. We interpret the "assignment" policies as providing them with the right function $m(s,c)$. The important point here is that, without further structure, the effect of this policy is largely undetermined. Call $\widetilde{c}^*(s)$ the job type that maximizes $b + \alpha(\widetilde{m}(s,c),c)\left[w(\widetilde{m}(s,c),c) - b\right]$ (i.e. under the belief that matching value is $\widetilde{m}(s,c)$) and $c^*(s)$ the chosen job type under knowledge of the true function $m(s,c)$. By definition, $V_U(s,c^*(s)) \geq V_U(s,\widetilde{c}^*(s))$. This is the only general restriction and it implies that *at least one* of the three outcomes, p, w or q, improves. Which of them improve depends on the way the change in the matching function shifts the locus of compatible p, w and q. In particular, it may happen that, with better knowledge, the unemployed worker optimally shifts her search towards a sector with less offers (lower p) but more stable jobs (higher q). This is likely if improved matching is more important to q than to p. As a result, it is important to evaluate this kind of policy with respect to as many outcomes as possible. Analysis of unemployment-employment transitions alone, as done in much of the labor market policy evaluation literature, may lead to misleading results.

The same holds for Job-search support. If treatment shifts λ to $\lambda' > \lambda$ (and affected workers are assumed to be knowledgeable about $m(s,c)$), it is certainly the case that $V_U(s, c^*(s|\lambda')) \geq V_U(s, c^*(s|\lambda))$. Still, this is again compatible with many changes of p, w and q, not just an improvement of unemployment exit rates. For that policy, such indeterminacy is not specific to this model and it also comes out in models used by van den Berg and van der Klaauw (2006) and Fougère, Pradel and Roger (2004).

Unfortunately, we do not observe wages in the data and we can only assess the effect of treatments on transitions between employment and unemployment. Within this model, the treatment effect parameters that we estimate empirically for transition from unemployment to employment reflect the ratio:

$$p(m(s, c^*(s)), c^*(s))/p(m(s, \widetilde{c}^*(s)), \widetilde{c}^*(s))$$

for "assignment" treatments and:

$$\lambda' p(m(s, c^*(s|\lambda')), c^*(s|\lambda'))/\lambda p(m(s, c^*(s|\lambda)), c^*(s|\lambda))$$

for the "job-search" treatment. For transition back from employment to unemployment, our empirical parameters measure:

$$q(m(s, c^*(s)), c^*(s))/q(m(s, \widetilde{c}^*(s)), \widetilde{c}^*(s))$$

and:

$$q(m(s, c^*(s|\lambda')), c^*(s|\lambda'))/q(m(s, c^*(s|\lambda)), c^*(s|\lambda))$$

respectively. We either compute averages over the distribution of s or make the effects vary with some observed individual characteristics that capture some of the variation in s.

One must be careful, however, not to interpret transitions for the untreated as the transition rate that would be observed if the policy was not implemented at all. By altering market frictions and the quality of the matches, the policy does modify the general equilibrium of the labor market, in particular wages and job-creation, and all outcomes are potentially different with and without the policy. Following most of the microeconometric evaluation literature, we do not attempt to build the corresponding general equilibrium counterfactuals. Yet, notice that even in the short-run, that is before market tension adjusts in equilibrium, the policy does not necessarily improve treated outcome *at the cost of* the untreated (the so-called "displacement" effect), because, for a given level of demand, better assignment can improve everyone's outcome at a time.

4 Data and descriptive analysis

The empirical analysis is based on longitudinal data extracted from ANPE records. We use a 1/12 nationally representative sample of all unemployed persons[4] and we sample all inflow spells between July 2001 and September 2003 but retain only the first spell observed on this period per individual, so as to avoid correlation of unobservables over consecutive spells. Data end in June 2004 and unemployment spells are arbitrarily truncated at 900 days because information becomes very poor after that duration. The data contain a large number of individual characteristics and unemployment history can be traced because individual data is available back to 1993. As emphasized by Heckman et al. (1997), controlling for individual labor market history is of central importance. We retain the following characteristics: gender, nationality, children, marital status, educational level, age, region of residence, reason of entry into unemployment, unemployment history (cumulative unemployment duration since July 1993 and since July 1999), unemployment recurrence (number of spells since July 1999), welfare transfer (RMI) and type of unemployment benefit eligibility.[5]

Entry into and exit from unemployment are recorded on a daily basis, so that we model duration in continuous time. In this data, unemployment differs from the ILO conventional notion, in the sense that people are recorded as job seekers as long as they report so to ANPE on a monthly filled form, even if they have held occasional or short-term jobs, which they have to declare. Some unemployed are classified as "not immediately available" because they suffer from health problems or cannot immediately drop their current occupation to take a job: the corresponding spells are not kept in the sample, as well as that of the handicapped. We also truncate spells when the unemployed reaches 55.

Transitions may occur towards other destinations than employment but they will be treated as censoring, which implies that they depend upon a disjoint subset of parameters. Although undesirable in some instances, this hypothesis maintains tractable estimation. "Other destinations" include training, illness, inactivity and, most importantly, subsidized public employment. In addition, some unemployed (about 20%) do not send their monthly form at some point so that they are known to exit but the destination is unobserved. Therefore, estimation must be limited to individuals with known exit.[6]

ANPE also provided data on the services that benefitted each unemployed worker, with a date for the effective beginning of the scheme. This has been matched with the data on unemployment spells. As reported in Table 2, the sample that is used in

[4] The sample consists of all individuals born on March of an even year or October of an odd year. This sample, named "Fichier historique statistique" is updated routinely by ANPE.

[5] In France, the period of entitlement to unemployment benefit is conditional on the length of the last employment spell and on age. The type of UB scheme refers to the length of the entitlement period. The data does not include any information about the amount of unemployment benefit paid to the individual.

[6] This assumes exogeneity of monthly declaration with respect to realizations of transitions. As the model is entirely conditional on observed and unobserved constant heterogeneity (see next section), this is a mild hypothesis.

estimation contains 390, 945 spells, among which 56, 784 receive some treatment. Less than half the transitions is toward employment: as a result, the sample that provides information on recurrence is much smaller and the number of treatments is more limited (15, 419). We will see below that this puts a limit to specification flexibility in this part of the analysis.

The transitions into the schemes have already been described in Section 2. Table 3 indicates that assignment is certainly not random. Column 1 gives some statistics on the characteristics of individuals who receive no treatment while columns 2 to 5 contain the same information for individuals who have received treatment. Women receive a treatment more often, especially of the project type, probably because their attachment to the labor market is weaker. Education has varying effects: those who benefit Projet assessment are more educated, whereas Skill assessment and Project support are more targeted to intermediary levels. Generally, schemes are less often provided to younger individuals. Being on Welfare has some positive effect as well as receiving unemployment benefit. Finally, having experienced other unemployment spells (since July 1999) increases the likelihood to receive a treatment. This general picture is robust to inclusion of these and additional variables into entirely specified duration models (see Table 5).

When a transition from unemployment to employment takes place, we define an "employment duration" as the time until the individual is back to reported unemployment. Because sampling is based on individuals and not spells, we are certain to observe the individual again in that case. Strictly speaking, the person may not have been in employment all the time, so it is proper to consider that we measure more exactly *recurrence*. With respect to the explicit objectives of ANPE schemes, this is an important dimension.

Figure 2 displays the empirical hazard rates of the transitions to employment and unemployment. As usual, the unemployment-employment transition exhibits a decreasing pattern with a small increase at one year that may be due to specific employment policies, including those considered here. The same pattern is found for the employment-unemployment transition, with peaks at 3, 6 and 12 months that may be related to standard contract duration.

5 Measuring the causal effect of counseling in a duration model framework

Access to treatments is likely non-random and is based on the caseworker decision and the unemployed agreement. Both depend on observed and unobserved (to the econometrician) characteristics. In a duration framework, it is possible to identify separately the causal effect of treatment on subsequent duration and the distribution of unobserved characteristics, although both contribute to observed correlations between durations. Abbring and van den Berg (2003) provide identification conditions for the mixed proportional hazards model, based on earlier literature (Elbers and Ridder, 1982, Heckman and Honoré, 1989, Honoré, 1993). Identification is non-parametric, in the sense that no functional form must be assumed for the baseline hazards or the multivariate distribution of unobserved heterogeneity. Moreover, it does not require exclusion restrictions. Abbring and van den Berg show that the elapsed duration until treatment contains useful information to disentangle the causal effect from the effect induced by selection on unobservables. The competing hazards model until entry in a treatment or exit to employment - whichever occurs first - identifies the joint distribution of unobservables. The remaining duration identifies the causal effect of the treatment. The exact timing of events is important because the causal effect is revealed by the change in the unemployment-employment transition hazard rate that occurs once treatment is received (if treatment is effective). This can be distinguished from unobserved heterogeneity because the latter is assumed constant over a spell. In contrast, if unobserved shocks occurred along the spell and their timing was correlated with that of treatment, this identification would fail. Arguably, even if this possibility cannot be excluded, controlling for population heterogeneity still goes a long way towards reducing potential bias.

Identification requires that the durations until treatment vary sufficiently. It implies that we should observe individuals at many dates of entry into treatment. As shown in Figure 1, this condition is fulfilled in our data. Even if there are peaks around compulsory meetings (at 0, 6, 12 months), entry rates into the various treatments remain positive at all times, because individuals or caseworkers can request interviews more often than the legal requirement. It is also required that unemployed individuals do not anticipate the exact date at which they will enter into a particular program. Otherwise, the program could have some effect *before* actual participation.[7] Such an anticipation is very unlikely within the PAP scheme, because the average monthly probability to enter a scheme is almost always well below 1% (see Figure 1), due to rationing. Even, if this is higher for some subpopulations, it is unlikely that it can induce significant bias, because the decision to send an unemployed worker to a program depends greatly on the agent in charge and on the number of slots available. Besides, the time between the prescription of a program and its effective start is very short in principle, preventing anticipation behaviour.

In this section, we present the statistical model used to identify the causal effect of treatment on our data. We first present the benchmark model based on Abbring and van den Berg (2003), then extend it to account for unemployment recurrence.

