DISCUSSION PAPER SERIES

No. 2604

USING MATCHING ESTIMATORS TO EVALUATE ALTERNATIVE YOUTH EMPLOYMENT PROGRAMS: EVIDENCE FROM FRANCE, 1986–8

Thomas Brodaty, Bruno Crépon
and Denis Fougère

LABOUR ECONOMICS

Centre for Economic Policy Research

www.cepr.org

ISSN 0265-8003

USING MATCHING ESTIMATORS TO EVALUATE ALTERNATIVE YOUTH EMPLOYMENT PROGRAMS: EVIDENCE FROM FRANCE, 1986–8

Thomas Brodaty, EUREQUa
Bruno Crépon, CREST-INSEE
Denis Fougère, CREST-INSEE and CEPR

Discussion Paper No. 2604
November 2000

Centre for Economic Policy Research
90–98 Goswell Rd, London EC1V 7RR, UK
Tel: (44 20) 7878 2900, Fax: (44 20) 7878 2999
Email: cepr@cepr.org, Website: www.cepr.org

CEPR Discussion Paper No. 2604

November 2000

ABSTRACT

Using Matching Estimators to Evaluate Alternative Youth Employment Programs: Evidence from France, 1986–8*

In this Paper we apply the statistical framework recently proposed by Imbens (1999) and Lechner (1999) to identify the causal effects of multiple treatments under the conditional independence assumption. We show that under this assumption, matching with respect to the ratio of the scores allows us to estimate non-parametrically the average conditional treatment effect for any pair of treatments. Consequently it is possible to estimate this effect by implementing non-parametric matching estimators, which were recently studied by Heckman, Ichimura, Smith and Todd (1998) and Heckman, Ichimura and Todd (1998). The application concerns the youth employment programs that were set up in France during the 1980s to improve the labour market prospects of the most disadvantaged and unskilled young workers. The empirical analysis makes use of non-experimental longitudinal micro data collected by INSEE (Institut National de la Statistique et des Etudes Economiques, Paris) from 1986 to 1988.

JEL Classification: J24, J68
Keywords: competing-risks duration models, econometric evaluation methods, matching estimators, multiple treatments, youth employment policies

Thomas BRODATY
EUREQUa
Université de Paris 1
Maison des Sciences Economiques
106-112, Boulevard de l'Hopital
75647 Paris Cedex 13,
FRANCE.
Tel: (33 1) 44 07 82 17.
Fax: (33 1) 44 07 82 31.
E-mail: brodaty@univ-paris1.fr

Bruno Crépon
CREST-INSEE
15 Boulevard Gabriel Péri
92245 Malakoff
FRANCE
Tel: (33 1) 41 17 60 3
Fax: (33 1) 41 17 60 4
Email: crepon@insee.fr

Denis Fougère
Research Director
Microeconomics Laboratory
CREST/INSEE
15 Boulevard Gabriel Péri
92245 Malakoff Cédex
FRANCE
Tel: (33 1) 41 17 77 13
Fax: (33 1) 41 17 76 34
Email: fougere@ensae.fr

*This Paper is a revised version of our presentation to the ZEW Research Conference on 'Econometric Evaluations of Active Labour Market Policies in Europe' (Mannheim, 25–6 June 1999). We thank Bernd Fitzenberger, James J Heckman, Guido W Imbens and Michael Lechner for their very valuable comments and remarks during this highly stimulating Conference. We thank also Moshe Buchinsky, Andrew Foster, Tony Lancaster, Jean-Marc Robin and participants in seminars at Brown University, Halle-Salle (Germany), EUREQUa (Université Paris-1), INSEE (Paris), Toulouse, Marseille, and conferences in Milano (EALE/SOLE 2000 Conference) and Seattle (World Congress of the Econometric Society, August 2000). Their suggestions and the remarks of two referees helped us to improve substantially the first version of our communication. All remaining errors are ours.