5.1 Benchmark model

The empirical model distinguishes the four treatment schemes presented above, indexed by $P \in \{1, 2, 3, 4\}$. Employment is an absorbing state for the moment. Individuals enter unemployment and may exit to one of the treatments or to employment, whichever occurs first. Because we restrict the sample to spells with at most one treatment, people in one of the treatments may then only exit to employment (before censoring). We model the assignment to treatment as a competing risk model. The causal effect of treatment is defined as a shift in the hazard of the transition toward employment, once treatment has started. This effect may depend on observed characteristics of individuals and may

[7] A neat example of anticipation is provided by Black et al. (2003) for the *Worker Profiling and Reemployment Services* system in Kentuky.

vary with elapsed duration since entry into treatment.

Call t_U total unemployment duration and t_P the duration until treatment (for individuals without treatment, $t_U = t_P$); x is a set of observed variables and $v = (v_U, v_1, v_2, v_3, v_4)$ is the vector of unobserved characteristics that govern transition from unemployment to employment and transitions to each of the treatments. The conditional hazard rates for transition to each treatment k as function of time since unemployed are:

$$h_k(t|x, v_k) = \theta_k(t)\psi_k(x)v_k, \quad k \in \{1, 2, 3, 4\}$$

where the functions $\theta_k(.)$ and $\psi_k(.)$ represent respectively the baseline hazard and the effect of observed characteristics on the conditional hazard. The joint distribution of the duration to treatment and the received treatment is:

$$f(t, P|x, v) = \left[\prod_{k=1}^{4} h_k(t|x, v_k)^{1(P=k)}\right] \prod_{k=1}^{4} S_k(t|x, v_k)$$

where $S_k(t|x, v_k) = \exp\left(-\int_0^t h_k(s|x, v_k)ds\right)$ is a survival function; the probability that no treatment has been received up to a duration t is the product of the survival functions, as they are (conditionally) independent.

The hazard rate of transitions from unemployment to employment, conditional on the set of observed and unobserved characteristics, received treatment P and duration t_P until treatment is:

$$h_U(t|t_P, P, x, v_U) = \theta_U(t)\psi_U(x)v_U \prod_{k=1}^{4} \left[\delta_k(t - t_P)\varphi_k(x)\right]^{1(P=k)}$$

The term within square brackets captures the treatment causal effect. It may shift the hazard rate differently according to individual characteristics ($\varphi_k(.)$) and time since treatment ($\delta_k(.)$). The simplest case is when $\delta_k(t - t_P)\varphi_k(x) = \exp(b_k)$, which we label the constant effect model. The corresponding survival function is $S_U(t|t_P, P, x, v_U) = \exp\left(-\int_0^t h_U(s|t_P, P, x, v_U)ds\right)$.

Because in general unobserved heterogeneity (v_1, v_2, v_3, v_4) may be correlated with v_U, we have to model jointly the durations t_U and t_P as well as P. Let denote $c(U) = 1$ when the unemployment spell is not censored and $c(U) = 0$ when it is. The full density of endogenous observations $L(t_U, t_P, P|x, v)$ can be computed from the conditional and marginal densities $f(t_U|t_P, P, x, v)$ and $f(t_P, P|x, v)$, enabling us to write the contributions to the likelihood, accounting for censored durations, as:

$$\begin{aligned} L(t_U, t_P, P|x, v) &= \left[h_U(t_U|t_P, P, x, v_U) \prod_{k=1}^{4} h_k(t_P|x, v_k)^{1(P=k)}\right]^{c(U)} \times \\ & S_U(t_U|t_P, P, x, v_U) \prod_{k=1}^{4} S_k(t_P|x, v_k) \end{aligned}$$

To compute the joint distribution of endogenous variables conditional on the observables only, we have to integrate out the unobserved terms. The likelihood is therefore:

$$L(t_U, t_P, P|x) = \int L(t_U, t_P, P|x, v)\, dG(v)$$

where $G(v)$ is the mixture distribution. Identification of $\theta_k(.)$, $\psi_k(.)$, $\delta_k(.)$ $\varphi_k(.)$, for $k = 1 \ldots 4$, $\theta_U(.)$, $\psi_U(.)$, and $G(.)$ follows from Abbring and van den Berg (2003).

5.2 Introducing employment duration

In our data, individuals enter, exit and sometimes reenter unemployment. We consider as an "employment" spell, a spell that begins with an exit from unemployment to employment. The duration of the spell is known when the individual reenters unemployment, otherwise the spell is censored. Because it is unsure that a job has been held during the whole period, recall that the hazard of the transition from "employment" to unemployment is, strictly speaking, a measure of recurrence.

The hazard rate of employment duration is:

$$h_E(t|P, x, v_E) = \theta_E(t)\psi_E(x)v_E \prod_{k=1}^{4} [\gamma_k(x)]^{1(P=k)}$$

The causal effect of treatment k on employment duration is $\gamma_k(x)$, and may depend on covariates x. Other notations are obvious. For individuals that exit from unemployment to employment, the likelihood involves an additional term which is the likelihood of the employment spell:

$$L_E(t_E, |P, x, v_E) = h_E(t|P, x, v_E)^{c(E)} \exp\left(-\int_0^t h_E(s|P, x, v_E)ds\right)$$

where $c(E) = 1$ when the employment spell is not censored and $c(E) = 0$ otherwise. The total conditional likelihood is now:

$$L(t_U, t_P, t_E, P|x, v) = L_E(t_E, |P, x, v_E)^{c(U)} L(t_U, t_P, P|x, v)$$

It must also be integrated over the distribution of unobserved terms, enlarged to $(v_U, v_E, v_1, v_2, v_3, v_4)$. The Appendix provides a sketch of the demonstration that this distribution, as well as the additional terms $\theta_E(.)$, $\psi_E(.)$ and $\gamma_k(.)$, are also identified, using identification results from successive durations models.

5.3 Specification issues

A joint distribution of unobserved heterogeneity with a completely flexible covariance matrix would be very difficult to estimate in practice, as the number of parameters is very large. We choose to model the distribution as a two-factor loading model, assuming that there are two fundamental unobserved factors V_1 and V_2 that enter every duration. The specification of the unobserved terms is thus:

$$v_k = \exp(\alpha_k^1 V_1 + \alpha_k^2 V_2)$$

Let Γ a 6×2 matrix formed by the coefficients α_k^1, α_k^2. The log-unobserved terms are therefore $w = \log(v) = \Gamma V$, with $V = (V_1, V_2)$. The covariance matrix of w is:

$$Var(w) = \Gamma Var(V) \Gamma'$$

The two-factor loading specification imposes no constraint on the correlation matrix, but identification requires some normalization. Clearly for any 2×2 orthogonal matrix[8] Q : $Var(w) = \Gamma Var(V)\Gamma' = \Gamma Q'QVar(QV)Q'Q\Gamma'$. Thus, if Γ and $Var(V)$ are solutions, then $\Gamma Q'$ and $Var(QV)$ are also solutions. This problem can be avoided by assuming that the two underlying factors are uncorrelated and by imposing a restriction on Γ, namely $\alpha_k^2 = 0$ for some k. A frequent and natural choice is to model the unobserved factors as a discrete distribution with mass points, following Heckman and Singer (1984), and interpret it as an approximation to a non-parametric distribution. We assume that V_1 and V_2 are independent and are both distributed on the support $\{-1, 1\}$ with distinct probabilities. The information contained in $Var(w)$ is sufficient to identify the 11 parameters in Γ and the two probabilities. Therefore, no additional constraints are required to normalize the means.

All model parameters are in exponential form. In particular, we model the contribution of explanatory variables as:

$$\psi_k(x) = \exp(x\beta_k)$$

The explanatory variables we introduce are of two types. The first type includes variables like gender, age, education, region of residence, nationality, children, marital status. These can reasonably be considered exogenous. The second type of conditioning variables (note it x_p) is based on past labor market history: reason for entry into unemployment, unemployment recurrence, welfare transfer, unemployment benefit track. They are functions of passed values of endogenous variables, which may generate correlation with unobserved heterogeneity. However, this is not an issue here, as the treatment parameters are still consistently estimated if we specify unobserved heterogeneity conditional on observed covariates rather than the opposite. Assume that the unobserved terms are in fact $v_k = \exp(x_p\rho_{kp} + \alpha_k^1 V_1 + \alpha_k^2 V_2)$: it is clear that the parameters ρ cannot be disentangled from the parameters β, but this does not affect the consistency of the parameters δ and γ of main interest.[9]

Flexibility of the baseline hazard is limited by the practical difficulties in estimating it jointly with the unobserved heterogeneity distribution (Baker and Melino, 2000). We adopt a piecewise constant hazard for the duration dependence functions $\theta_k(t)$, of the form:

$$\theta_k(t) = \sum_{l=1}^{l=L} e^{\theta_{kl}} 1(t \in I_l)$$

For unemployment duration, we allow for seven intervals, the first six of them being of equal length of 90 days, i.e. covering the first one and a half year of unemployment: $I_1 = [1, 90]$, $I_2 = [91, 180]$, $I_3 = [181, 270]$, $I_4 = [270, 360]$, $I_5 = [361, 450]$, $I_6 = [451, 540]$,

[8] An orthogonal matrix satisfies $QQ' = I$.

[9] This is in the spirit of Wooldridge's (2002) treatment of initial condition in a panel setup. He proposes to model the distribution of unobserved heterogeneity conditional on the first observation and use it to derive the density of observations. In contrast to this approach, however, we do not aim to recover all structural parameters.

$I_7 = [541, 900[$. For all other durations, we set five intervals, the first four of them lasting 90 days $I_1 = [1, 90]$, $I_2 = [91, 180]$, $I_3 = [181, 270]$, $I_4 = [270, 360]$, $I_5 = [361, 900]$.

Because local maxima are likely, we run optimization a number of times with randomly chosen starting values. The tolerance for the gradient was set to 10^{-6} and we used Gauss optmum library with the BFGS algorithm in order to be able to deal with the very large number of observations and parameters . Analytical gradients were used to speed-up optimization and to avoid imprecision in the Hessian computation. Out of, say, fifteen sets of random starting values, most would converge to the same maximum, and a few would converge to another set of parameters showing a lower likelihood. Having also checked the Hessian closely, we are thus confident that the reported estimates are at a global maximum.

6 Estimation results

6.1 Constant effect model

Table 4 and 5 show the estimates of the constant effect model. Table 4 reports the treatment effects and unobserved heterogeneity parameters; it also reports parameters of a model without unobserved heterogeneity. Comparison between the two illustrates that assuming selectivity on the observables only, as with matching methods, would be misleading in some instances. Table 5 contains the estimated effects of duration dependance and individual characteristics on the transition rate from unemployment to work, from employment back to unemployment and to each of the counseling schemes.

True duration dependence (Table 5) is graphed in Figures 3 and 4. Figure 3 describes the estimated duration dependence of the hazards into the schemes. As in the empirical hazard rates (see Figure 1), we observe peaks related to compulsory interviews at 0, 6 and 12 months. Figure 4 displays the estimated duration dependence of the hazard rates of exits to employment and to unemployment. They are flatter than their empirical counterparts (Figure 2), as a result for accounting for unobserved heterogeneity. The unemployment-employment transition exhibits a non-monotonic true duration dependence, decreasing slightly over the first 9 months of unemployment then increasing after 12 month and decreasing again. The employment-unemployment transition exhibits a more pronounced U-shape pattern, first increasing over the first 9 months of unemployment and then deceasing. After 12 months of employment, the hazard rate has dropped by 40%. Peaks at 6 and 12 months may be related to standard contract duration.

Heterogeneity parameters are precisely estimated and the distribution of heterogeneity is balanced, with the four types defined by the combination of the values of $(V1, V2)$ representing 40%, 33%, 15% and 12% of the population. The higher loading factor for all processes, but unemployment duration, is for the same heterogeneity component, $V1$: this implies that a significant share of correlations between durations can be attributed to unobserved factors. The effects of covariates are also precisely estimated and they are in line with the descriptive statistics of Table 3. Notice that the cohort effect obviously captures the cycle: cohorts entering at the end of the period have longer unemployment and shorter employment duration.

We now turn to the effects of treatments on the transition rate from unemployment to work. When unobserved heterogeneity is *not* allowed for, Skill assessment and Job-search support have a positive and significant impact on the exit rate towards work (column1, Table 4). A negative but small impact is found for Project assessment, and Skill assessment has no impact. Introducing correlated unobserved heterogeneity changes some of these results (column 2, Table 4). In particular, the effect of the Job-search support program reinforces strongly: the transition rate of individuals attending this scheme increases by about 73% ($exp(0.55) - 1$) instead of 46% in the model with no unobserved heterogeneity. The causal effect of Skill assessment is more limited (27%). The negative effect of Project support is small (−12%). Generally, in this constant effect specification, Project-type schemes have non-significant or negative effects, something that we will be able to interpret below.

Regarding the effects of counseling on transitions from employment back to unemployment, we observe striking differences whether the model allows or not for selection on unobservables. With *no* unobserved heterogeneity, most effects are positive but small (6 to 14%). The picture is reversed when allowing for correlated unobserved heterogeneity, implying strong selectivity into treatment. All schemes have strong causal effects, as they decrease recurrence by 49 to 58%.[10] It is important to emphasize that, because subsidized public employment is not considered among the work exits (and is treated among "other destinations"), this result is *not* driven by the relatively long average length of the subsidized jobs.

The nature of selectivity into the treatments is detailed in Table 6. Because the estimated correlation between unobserved heterogeneity terms is sensitive to included covariates, only total heterogeneity correlation is relevant to economic interpretation. It is computed as $corr(\psi_i(x)v_i, \psi_j(x)v_j)$, $i, j \in \{U, E, 1, 2, 3, 4\}$ in the population and is displayed on the top panel. All treatments, but Project support, are provided more intensely to individuals who have ex-ante lower unemployment-employment transition rates. Those negative correlations are rather small, however, leading to limited selectivity effect. The higher unobserved contribution is for Job-search support and this is indeed where the causal effect is most affected by selectivity correction in Table 4. Along the same logic, but with much stronger correlations, treatments are provided to individuals who are more at risk of recurrence. This is driven by unobserved heterogeneity: correlation are all above 0.9 because, in the factor loading approximation to the distribution of v, only one factor happens to be significant for v_E.

The direction of selectivity provides insights on program implementation. It can be considered that ANPE caseworkers are effective in selecting individuals who need treatment in the sense that they are particularly at risk of unemployment and recurrence, and this is why favorable treatment effects are underestimated when unobserved heterogeneity is not taken into account. This is important, because the caseworkers could be tempted to adopt the opposite strategy, so as to provide the illusion that they are

[10] These effects are not altered when employment duration is allowed to depend on previous unemployment duration (t_U).

efficient (or the institution is). In the case of recurrence into unemployment, it seems that caseworkers select individuals who have basic characteristics - such as education and age - that make them able to succeed on the labor market (as witnessed by negative correlation of observed heterogeneity), but have specific personal or motivation problems or bad knowledge of the labor market (unobserved to us) that require some intensive help.

6.2 Heterogenous effects model

In Table 7, we allow treatment effects to vary with some selected individual characteristics, with date of entry into unemployment and with time elapsed since treatment.[11] Significant heterogeneity of the effects is found only for the unemployment-employment transition; obviously, there is not enough information on employment duration for this very data-demanding section. We thus only comment the upper panel of Table 7. The most striking feature is the retention effect: Project assessment and Project support have a negative effect (for the reference person) immediately after treatment has started, as in the constant effect model. But we now allow this effect to change after three months: it becomes positive, with a net effect of 25% and 19% respectively (at the reference individual, pertaining to the early 2002 cohort). This is perfectly coherent with the function of these schemes: they are meant to build up a professional project and a search strategy before intensive search actually takes place. Because they last up to 3 months, retention effect is apparent during this period. Then, search becomes more efficient for the treated. Interestingly, Job-search support does not display such a feature, which is in accordance with its purpose, as described in Section 2. Overall, the orders of magnitude of the treatment effects on transitions towards employment are comparable with those found in the UK by Dolton and O'Neill (1996) for the Restart program and by Blundell et al. (2004) for the New Deal (about $20-30\%$).

All schemes are particularly efficient for the treated with some unemployment experience, a category that indeed more often receives the schemes (see Table 5). Assessment schemes are more efficient for the young, probably because new entrants are less informed and need help to fit their search strategy with the labor demand. However, the young are less often provided these treatments (Table 5). In contrast, Job-search support seems useful to more experienced persons as well as to more educated ones. It is indeed provided to more educated, but not to the older. Perhaps surprisingly, the educated also benefit more from Project support, but they receive it less often. Overall, correlations between treatment propensity and treatment efficiency are negative.[12] We compute them as $corr(\psi_k(x), \varphi_k(x))$, $k \in \{1, 2, 3, 4\}$ where $\psi_k(x)$ is the covariate contribution to the hazard rate into treatment and $\varphi_k(x)$ is from Table 7 (excluding month of entry and the incremental effect, so as to concentrate on individual heterogeneity). Correlations are: -0.38 for Skill assessment, -0.42 for Project assessment, -0.15 for Search support and -0.23 for Project support. This implies that, *as far as observed heterogeneity is*

[11] The rest of the model is close to Tables 4 and 5 and is not reported.

[12] This is similar to Black et al. (2003).

concerned, the schemes are not allocated to those who benefit them the most. Arguably, unobserved determinants may be present and be more decisive. Furthermore, we noticed that the schemes are offered to those who need them more, in the sense that their unemployment risk is higher: these persons may not also show the highest treatment effects.

Finally, treatment effects vary very strongly according to the date of entry into unemployment: they are lower for those entered at the end of the period (September 2003) that for those entered at reform starting time (July 2001). For instance, the point-estimate effect of Job-search support can switch from 165% to 61% for the reference individual, and that of Project support, from 52% to 6%. This finding can be driven by several factors. The rise in the number of participants since the launch of the program could reduce its beneficial effect if displacement plays a significant role. This could also reflect a maturation effect (see Blundell *et al.*, 2004): the caseworkers are less involved in the project two years after the launch of the program than they initially were. Finally, the effect of counseling schemes can be sensitive to the state of the labour market and decrease in a recession.

All these results are compatible with the analysis of Section 3 and with the nature of the various schemes. The intensive Job-search support has strong effects on both unemployment and employment durations, implying that improved search efficiency has general impacts on search strategy. As could be expected, the "assignment" schemes have stronger effects on employment duration than on unemployment duration: favoring assignment-efficient search strategy acts primarily on the quality of jobs, but not necessarily at the cost of longer search. Finally, the lighter scheme, Skill assessment, has no effect on unemployment duration, but still has some effect (the smallest one) on employment duration, something that is again compatible with theory.

7 Simulations

The parameters presented above are difficult to interpret directly because the absolute effects on exit rates depend on the baseline value of the hazard function and, when heterogeneous effects are considered, on the distribution of covariates. We quantify the effects of the schemes by way of simulations.

We first compute a set of parameters that are similar to the "treatment on the treated" used in the evaluation literature. We first examine how exit rates towards employment shift after treatment has been received. Decompose the entire unemployment duration as $t_U = t_P + t_R$ where t_P is duration until a treatment occurs and t_R is residual duration once treatment has been received. We compute the cumulative distribution of t_R on the population that has received a given treatment. We then compute this same cumulative, but with treatment effect set to zero. Our parameter is the difference between the two. Formally, for treatment $k \in \{1 \dots 4\}$:

$$\begin{aligned} UE_k(t) &= \int_x P(t_R < t | P = k, \delta_k(t) = \widehat{\delta}_k(t), \varphi_k(x) = \widehat{\varphi}_k(x)) \\ -P(t_R &< t | P = k, \delta_k(t) = 0, \varphi_k(x) = 0) dK(x | P = k) \end{aligned}$$

where $K(x|p = k)$ is the empirical distribution of observed covariates in the treated population. Implicit in the condition $P = k$ is the fact that unemployment duration up to treatment (t_P) is not randomly distributed: the parameter is thus evaluated on a selected population both in terms of duration dependence and individual heterogeneity. We define analogously a treatment on the treated parameter for employment duration:

$$\begin{aligned} EU_k(t) &= \int_x P(t_E < t|P = k, \gamma_k(x) = \widehat{\gamma}_k(x)) \\ -P(t_E &< t|P = k, \gamma_k(x) = 0) dK(x|P = k) \end{aligned}$$

This measures the shift in exit rate from employment back to unemployment for the population that received a given treatment.