Submitted 28 September 2000

NON-TECHNICAL SUMMARY

In this Paper we apply the statistical framework developed by Imbens (1999) and Lechner (1999) to identify and to estimate the causal effects of multiple treatments under the conditional independence assumption. The application concerns the youth employment programs that were set up in France during the eighties to improve the labour market prospects of the most disadvantaged and unskilled young workers. The empirical analysis makes use of non-experimental longitudinal micro data collected by INSEE (Institut National de la Statistique et des Etudes Economiques, Paris) from 1986 to 1988. These data are based on administrative records supplemented by a series of four interviews over one and a half years; they provide information on the dates of entry into training programs and on the duration of subsequent spells of employment and unemployment. These data were previously used by Bonnal, Fougère and Sérandon (1997) to estimate the impact of youth employment schemes on subsequent unemployment and employment duration of recipients using a reduced-form multi-state multi-spell transition model that includes participation in these programs as an additional state.

In this Paper, we propose to re-examine the impact of these programs on the subsequent employment status by implementing matching estimators, which were recently studied by Heckman, Ichimura, Smith and Todd (1998) and Heckman, Ichimura and Todd (1998). Such estimators are derived from a causal model and their identification does not rely on the assumption of constant treatment effects and on distributional assumptions. Let us recall briefly the statistical framework that is presented more extensively in Imbens (1999) and Lechner (1999). Evaluation methods usually try to compare two potential outcomes, which are associated with two regimes, generally called treatment and non-treatment. Identification assumptions as well as estimation methods have been extensively studied in this context. The conditional independence assumption, which states that the assignment to treatment T and the response variable Y are conditionally independent given observable covariates X, has received a lot of attention. It leads to various estimation methods in which the propensity score of being treated plays a key role. However, treatments are usually not homogenous in practice, at least in the field of the evaluation of active labour market policies. The treatment status is the aggregation of various treatments whose efficiency may strongly differ. So it is of interest to adapt the previous methods to the case where mutually exclusive treatments are possible and to examine how their relative efficiency can be estimated. We introduce $(K+1)$ treatments. The assignment to one specific treatment k ($k=0,\dots,K$) is defined by $T=k$ and the potential output associated with treatment k is denoted Y_k. Our parameters of interest are $E(Y_k - Y_{k'} \mid T=k)$. For identifying the relative effect of treatment k with respect to treatment $k' \neq k$, we assume that the treatment indicator is conditionally independent of the potential outputs given the values of the observable covariates; this assumption is denoted by $(Y_0 \ldots Y_K) \perp T \mid X$. Then we apply matching methods developed by Heckman et al. (1998) to the individuals who receive treatments *k* or *k'*. Thus our evaluation of treatment *k* against treatment *k'* is not the same as our evaluation of treatment *k'* against treatment *k.*

The literature on estimation by matching has often emphasized the importance of the propensity score specification. Due to the fact that our sample is extracted from the stock of unemployed people at a given date (August 1986) and is subject to right-censoring, a natural specification of the treatment probabilities may be derived from a competing-risks duration model. The complete design of the process evaluated can be summarized as follows: initially (August 1986), all individuals are unemployed; when exiting unemployment, they may enter regular employment or one among two types of programs, the first type including ‘community jobs’ in the public sector and short training programs in public training centres, the second corresponding to workplace training programs in the private sector; the sample observation ends in May 1988; all durations are measured in months; the outcome variables are alternatively the probability to be employed in a regular job 1, 3 or 6 months after the end of the program, and the number of months spent in regular employment over the six-month period following the program.

Usually the literature on evaluation distinguishes between a selection bias that may result from selection on observables and/or from selection on unobservables (see, for instance, Heckman and Robb (1985), or Heckman and Hotz (1989)). Due to the form of the above conditional independence assumption, it should be mentioned that our Paper obviously considers the situation where selection only results from characteristics that are observable to the analyst. This is an important difference to the study by Bonnal, Fougère and Sérandon (1997) which is based on the assumption that both observable and unobservable characteristics affect the process of assignment to programs. The fact that in our study, the intensity of transition from the initial unemployment spell to other states is allowed to be affected by more observable covariates than in Bonnal, Fougère and Sérandon (1997), is an argument for using the conditional independence assumption. If this assumption would not hold, alternative evaluation strategies could be the ones implemented by Bonnal, Fougère and Sérandon (1997) who consider selection on unobservables.