In order to account for the fact that only a limited share of the unemployed workers receive a treatment, we also measure the effect of the presence of the schemes on the distributions of t_U and t_E in the whole population. This measures the impact of treatments altogether with the intensity of assignment to the treatments. Because of that, the starting date is now entry into unemployment (and no longer entry into treatment). The statistics is simply:

$$\begin{aligned} UE(t) &= \sum_k P(t_k < t, P = k) \int_x P(t_U < t|P = k, \delta_k(t) = \widehat{\delta}_k(t), \varphi_k(x) = \widehat{\varphi}_k(x)) \\ -P(t_U &< t|P = k, \delta_k(t) = 0, \varphi_k(x) = 0) dK(x|P = k) \end{aligned}$$

because the differential for the untreated ($t_k \geq t$) is 0. Notice that this parameter increases with t both because the share of unemployed that have exited increase with elapsed time but also because the proportion of treated also increases. Accordingly:

$$\begin{aligned} EU(t) &= \sum_k P(P = k) \int_x P(t_E < t|P = k, \gamma_k(x) = \widehat{\gamma}_k(x)) \\ -P(t_E &< t|P = k, \gamma_k(x) = 0) dK(x|P = k) \end{aligned}$$

The simplest way to measure all these parameters is to simulate the model. We first draw a random term in the distribution of unobserved heterogeneity for each individual in the sample. This allows us to compute the quantities $\psi_U(x)v_U$, $\psi_E(x)v_E$, $\psi_k(x)v_k$, $k = 1, \ldots, 4$. We then draw four independent values for the durations upon each treatment, i.e. draws in the distribution of t_1 to t_4 conditional on x and v.[13] The duration to potential treatment is $t_P = \min(t_1, t_2, t_3, t_4)$ and $P = \arg\min(t_1, t_2, t_3, t_4)$. We then draw in the distribution of t_U conditional on x, v_U and t_P.[14] The duration dependence is now a function of duration to potential treatment. Once this duration is drawn, we can define the actual duration to treatment, which is censored if exit to employment happens

[13]To this aim, we must take the inverse of the survival function $S_k(x,t) = exp\left(-\psi_k(x)v_k \int_0^t \theta_k(s)\, ds\right)$. This is simply the solution of $\int_0^t \theta_k(s)\, ds = \ln(1-u_k)/\psi_k(x)v_k$, with u_k a draw in the uniform $[0,1]$ distribution.

[14]This is performed along the same lines : we draw a random number in the uniform distribution and solve $\int_0^t \theta_U(s, t_P)\, ds = \ln(1-u_U)/\psi_U(x)v_U$.

before treatment ($t_U < t_P$). Simulation of the employment duration is performed in a similar way. In the end, every observation is given a sequence of durations, which is not the observed one, but is compatible with estimated joint distributions. This is done again with the effects parameters set to zero. It is important to recall that these simulations provide a synthetic view of the estimated effects, but they are not simulations in the general equilibrium sense.

In order to account for the precision of the estimators, we make these simulations again several hundred times, each time with a different draw into the normal distribution of the entire vector of parameters, using its estimated variance-covariance. The dashed lines in Figures 5 and 6 provide the corresponding 5% and 95% confidence intervals.

Figures 5a to 5d present the parameters $UE_k(t)$ (labelled "exit to employment") and $EU_k(t)$ ("exit to unemployment") for each of the four schemes. Consider first exit to employment. The baseline exit cumulative is not presented in the figures: it is to the order of 30% after 18 months and it reaches about 40% after 900 days. Treatments thus have strong impacts: the share of the unemployed that has exited to a job at some point after treatment increases by up to 5 percentage points for assessment schemes, 10 percentage points for Job-search support and 3 percentage points for Project support, under the effect of treatment. As soon as 5 months after treatment, Job-search support increases by 4 points the proportion that has found a job. The retention effect of the two project schemes is also visible in the figures.

The baseline order of magnitude of recurrence is 30% after 6 months and 70% after 900 days (this rapidly increasing cumulative function is typical of the specific population that benefits more often from treatment). Its shape is strongly affected by the schemes: as compared to the rates that would be observed in the absence of any treatment effect, recurrence is 17 to 25 points lower after 900 days and as much as 7 to 15 percentage points lower after only 6 months.

Figure 6 presents parameters $UE(t)$ and $EU(t)$. They account both for the effects of all four treatments altogether and for treatment intensity. Therefore, they measure the overall impact of the money spent on the program. Because less than one fifth of the population receives a schemes at some point, effects are much smaller. They remain significant, however. Due to retention effects and to the fact that schemes are provided progressively, the impact on exit to employment increases very slowly. After a year the gain is only half a percentage point and it reaches one point only after two years. The impact on exit to unemployment increases faster: the program reduces overall recurrence by 7 points (from 33% to 26%) after only one year.

8 Conclusion

This paper evaluates the causal effects of job search assistance schemes that became central in the public unemployment services since the July 2001 reform in France. Although this is only one dimension of this reform, it is a major innovation in the national context, with substantial budgetary effort, and one that lacks systematic evaluation in the literature. Moreover, several schemes aim at favoring search strategies that enhance the assignment of workers to jobs. This type of intervention may decrease unemployment duration but would more surely lower unemployment recurrence. The latter aspect is not routinely considered in the literature, although it has major implications for employment levels in the long run.

The available database makes estimation of those effects possible using identification results that rely only on duration information. Because the data is large, we can exploit all the flexibility that is available within this class of models, making the effects of the treatment depend on elapsed time and observed individual characteristics, even for a large number of potential treatments.

Generally, schemes are provided more often to those that need them the most, in the sense that their risk of long-term unemployment or recurrence is high. The schemes considered are found to have some impact on unemployment and employment duration. However, the magnitude of the impact is much larger on recurrence than on unemployment duration, especially for "assignment" schemes. Among the four counseling schemes analysed, the Job-search support program, which receives the largest financial effort from the public employment services, has the strongest effects overall. There is a retention effect on the "assignment" schemes that is consistent with their design. Heterogeneity of the effects is present in some instances and the efficiency of the schemes decreases with calendar time, to which the economic cycle probably contributes.

Treatment on the treated effects are large, but the overall effect is more limited given that only a small fraction of the unemployed receive treatment. This observation calls for a systematic cost-benefit analysis that cannot be implemented with the current data, because information on wages and the amount of unemployment benefits is lacking. Also, possible general equilibrium effect and displacement effects are not assessed. However, this analysis contributes to the evidence that counseling schemes are central to active labor market policies.

References

Abbring, J. H. and G.J. van den Berg (2002), “Dynamically assigned treatments: duration models, binary treatment models, and panel data models”, Working Paper 2002:20, IFAU, Institute for Labour Market Policy, forthcoming in *Empirical Economics*.

Abbring, J. H. and G.J. van de Berg (2003), “The non-parametric identification of treatment effects in duration models”, *Econometrica*, 71, 1491-1517.

Abbring, J. H., G.J. van den Berg and J. van Ours (2000), “The Effect of unemployment insurance sanctions on the transition rate from unemployment to employment”, mimeo.

Acemoglu, D. (2001), "Good jobs versus bad jobs", *Journal of Labor Economics*, 19, 1-21.

Acemoglu, D. and R. Shimer (1999), "Efficient unemployment insurance", *Journal of Political Economy*, 107, 893-928.

Ashenfelter O., Ashmore D. and Deschêne O. (2005), "Do unemployment insurance claimants actively seek work ? Evidence from randomized trials in four U.S. States", Journal of Econometrics, 125.

Baker, M. and A. Melino (2000), “Duration dependence and nonparametric heterogeneity: a Monte-Carlo study”, *Journal of Econometrics*, 96, 357-393.

Black D., J. Smith, M. Berger and B. Noel (2003), ”Is the threat of reemployment services more effective than the services themselves ? Evidence from random assignment in the UI system”, *American Economic Review*, 93, 1313-1327.

Blundell, R., M. Costa Dias, C. Meghir, and J. Van Reenen (2004), “Evaluating the employment impact of a mandatory job search program”, *Journal of the European Economic Association*, 2, 569-606.

Bonnal, L., D. Fougère and A. Sérandon (1997), “Evaluating the impact of French employment policies on individual labour market histories”, *Review of Economic Studies*, 6, 683-713.

Costrell R. and G. Loury, ”Distribution of ability and earnings in a hierarchical job assignment model”, *Journal of Political Economy*, 112, 1322-1363.

Dolton, P. and D. O’Neill (1996), “Unemployment duration and the restart effect: some experimental evidence”, *Economic Journal*, 106, 387-400.

Dolton, P. and D. O’Neill (2002), “The long-run effects of unemployment monitoring and work-search programs: experimental evidence from the United Kingdom”, *Journal of Labor Economics*, 20, 381-403.

Dormont, B., D. Fougère and A. Prieto (2001), “L’effet de l’allocation unique dégressive sur la reprise d’emploi”, *Economie et Statistique*, 343, 3-28.

Elbers, C. and G. Ridder (1982), "True and spurious duration dependence: the identifiability of the proportional hazard model", *Review of Economic Studies*, 64, 403-409.

Fougère, D., J. Pradel and M. Roger (2004), "The influence of the Public Employment Service on the search effort and on the probability of leaving unemployment", mimeo.

Freyssinet, J. (2002), “La réforme de l’indemnisation du chômage en France”, Document de travail 02.01, IRES.

Gautier P. and C. Teulings (2005), ”How large are search frictions ?”, mimeo.

Gerfin, M. and M. Lechner (2002), ”Microeconometric Evaluation of the Active Labour Market Policy in Switzerland”, *Economic Journal*, 112, 854-893.

Gritz, M. (1993), “The impact of training on the frequency and duration of employment”, *Journal of Econometrics*, 57, 21-51.

Heckman, J., and B. Honoré (1989), "The identifiability of the competing risk model", *Biometrika*, 76, 325-330.

Heckman, J., H. Ichimura and P. Todd (1997), "Matching as an econometric evaluation estimator: evidence from evaluation of a job training program", *Review of Economic Studies*, 64, 605-654.

Heckman, J. and B. Singer (1984), “A Method for minimizing the impact of distributional assumptions in econometric models for duration data”, *Econometrica*, 582, 271-320.

Honoré, B. (1993), "Identification results for duration models with multiple spells", *Review of Economic Studies*, 60, 241-246.

Johnson, T. and D. Klepinger (1994), "Experimental evidence on unemployment insurance work-search policies", *Journal of Human Resources*, 29, 695-717.

Klepinger D., T. Johnson and J. Joesch (2002), "Effects of unemployment insurance work-search requirements: the Maryland experiment", *Industrial and Labor Relations Review*, 56, 3-22.

Lalive, R., van Ours, J.C. and J. Zweimüller (2002a), “The effect of benefit sanctions on the duration of unemployment”, Discussion Paper 469, IZA, Bonn.

Lalive, R., van Ours, J.C. and J. Zweimüller (2002b), ”The impact of active labor market programs on the duration of unemployment”, Institute for Empirical Research in Economics, University of Zurich, working paper n° 41.

Marimon, R and F. Zilibotti (1999), “Unemployment vs. mismatch of talents: reconsidering unemployment benefits”, *Economic Journal*, 109, 266-291.

Martin, J. and D. Grubb (2001), “What works and for whom: a review of OECD countries’ experiences with active labour market policies”, IFAU Working paper 2001: 14.

Meyer, B. (1995), “Lessons from the U.S. unemployment insurance experiments”, *Journal of Economic Literature*, 33, 91-131.

Mortensen, D. and C. Pissarides (1994), “Job creation and job destruction in the theory of unemployment”, *Review of Economic Studies*, 61, 397-415.

OECD (2004), *OECD Employment outlook*, OECD, Paris.

Pissarides, C. (2000), *Equilibrium unemployment theory*, MIT Press.

Sattinger M. (1993), “Assignment models of the distribution of earnings”, *Journal of Economic Literature*, 31, 831-880.

Sianesi, B. (2004), “An Evaluation of the Swedish System of Active Labor Market Programs in the 1990s”, *Review of Economics and Statistics*, 86, 133-155.

Shimer R. (2005), “The Assignment of workers to jobs in an economy with coordination frictions”, *Journal of Political Economy*, forthcoming.

Teulings, C. (2005), “Comparative advantage, relative wages and the accumulation of human capital”, *Journal of Political Economy, 113, 425-461.*

Teulings, C. and P. Gautier (2004), ”The right man for the job”, *Review of Economic Studies*, 71, 553-580.

van den Berg, G., A. Holm and J. van Ours (2002), “Do stepping-stone jobs exist ? Early career paths in the medical profession”, *Journal of Population Economics*, 15, 647-665.

van den Berg, G. and B. van der Klaauw (2006), “Counseling and Monitoring of Unemployed Workers: Theory and Evidence from a Controlled Social Experiment”, *International Economic Review*, forthcoming.

van den Berg, G., B. van der Klaauw and J. van Ours (2004), “Punitive sanctions and the transition rate from welfare to work”, *Journal of Labor Economics,* 22, 211-241.

Weber A. and Hofer H. (2004), "Are job-search programs a promising tool ? A microeconometric evaluation for Austria", IZA Discussion paper 1075.

Wooldridge, J. (2002), “Simple solutions to the initial conditions problem in dynamic nonlinear panel data models with unobserved heterogeneity”, Cemmap working paper CWP18/02.

Appendix

Consider the trivariate model (t_U, t_P, t_E) for total unemployment duration, duration until (a unique) treatment and employment duration respectively. We observe:

$$\begin{cases} (t_U, t_P, t_E) & \text{if } t_U > t_P \\ (t_U, t_E) & \text{if } t_U \leq t_P \end{cases}$$

Following notations in text, hazard rates are:

$$\begin{aligned} &\theta_U(t_U)\psi_U(x)v_U\,[\delta]^{1(t_U>t_P)} \\ &\theta_P(t_P)\psi_P(x)v_P \\ &\theta_E(t_E)\psi_E(x)v_E\,[\gamma\,(x)]^{1(t_U>t_P)} \end{aligned}$$

We assume identifications conditions as required in Abbring and van den Berg (2003), so that all unemployment and treatment hazard parameters are identified. The densities of interest for the employment part of the model are:

$$\begin{aligned} f\,(t_U, t_P, t_E, t_U > t_P|x) &= \theta_U(t_U)\theta_P(t_P)\theta_E(t_E)\psi_U(x)\psi_P(x)\psi_E(x)\delta\gamma\,(x) \\ &\quad \times E_v[v_U v_P v_E \exp(-(Z_U(t_P) + \delta[Z_U(t_U) - Z_U(t_P)])\psi_U(x)v_U \\ &\quad - Z_P(t_P)\psi_P(x)v_P - Z_E(t_E)\psi_E(x)v_E\gamma\,(x))] \\ f\,(t_U, t_E, t_U \leq t_P|x) &= \theta_U(t_U)\theta_E(t_E)\psi_U(x)\psi_E(x) \\ &\quad \times E_v[v_U v_E \exp(-Z_U(t_U)\psi_U(x)v_U \\ &\quad - Z_P(t_U)\psi_P(x)v_P - Z_E(t_E)\psi_E(x)v_E)] \end{aligned}$$

where $E_v[.]$ denotes expectancy with respect to the joint distribution of unobserved heterogeneity (the corresponding integrals are supposed to exist) and $Z_k(.)$ are integrated baseline hazards. We normalize $\psi_E(x_0) = 1$ for some x_0 and $Z_E(t_E^*) = 1$ for some t_E^*. Identification proofs closely follow Honoré (1993).

Identification of $\psi_E(.)$:

Take the limit of $f\,(t_U, t_E, t_U \leq t_P|x)\,/ f\,(t_U, t_E, t_U \leq t_P|x_0)$ for $t_U \to 0$ and $t_E \to 0$:

$$\lim_{\substack{t_U \to 0 \\ t_E \to 0}} \frac{f\,(t_U, t_E, t_U \leq t_P|x)}{f\,(t_U, t_E, t_U \leq t_P|x_0)} = \frac{\psi_U(x)\psi_E(x)}{\psi_U(x_0)\psi_E(x_0)}$$

Therefore $\psi_E(x)/\psi_E(x_0)$ is identified, so is $\psi_E(x)$ up to a normalization.

Identification of $\gamma\,(.)$:

Consider the observable survival function:

$$\begin{aligned} S\,(t_U, t_P, t_E, t_U > t_P|x) &= E_v[\exp(-(Z_U(t_P) + \delta[Z_U(t_U) - Z_U(t_P)])\psi_U(x)v_U \\ &\quad - Z_P(t_P)\psi_P(x)v_P - Z_E(t_E)\psi_E(x)v_E\gamma\,(x))] \end{aligned}$$

The derivative with respect to t_E is:

$$\begin{aligned} \frac{\partial S\,(t_U, t_P, t_E, t_U > t_P|x)}{\partial t_E} &= -\theta_E(t_E)\psi_E(x)\gamma\,(x) \\ &\quad \times E_v[v_E \exp(-(Z_U(t_P) + \delta[Z_U(t_U) - Z_U(t_P)])\psi_U(x)v_U \\ &\quad - Z_P(t_P)\psi_P(x)v_P - Z_E(t_E)\psi_E(x)v_E\gamma\,(x))] \end{aligned}$$

Take the limit for $t_U \to 0$, $t_P \to 0$ and $t_E \to 0$:

$$\lim_{\substack{t_U \to 0 \\ t_E \to 0}} \frac{\partial S\left(t_U, t_E, t_U > t_P | x\right)}{\partial t_E} = -\theta_E(0)\psi_E(x)\gamma\left(x\right)$$

Accordingly for the survival on the untreated population:

$$\lim_{\substack{t_U \to 0 \\ t_E \to 0}} \frac{\partial S\left(t_U, t_P, t_E, t_U \leq t_P | x\right)}{\partial t_E} = -\theta_E(0)\psi_E(x)$$

This identifies $\gamma\left(x\right)$ because the rest of the model is already identified.

Identification of G:

Take observations with $t_U > t_P$. The survival function is:

$$\begin{aligned} S\left(t_U, t_P, t_E, t_U > t_P | x\right) &= E_v[\exp(-(Z_U(t_P) + \delta[Z_U(t_U) - Z_U(t_P)])\psi_U(x)v_U \\ &\quad -Z_P(t_P)\psi_P(x)v_P - Z_E(t_E)\psi_E(x)v_E\gamma\left(x\right))] \end{aligned}$$

This function has the form:

$$K(s_U, s_P, s_E) = E_v\left[\exp(-v_U s_U - v_P s_P - v_E s_E)\right]$$

As in Honoré (1993), identification of the distribution of unobserved heterogeneity is based on the function K, that is the Laplace transform of G. If K can be identified on an open set of R^3, then it can be extended on all R and the distribution of G is identified.

To show that K is identified on an open set, we can consider the function

$$\chi : (t_U, t_P, t_E, x) \to (Z_U(t_P) + \delta[Z_U(t_U) - Z_U(t_P)])\psi_U(x), Z_P(t_P)\psi_P(x), Z_E(t_E)\psi_E(x)\gamma\left(x\right)$$

Set $t_E = t_E^*$ so that $Z(t_E^*) = 1$. As Z_U, Z_P, ψ_U, ψ_P, ψ_E, δ and γ are identified, the function χ is identified. Moreover, as Z_U and Z_P are strictly increasing and provided there exists a variable with continuous distribution entering $\psi_E(x)$, then variation in t_U, t_P, and x will span at least an open set in R^3.

Identification of $Z_E(.)$:

$Z_E(.)$ is identified by letting t_E vary in the survival functions, with x, t_U and t_P fixed.

Table 1: Cost and volume of the PAP schemes in 2003

	Unit cost	Volume	Total cost (millions)	Share in total cost
Meetings	26.43 €	13 096 151	346.19 €	34%
Workshops	38.67 €	1 218 604	47.13 €	5%
Skill assessment	224.86 €	205 436	46.19 €	5%
Project assessment	881.99 €	150 926	133.12 €	13%
Search or Project support	541.69 €	810 032	438.79 €	43%
Total	65.33 €	15 481 149	1 011.41 €	100%

Source: ANPE.