Our results highlight the variability of program effects, both between programs and among recipients of the same program. We also show that, when one program performs, on average, better than another one, its relative efficiency tends to increase with the ratio of the propensity scores. For instance, if the output variable is the probability to be employed in a job with a long-term contract, or the time spent in the employment state over the six-month period after the program, there are no significant differences between programs. On the whole, it appears that a job with a fixed-term contract is more effective than the employment programs. Among these programs, on-the-job training programs in the private sector (associated with higher amounts of vocational and specific training) give better results than the programs in the public sector. This general result confirms the conclusions of the paper written by Bonnal, Fougère and Sérandon (1997), that were deduced from a very different approach.

But our Paper contains further results. We have also studied the relative effects of the different programs on subintervals of the common support, that is for particular values of the conditional probabilities. This exercise allowed us to emphasize the variability of the effects of a program for recipients who have very different conditional probabilities to participate. We found that, in general, comparisons between various treatments show that positive effects on the whole common support are usually associated with significant positive effects on the highest part of the support and no significant effect on the lower part; at the opposite end, negative effects on the whole common support are usually associated with significant negative effects on the lower part of the support and no significant effect on the highest part. Positive effects on the higher part of the support suggest that the highest effectiveness is obtained for individuals who have the highest conditional probability to participate. Thus our results give an idea of what could be a way of improving the assignment of applicants through treatments.

1 Introduction

In this paper we apply the statistical framework developed by Imbens (1999) and Lechner (1999) to identify and to estimate the causal effects of multiple treatments under the conditional independence assumption. The application concerns the youth employment programs which were set up in France during the eighties to improve the labor market prospects of the most disadvantaged and unskilled young workers. The empirical analysis makes use of nonexperimental longitudinal micro data collected by INSEE (Institut National de la Statistique et des Etudes Economiques, Paris) from 1986 to 1988. These data are based on administrative records supplemented by a series of four interviews over one and a half years; they provide information on the dates of entry into training programs and on durations of subsequent spells of employment and unemployment. These data were previously used by Bonnal, Fougère and Sérandon (1997) to estimate the impact of youth employment schemes on subsequent unemployment and employment durations of recipients using a reduced-form multi-state multi-spell transition model that includes participation in these programs as an additional state.[1]

In this paper, we propose to re-examine the impact of these programs on the subsequent employment status by implementing matching estimators, which were recently studied by Heckman, Ichimura, Smith and Todd (1998) and Heckman, Ichimura and Todd (1998). Such estimators are derived from a causal model and their identification do not rely on the assumption of constant treatment effects and on distributional assumptions.

Let us recall briefly the statistical framework which is presented more extensively in Imbens (1999) and Lechner (1999). Evaluation methods usually try to compare two potential outcomes which are associated with two regimes, generally called treatment and non treatment. Identification assumptions as well as estimation methods have been extensively studied in this context. The conditional independence assumption, which states that the assignment to treatment T and the response variable Y are conditionally independent given observable covariates X, has received a lot of attention. It leads to various estimation methods in which the propensity score of being treated plays a key role. However, treatments are usually

1 There are just a few empirical studies using French data that adopt the spirit of the literature on program evaluations (Heckman and Smith, 1995). Almost all of them use observational data, as opposed to experimental data. In addition, just a few among the few control for selection on unobserved heterogeneity (Bonnal, Fougère and Sérandon, 1997, Magnac, 1997). Their main results can be summarized as follows. Training programs directed at unemployed young persons have no effect on post-training wages or employment probabilities unless they have a large training content. On the other hand, payroll tax subsidies have significant effects on employment probabilities of low-wage workers, but their largest effects concern workers between 25 and 30 (see, Fougère, Kramarz and Magnac, 2000).