Table 2: Spell sample statistics

Unemployment spells	
total number of spells	390 945
exit to employment	146 239
exit to other destination	161 299
censoring	83 407
no treatment	334 161
Skill assessment	8 442
Project assessment	8 564
Job-search support	24 440
Project support	15 338
Employment spells	
total number of spells	146 239
exit to unemployment	56 095
censoring	90 144
no treatment	130 820
Skill assessment	2 025
Project assessment	2 134
Job-search support	7 662
Project support	3 598

Source: FHS-ANPE, authors computation. First unemployment spell per individual, excluding unknown destination.

Table 3: Sample individual characteristics

	Spells with no treatment	Skill assessment	Project assessment	Job-search support	Project support
male	47%	43%	39%	42%	39%
female	53%	57%	61%	58%	61%
elementary school	21%	15%	8%	23%	17%
lower secondary	42%	48%	34%	42%	46%
upper secondary	17%	20%	23%	14%	19%
higher education	20%	17%	35%	22%	18%
age below 25	30%	16%	9%	22%	26%
age 25-30	20%	18%	20%	16%	20%
age 30-40	28%	36%	41%	28%	30%
age 40-50	17%	23%	23%	25%	19%
age 50-55	6%	7%	7%	10%	4%
welfare income	9%	10%	7%	14%	12%
unemployment benefit	66%	75%	79%	70%	66%
neither	25%	16%	14%	17%	23%
unemployment recurrence	47%	51%	60%	51%	56%
no recurrence	53%	49%	40%	49%	44%

Source: FHS-ANPE, authors computation. First spell per individual, excluding unknown destination. 390945 spells.

Table 4: Estimates of constant treatment effects and unobserved heterogeneity

	Without UH		With UH	
	coeff.	sd	coeff.	sd
Treatment effects: Unemployment - Employment				
Skill assessment	**0.210**	(0.022)	**0.241**	(0.074)
Project assessment	-0.004	(0.022)	-0.103	(0.067)
Job-search support	**0.375**	(0.012)	**0.547**	(0.031)
Project support	**-0.072**	(0.017)	**-0.131**	(0.053)
Treatment effects: Employment - Unemployment				
Skill assessment	-0.048	(0.040)	**-0.667**	(0.082)
Project assessment	**0.097**	(0.040)	**-0.879**	(0.067)
Job-search support	**0.065**	(0.021)	**-0.804**	(0.053)
Project support	**0.127**	(0.029)	**-0.688**	(0.059)
Factor loading parameters				
Unemployment - Employment				
$\alpha 1$			**-0.075**	(0.025)
$\alpha 2$			**1.025**	(0.013)
Employment - Unemployment				
$\alpha 1$			**0.701**	(0.033)
$\alpha 2$			-0.048	(0.034)
Skill assessment				
$\alpha 1$			**0.844**	(0.099)
$\alpha 2$			0.059	(0.073)
Project assessment				
$\alpha 1$			**1.927**	(0.132)
$\alpha 2$			**0.211**	(0.074)
Job-search support				
$\alpha 1$			**1.389**	(0.049)
$\alpha 2$			0.000	
Project support				
$\alpha 1$			**1.263**	(0.072)
$\alpha 2$			**0.137**	(0.056)
Probabilities				
prob(V1=-1)			**0.726**	(0.031)
prob(V1=1)			0.274	
prob(V2=-1)			**0.447**	(0.018)
prob(V2=1)			0.553	

Source: FHS-ANPE. First spell per individual, excluding unknown destination. 390945 spells. Other parameters are presented in Table 5. In bold, estimates significantly different from zero at 5%. $\alpha 1$ and $\alpha 2$ are the coefficients of the two loading factors in each exit destination. Probabilities are the probability distribution of the two factors.

Table 5: Estimated effect of duration and individual characteristics on the transition rates

	Unemp.-Emp.		Skill assessment		Project assessment		Job-search support		Project support		Emp.-Unemp.	
	coeff.	sd	coeff.	sd	coeff.	sd	coeff.	sd	coeff.	sd	coeff.	sd
Intercept	**-7.039**	(0.034)	**-9.780**	(0.097)	**-12.091**	(0.180)	**-8.280**	(0.080)	**-8.356**	(0.091)	**-6.887**	(0.048)
Duration dependence (< 3 months)												
3-6 months	**-0.019**	(0.008)	**-0.426**	(0.035)	**-0.806**	(0.037)	**-0.426**	(0.021)	**-0.684**	(0.026)	**0.295**	(0.013)
6-9 months	**-0.020**	(0.011)	-0.017	(0.041)	**-0.112**	(0.040)	**0.383**	(0.022)	**-0.130**	(0.031)	**0.280**	(0.018)
9-12 months	**-0.252**	(0.014)	**-0.328**	(0.056)	**-0.407**	(0.057)	**0.108**	(0.031)	**-0.396**	(0.043)	-0.043	(0.024)
12-15 months (col. 1) or > 12 months (col. 2-6)	**-0.201**	(0.017)	0.051	(0.069)	**0.171**	(0.066)	**0.518**	(0.037)	-0.036	(0.052)	**-0.445**	(0.027)
15-18 months	**-0.259**	(0.021)										
> 18 months	**-0.212**	(0.021)										
Personal characteristics												
male	**0.258**	(0.007)	**-0.128**	(0.025)	**-0.402**	(0.025)	**-0.209**	(0.016)	**-0.349**	(0.019)	-0.087	(0.010)
no children	0.007	(0.009)	**-0.138**	(0.029)	-0.017	(0.030)	-0.010	(0.019)	**-0.046**	(0.023)	**0.083**	(0.014)
not French	**-0.551**	(0.013)	**0.196**	(0.039)	**-0.467**	(0.054)	**0.110**	(0.026)	-0.006	(0.032)	**0.262**	(0.020)
married	**0.032**	(0.009)	**0.117**	(0.028)	-0.028	(0.029)	**-0.045**	(0.018)	**-0.132**	(0.022)	**-0.104**	(0.013)
Education (elementary school)												
higher education	**0.041**	(0.011)	**0.166**	(0.042)	**1.483**	(0.048)	**0.178**	(0.025)	-0.011	(0.031)	**-0.374**	(0.017)
upper secondary	**-0.098**	(0.011)	**0.576**	(0.040)	**1.347**	(0.049)	**-0.113**	(0.026)	**0.243**	(0.030)	**-0.085**	(0.016)
lower secondary	**-0.203**	(0.009)	**0.447**	(0.034)	**0.763**	(0.046)	-0.031	(0.020)	**0.248**	(0.025)	**0.033**	(0.014)
Age (< 25 years)												
25 to 30 years	**-0.306**	(0.011)	**0.217**	(0.041)	**0.840**	(0.048)	**-0.251**	(0.026)	-0.021	(0.029)	**-0.096**	(0.015)
30 to 40 years	**-0.359**	(0.011)	**0.372**	(0.040)	**1.234**	(0.047)	**-0.086**	(0.025)	-0.004	(0.029)	**0.013**	(0.016)
40 to 50 years	**-0.455**	(0.012)	**0.349**	(0.044)	**1.288**	(0.051)	**0.231**	(0.027)	-0.024	(0.032)	**0.079**	(0.018)
50 to 55 years	**-0.377**	(0.019)	**0.276**	(0.059)	**1.030**	(0.066)	**0.260**	(0.034)	**-0.297**	(0.051)	**0.062**	(0.031)
Region of residence (Paris)												
R1 (high unemployment rate)	**0.056**	(0.010)	**0.210**	(0.034)	**0.366**	(0.036)	**0.514**	(0.024)	**0.213**	(0.027)	**0.311**	(0.017)
R2 (medium unemployment rate)	**0.181**	(0.011)	0.028	(0.037)	**0.176**	(0.038)	**0.420**	(0.025)	**0.164**	(0.028)	**0.342**	(0.018)
R3 (low unemployment rate)	**0.334**	(0.010)	**0.114**	(0.034)	**0.237**	(0.035)	**0.358**	(0.024)	**0.205**	(0.027)	**0.372**	(0.016)
Reason of entry into unemployment (first entry)												
firing	**0.285**	(0.015)	0.044	(0.052)	**0.447**	(0.061)	**-0.328**	(0.033)	**-0.237**	(0.037)	**-0.224**	(0.023)
demission	**0.596**	(0.017)	-0.047	(0.061)	**0.302**	(0.068)	**-0.323**	(0.039)	**-0.341**	(0.044)	**-0.143**	(0.025)
end of contract	**0.725**	(0.013)	**-0.244**	(0.051)	-0.034	(0.060)	**-0.466**	(0.030)	**-0.487**	(0.035)	0.017	(0.020)
others	**0.145**	(0.014)	-0.018	(0.047)	**0.210**	(0.058)	**-0.286**	(0.028)	**-0.251**	(0.032)	**-0.072**	(0.021)
Unemployment history:												
log of cumulative duration (standardized mean value)												
since July 1993	**-0.032**	(0.004)	**-0.045**	(0.013)	**-0.065**	(0.014)	**-0.055**	(0.010)	**-0.070**	(0.011)	**0.035**	(0.006)
since July 1999	**-0.646**	(0.013)	**-0.382**	(0.048)	**-0.648**	(0.054)	**-0.164**	(0.032)	**-0.543**	(0.038)	**0.044**	(0.019)
Unemployment recurrence (1st spell since July 1999)												
2nd spell since July 1999	**0.922**	(0.024)	**0.491**	(0.087)	**0.771**	(0.097)	**0.066**	(0.059)	**0.624**	(0.068)	0.053	(0.034)
3th spell since July 1999	**1.068**	(0.028)	**0.596**	(0.099)	**0.861**	(0.111)	0.114	(0.066)	**0.665**	(0.078)	**0.159**	(0.039)
> 3th spells since July 1999	**1.264**	(0.029)	**0.641**	(0.106)	**0.872**	(0.121)	0.126	(0.070)	**0.676**	(0.084)	**0.376**	(0.041)
Cohort effect:												
log of calendar month of entry (standardized mean v	**-0.134**	(0.004)	**0.237**	(0.014)	**0.412**	(0.018)	**0.205**	(0.009)	**0.223**	(0.011)	**0.072**	(0.005)
Social transfers (no RMI)												
RMI	**-0.727**	(0.014)	0.068	(0.043)	-0.079	(0.050)	**0.387**	(0.026)	**0.215**	(0.031)	**0.135**	(0.021)
Unemployment benefits (no UB)												
UB - 122 days	**-0.686**	(0.016)	**-0.353**	(0.065)	**-0.389**	(0.074)	**-0.407**	(0.039)	**-0.536**	(0.049)	**0.270**	(0.020)
UB - 213 days	**-0.740**	(0.014)	**-0.136**	(0.050)	**-0.291**	(0.059)	**-0.224**	(0.032)	**-0.337**	(0.038)	**0.272**	(0.019)
UB - 456 days	**-0.723**	(0.013)	**-0.134**	(0.050)	**-0.069**	(0.056)	**-0.089**	(0.031)	**-0.292**	(0.038)	**0.165**	(0.018)
UB - 700 days	**-1.041**	(0.014)	**0.147**	(0.046)	**0.130**	(0.047)	**0.105**	(0.031)	**-0.100**	(0.036)	**0.179**	(0.024)
UB - 912 days	**-0.861**	(0.010)	-0.019	(0.038)	0.075	(0.040)	**-0.047**	(0.024)	**-0.222**	(0.029)	**0.036**	(0.014)
UB for > 50 years old	**-1.821**	(0.026)	**-0.205**	(0.076)	0.140	(0.080)	**0.096**	(0.043)	**-0.569**	(0.066)	0.088	(0.045)
# parameters					239							
# observations					390 945							

Source: FHS-ANPE. First spell per individual, excluding unknown destination. A16Other parameters are presented in Table 4. In brackets, reference category. In bold, estimates significantly different from zero at 5%.

Table 6 : Correlation between heterogeneity terms in hazard rates

	Total heterogeneity					
	S	Pa	J	Ps	U	E
Skill assessment (S)	1.000					
Project assessment (Pa)	0.791	1.000				
Job-search support (J)	0.827	0.670	1.000			
Project support (Ps)	0.857	0.667	0.897	1.000		
Unemployment-Employment (U)	-0.107	-0.047	-0.071	0.017	1.000	
Employment-Unemployment (E)	0.633	0.363	0.703	0.619	-0.117	1.000
	Observed heterogeneity					
	S	Pa	J	Ps	U	E
Skill assessment (S)	1.000					
Project assessment (Pa)	0.663	1.000				
Job-search support (J)	0.456	0.378	1.000			
Project support (Ps)	0.593	0.349	0.706	1.000		
Unemployment-Employment (U)	-0.329	-0.209	-0.118	0.064	1.000	
Employment-Unemployment (E)	-0.132	-0.368	-0.071	-0.170	-0.150	1.000
	Unobserved heterogeneity					
	S	Pa	J	Ps	U	E
Skill assessment (S)	1.000					
Project assessment (Pa)	0.988	1.000				
Job-search support (J)	0.996	0.972	1.000			
Project support (Ps)	0.996	0.998	0.986	1.000		
Unemployment-Employment (U)	-0.031	0.027	-0.092	0.007	1.000	-0.153
Employment-Unemployment (E)	0.986	0.951	0.997	0.969	-0.153	1.000

Source: FHS-ANPE. Based on estimates presented in Tables 4 and 5. The modelization of individual heterogeneity in hazard rates introduces two components, one observed and the other unobserved. The table gives the covariance matrix of these components accross exit destinations.

Table 7: Estimates of heterogeneous treatment effects

	Skill assessment		Project assessment		Job-search support		Project support	
	coef.	sd	coef.	sd	coef.	sd	coef.	sd
	Unemployment - Employment transition							
constant	0.138	(0.092)	**-0.256**	(0.087)	**0.571**	(0.046)	-0.016	(0.074)
log(calendar month of entry)	**-0.085**	(0.029)	**-0.106**	(0.034)	**-0.135**	(0.015)	**-0.097**	(0.022)
male	-0.074	(0.054)	0.025	(0.054)	0.055	(0.029)	-0.042	(0.042)
at most lower secondary degree	0.018	(0.056)	-0.032	(0.055)	**-0.295**	(0.031)	**-0.109**	(0.042)
<30 years old	**0.133**	(0.057)	**0.198**	(0.058)	**-0.125**	(0.031)	0.023	(0.042)
>1 unemployment spells since July 1999	**0.187**	(0.054)	**0.139**	(0.055)	**0.163**	(0.030)	**0.133**	(0.042)
incremental effect: > 3 months after treatement start	0.015	(0.052)	**0.476**	(0.051)	**0.059**	(0.024)	**0.189**	(0.038)
	Employment-Unemployment transition							
constant	**-0.454**	(0.115)	**-0.926**	(0.096)	**-0.804**	(0.068)	**-0.681**	(0.084)
log(calendar month of entry)	0.052	(0.046)	0.100	(0.053)	-0.038	(0.021)	-0.023	(0.030)
male	-0.127	(0.091)	0.041	(0.082)	-0.007	(0.043)	-0.058	(0.061)
at most lower secondary degree	0.012	(0.093)	0.096	(0.082)	0.067	(0.046)	0.082	(0.063)
<30 years old	0.067	(0.090)	0.114	(0.085)	0.019	(0.044)	0.008	(0.061)
>1 unemployment spells since July 1999	**-0.293**	(0.088)	-0.060	(0.082)	-0.033	(0.044)	-0.098	(0.060)

Source: FHS-ANPE. First spell per individual, excluding unknown destination. 390945 spells. Other parameters not presented. In bold, estimates significantly different from zero at 5%.

Figure 1: Schemes empirical duration dependence - Kaplan-Meier estimates

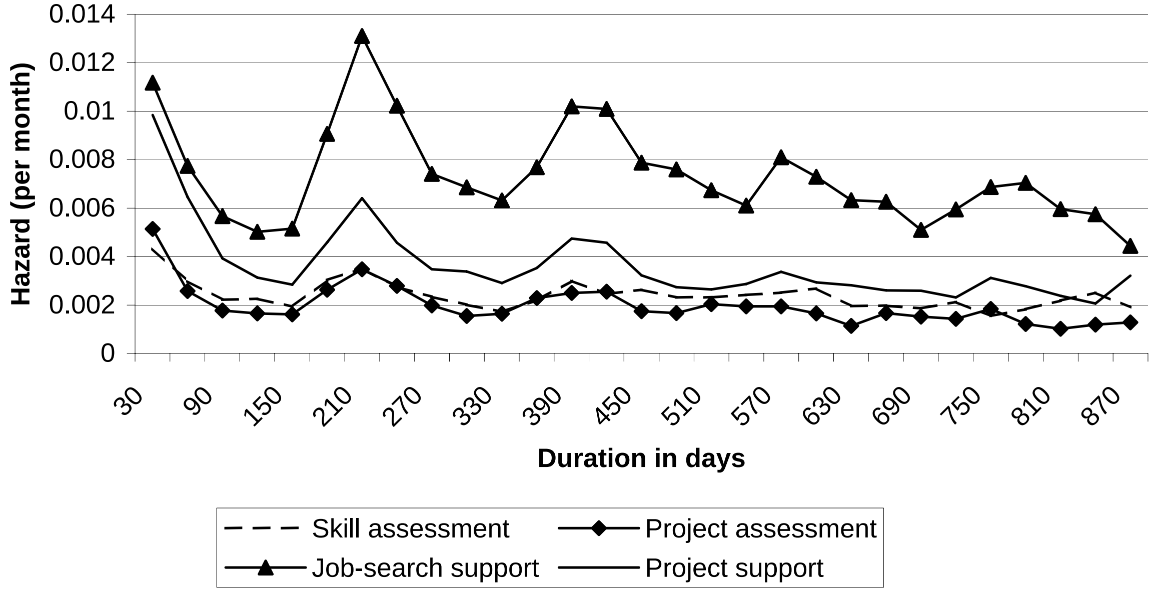


Figure 2: Unemployment and employment empirical duration dependence - Kaplan-Meier estimates

Figure 3: Schemes estimated duration dependence (from the full model)

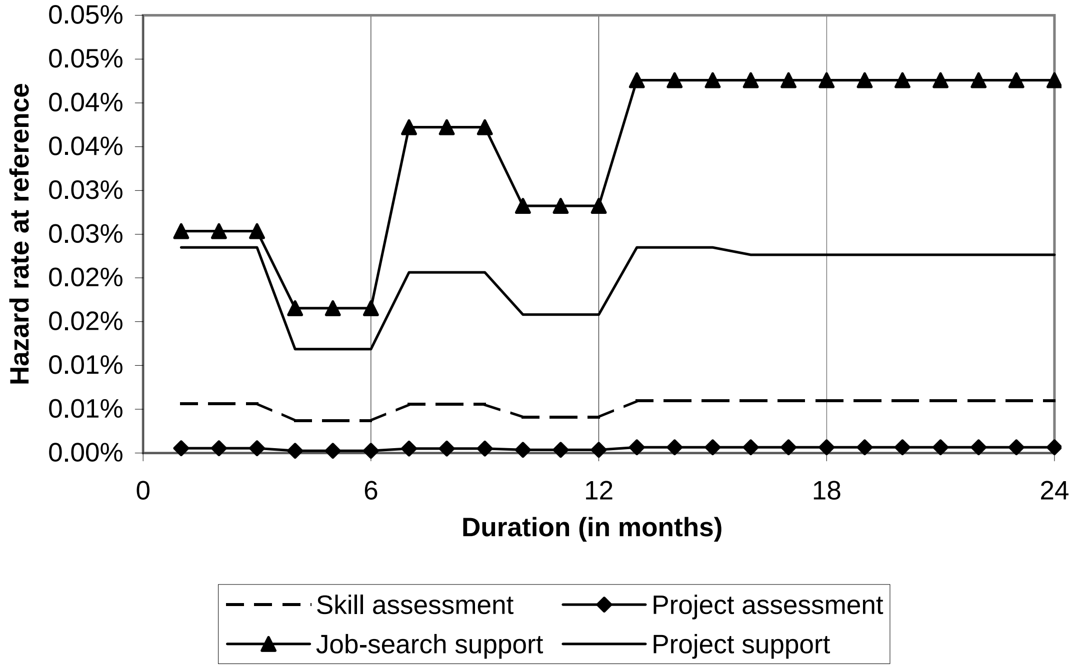


Figure 4: Unemployment and employment estimated duration dependence (from the full model)