not homogenous in practice, at least in the field of the evaluation of active labor market policies. The treatment status is the aggregation of various treatments whose efficiency may strongly differ. So it is of interest to adapt the previous methods to the case where mutually exclusive treatments are possible, and to examine how their relative efficiency can be estimated. We introduce $(K+1)$ treatments. The assignment to one specific treatment k ($k=0,\ldots,K$) is defined by $T=k$, and the potential output associated with treatment k is denoted Y_k. Our parameters of interest are $E(Y_k - Y_{k'} \mid T=k)$. For identifying the relative effect of treatment k with respect to treatment $k' \neq k$, we assume that the treatment indicator is conditionally independent of the potential outputs given the values of the observable covariates; this assumption is denoted by $(Y_0 \ldots Y_K) \perp T \mid X$.[2] Then we apply matching methods developpped by Heckman et al. (1998) to the individuals who receive treatments k or k'. Thus our evaluation of treatment k against treatment k' is not the same as our evaluation of treatment k' against treatment k.

The literature on estimation by matching has often emphasized the importance of the propensity score specification. Due to the fact that our sample is extracted from the stock of unemployed people at a given date (August 1986) and is subject to right-censoring , a natural specification of the treatment probabilities may be derived from a competing-risks duration model. The complete design of the process evaluated can be summarized as follows: initially (August 1986), all individuals are unemployed; when exiting unemployment, they may enter regular employment or one among two types of programs, the first type including "community jobs" in the public sector and short training programs in public training centers, the second corresponding to workplace training programs in the private sector; the sample observation ends in May 1988; all durations are measured in months; the outcome variables are alternatively the probability to be employed in a regular job 1, 3 or 6 months after the end of the program, and the number of months spent in regular employment over the six months period following the program.

Usually the literature on evaluation distinguishes between a selection bias that may result from selection on observables and/or from selection on unobservables (see, for instance, Heckman and Robb (1985), or Heckman and Hotz (1989)). Due to the form of the above conditional independence assumption, it should be mentioned that our paper obviously considers the situation where selection only results from characteristics which are observable to the analyst. This is an important difference to the study by Bonnal, Fougère and Sérandon (1997) which is based on the assumption that both observable and unobservable characteristics affect the process of assignment to programs. The fact that in our study, the intensity of transition from the initial unemployement spell to other states is

2 Usually the symbol $\perp$ stands for orthogonality and not for statistical independence. Due to the inability of the software to produce the correct symbol, $\perp$ stands for statistical independence throughout our text.

allowed to be affected by more observable covariates than in Bonnal, Fougère and Sérandon (1997), is an argument for using the conditional independence assumption. If this assumption would not hold, alternative evaluation strategies could be the ones implemented by Bonnal, Fougère and Sérandon (1997) and by Magnac (1997) who consider selection on unobservables.

Our results highlight the variability of program effects, both between programs and among recipients of the same program. We also show that, when one program performs on average better than another one, its relative efficiency tends to increase with the ratio of the propensity scores. In the next Section, we recall the general framework for the evaluation problem with multiple treatments and we show that, under the conditional independence assumption, matching with respect to the ratio of the scores $Pr(T=k \mid X)$ and $Pr(T=k' \mid X)$ allows to estimate nonparametrically the average conditional treatment effect $E(Y_k - Y_{k'} \mid T=k)$ for a pair of treatments k and k'. Section 3 gives a description of youth employment programs in France and Section 4 presents the data we use. In Section 5, we introduce the specification of our propensity scores, which are derived from a competing-risks duration model, and we discuss their estimates. In Section 6, we report and comment the results obtained for different response variables through kernel matching estimation. Section 7 concludes.

2 The Evaluation Problem with Multiple Treatments

The general framework that we use is the one developed by Imbens (1999) and Lechner (1999) for the evaluation of programs involving multiple exclusive treatments. This framework generalizes the modelling that Rubin (1974, 1977) introduced for the case of a unique treatment. Let us recall briefly the formalism introduced by Imbens (1999) and Lechner (1999). We assume that there are K+1 exclusive treatments, denoted 0,1,…,K, the value 0 corresponding to the absence of treatment. For the individual i the assignment to a given treatment is indicated by the variable T_i taking values in $\{0,1,\ldots,K\}$. K+1 potential outputs, which are denoted $Y_{0,i}, Y_{1,i}, \ldots, Y_{K,i}$ are associated with the K+1 possible treatments.