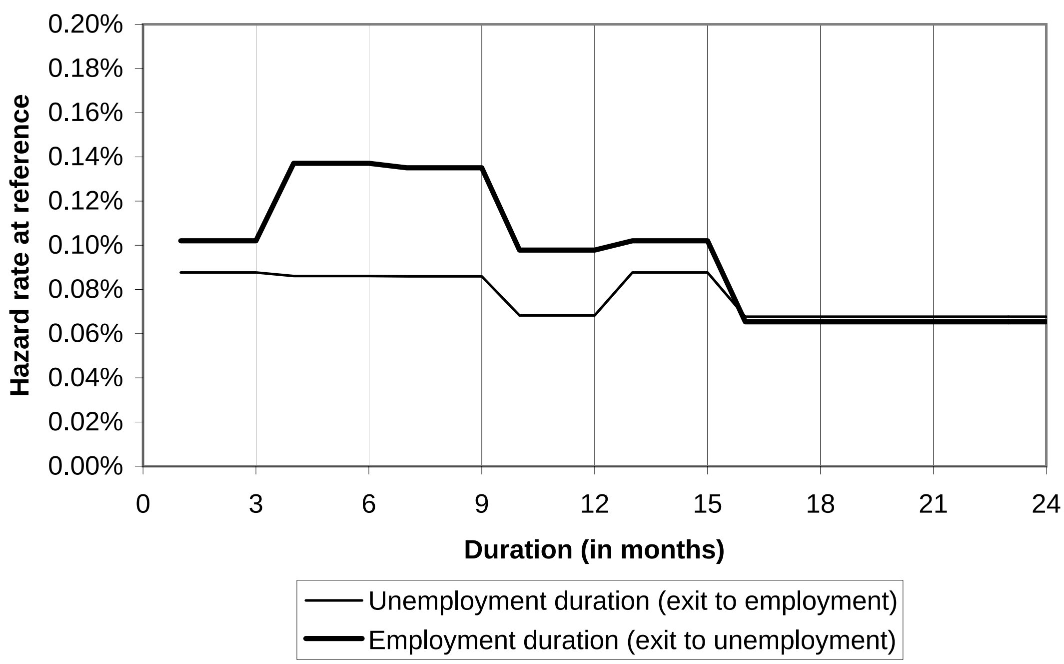


Figure 5a: Simulated effect of Skill assessment

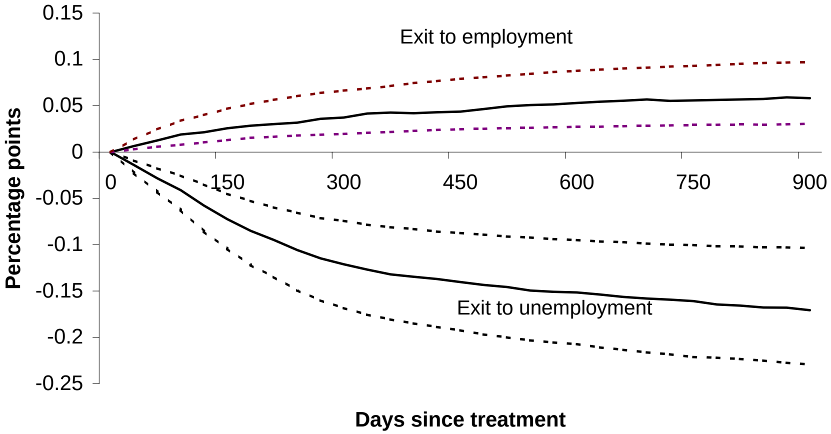


Figure 5b: Simulated effect of Project assessment

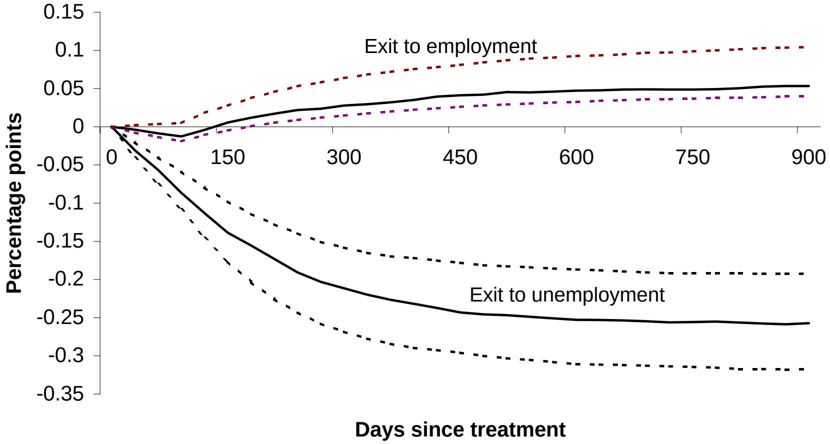


Figure 5c: Simulated effect of Job-search support

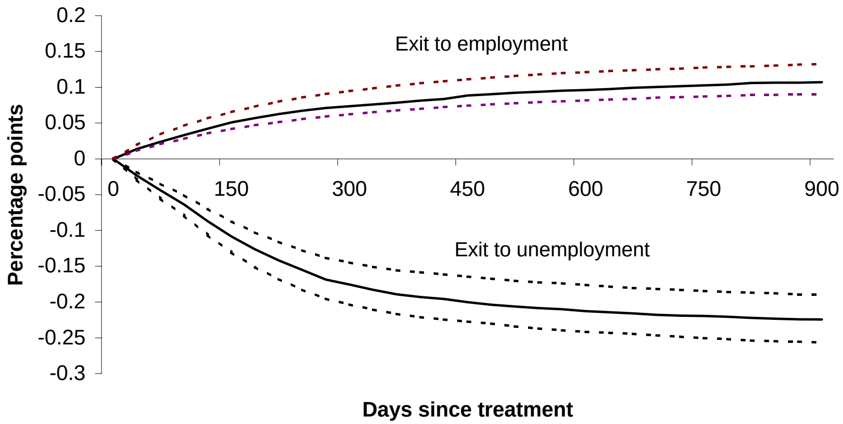
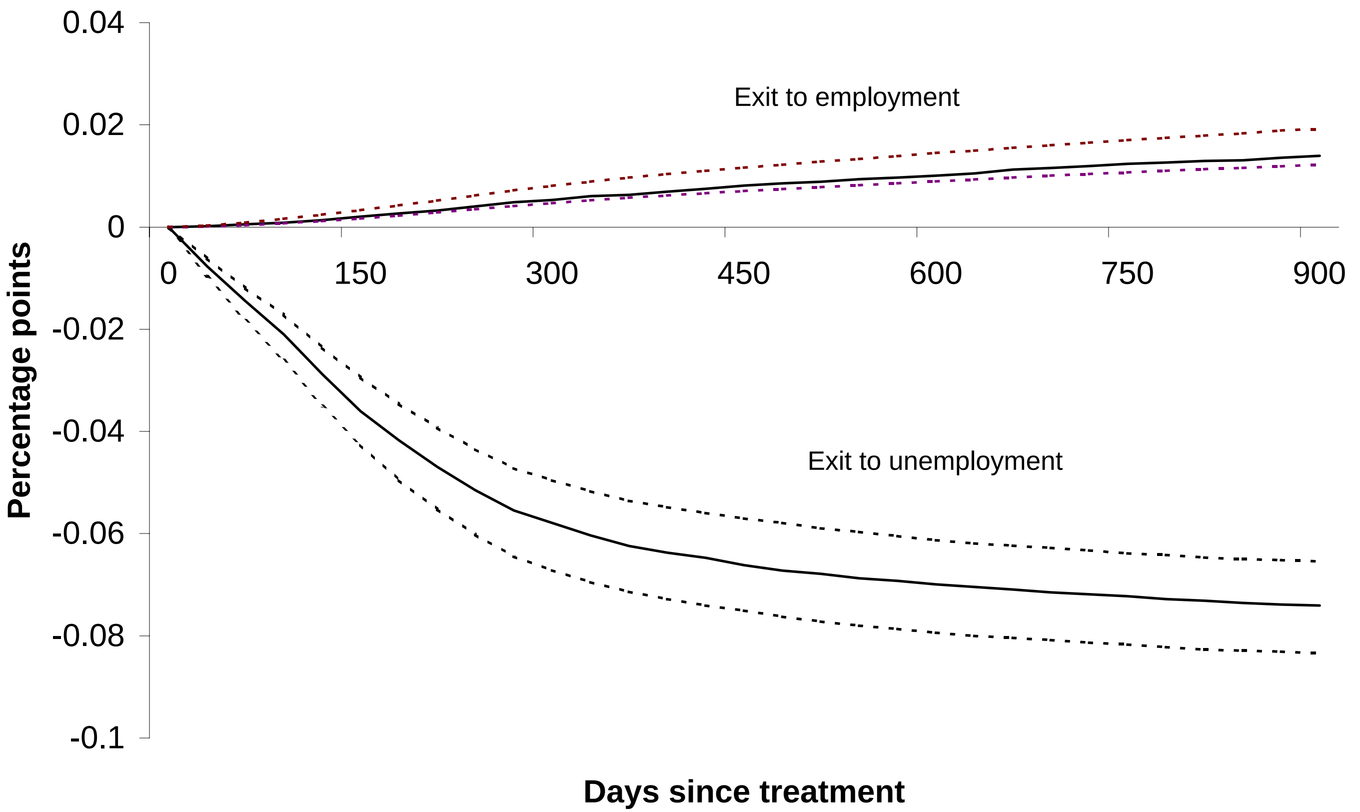


Figure 5d: Simulated effect of Project support

Shifts in the cumulative exit rates to employment and to unemployment due to program participation, for the population that received each treament. The shift to employment is the shift t days after treatment has been received (it mixes individuals that received treatment at different dates).

Figure 6: General simulated effect of all treatments



Shift in the global cumulative exit rate to employment, t days after entering unemployment, due to program participation, accounting for program assignment up to day t, and shift in the global cumulative exit rate to unemployment, t days after the begining of an employment spell (individual exiting to employment before receiving treatment have a zero treatment effect).