The identifying assumption studied in these papers is the conditional independence of the treatment indicator and the potential outputs given the values of the observable covariates. This assumption means that there exists a set of observables X_i such that $(Y_{0,i}, Y_{1,i}, \ldots, Y_{K,i}) \perp T_i \mid X_i$. As a generalization of Rubin's results, various parameters of the distribution of treatment effects may be identified for any pair of treatments $\{k, k'\}$, $k \neq k'$; for instance, we may then identify the average unconditional effect of treatment k with respect to treatment k', equal to $E(Y_{k,i} - Y_{k',i})$, or the average conditional effect given that individual i is assigned to treatment k denoted $E(Y_{k,i} - Y_{k',i} \mid T_i = k)$. Lechner (1999) also considers the conditional expectation $E(Y_{k,i} - Y_{k',i} \mid T_i \in \{k\ ,\ k'\},\ k \neq k')$, which is specific to this framework.

The estimation methods of these parameters extend the ones used in the one-treatment case initially proposed by Rubin (1977) under the assumption of conditional independence on observables. In this literature, an important practical result, due to Rosenbaum and Rubin (1983), is that conditional independence on observables implies conditional independence on the propensity score. In particular, this result allows for one-dimensional matching instead of full matching on all characteristics. Recently this procedure was extensively studied by Heckman and his coauthors in a series of papers where the matching principle is extended through kernel or nearest neighbour techniques to provide a non parametric estimate of the treatment effect given the value of the propensity score (see, for instance, Heckman, Ichimura and Todd, 1998, Heckman, Ichimura, Smith and Todd, 1998, and Heckman and Smith, 1999).[3]

In this paper, we mainly focus on average conditional treatment effects given assignment to treatment k namely $E(Y_{k,i} - Y_{k',i} \mid T_i=k)$. Results available up to now require to match observations simultaneously on the two scores $Pr(\ T_i=k \mid X_i\ )$ and $Pr(\ T_i=k' \mid X_i\ )$. The following proposition shows that matching with respect to the ratio of these scores is sufficient to purge of the selectivity-on-observables bias. In this context, it is therefore possible to use directly the kernel matching techniques developed by Heckman, Ichimura and Todd (1998) and Heckman, Ichimura, Smith and Todd (1998) to estimate our parameters of interest.

Proposition 1. If the conditional independence assumption

$$(Y_{0,i}\ , Y_{1,i}\ ,\ldots, Y_{K,i}) \perp T_i \mid X_i$$

holds, then $\forall\ k \neq k'$

$$(Y_{k,i}\ , Y_{k',i}) \perp T_i \mid \Pi^{k/k'}(X_i)\ ,\ T_i \in \{k,k'\}$$

where $\Pi^{k/k'}(X_i)$ is a balancing score defined as $\Pi^{k/k'}(X_i) = \dfrac{\Pi^{k}(X_i)}{\Pi^{k}(X_i) + \Pi^{k'}(X_i)}$

and $\Pi^{k}(X_i) = Pr(\ T_i=k \mid X_i\ )$.

Proof. See Appendix.[4]

Two estimation methods may be derived from this property. The first method is the comparison of weighted means of outputs, and the second one is a matching procedure.[5]

---

3 Estimation by matching is not the only one technique which can be applied under the conditional independence assumption. Regression estimators or weighting techniques can also be implemented (see Dehejia and Wahba, 2000).

4 This point has also been shown by Lechner (1999).

Proposition 2. Estimation through weighting

Under the conditional independence assumption, the average treatment effect $E(Y_{k,i} - Y_{k',i} \mid T_i=k)$ given assignment to treatment k may be estimated as

$$E(Y_{k,i} \mid T_i=k) - E(Y_{k',i} \mid T_i=k) = E(Y_i \mid T_i=k) - E\left(Y_i \frac{\Pi^k(X_i)}{\Pi^{k'}(X_i)} \frac{P_i^{k'}}{P_i^k} \mid T_i = k \right) \quad (1)$$

where $P_i^k = \Pr(T_i = k)$.

Proof. See Appendix.

Proposition 3. Estimation through matching

To estimate the average conditional treatment effect $E(Y_{k,i} - Y_{k',i} \mid T_i=k)$ given assignment to treatment k, it is possible to match individuals receiving treatment k with individuals receiving treatment k' on the basis of the balancing score $\Pi^{k/k'}(X_i)$.

Proof. See Appendix.

3 Youth Employment Programs in France

Over the last twenty years, youth unemployment is the most striking feature of the French labor market. For workers between 15 and 24 years old, the unemployment rate increased from 13% in 1979 to 26.6% in 1999, after reaching a maximum, 29%, in 1987. This explains why active labor market policies were increasingly introduced in France since the mid-seventies, when unemployment started its increase (see DARES, 1996, for a historical description). These policies were targeted to the unemployed and to workers with the highest unemployment risks, among which young adults or older workers. These policies are similar to those implemented in other European countries (Scarpetta, 1993), France being a median user. Direct employment subsidies and incentives for human capital investments are the two main instruments of these policies. Almost any mixture of these two components can be found within French employment policies. For instance, public employment schemes such as community jobs (“Travaux d'Utilité Collective”) or the more recent program called “Contrats Emploi Solidarité” have

5 We will not conduct estimation through weighting in this paper. A further research will be devoted to the comparison of the relative performances of these two methods.

almost no component of training or learning by doing. At the other extreme, apprentice contracts have a very intensive training content.

Approximately fifty measures were implemented since 1974, even though only ten programs are still in use. These programs may be classified according to the characteristics of eligible participants, the level of implementation (local or national), the employment sector (public or private), or the legal status (training course or labor contract). Each year, 800,000 individuals between 15 and 25 years old are financially assisted through public programs which give them a training course or a subsidized job.[6]

Behind this profusion of measures, two main types of public interventions can be distinguished:

1. job creation in the public sector, thanks to massive wage subsidies, directed to low-skilled unemployed young adults,
2. promotion of training programs in the private sector, these programs include classroom education and on-the-job training in order to increase labor market experience and human capital.

Let us recall the main features of youth training programs which were in effect in France during the late 1980's. Most of these programs were introduced before, but the numbers of participants increased greatly after the 1986 Emergency Plan for Youth Employment ("Plan d'Urgence pour l'Emploi des Jeunes"). This Plan introduced strong incentives for private firms offering training places and facilitated the development of programs with alternating spells of work and training ("formations en alternance", for which we propose the term "workplace training programs"). For instance, the lower age limit for entry into such programs has been lowered from 18 to 16 years old, while the upper age limit for entry into the apprenticeship system has been raised from 20 to 25 years old.

To simplify, we can distinguish between two types of programs: the "workplace" training programs provided by private firms (including apprenticeship, qualification and adaptation contracts, and "courses for preparation to the working life"), and the "workfare" programs provided by the State and the public sector (including "community jobs" and "courses for the 16-to-25 years old"). For this second type of programs, the amount of vocational and specific training is generally lower. Table 1 gives an overview of the different programs that were set up in France during the eighties.

The apprenticeship contract is a training scheme which offers participants part-time work in the firm, complemented by part-time education in a public training

6 Of course, the number of recipients is lower, because the same young person may benefit from several programs in the same year. Let us recall that the number of recipients was highest in the mid-eighties: in 1987, almost one million young people benefited from the public programs.

center. Every participant prepares himself/herself for a national diploma; to obtain this diploma, a test has to be taken after completion of the contract. The applicant has to be between 15 and 25 years of age, the applicant must find a firm which is authorized to hire apprentices, and he/she has to be registered in a training center for apprentices. The apprenticeship contract, signed both by the employer and the employee, is registered by a local office of the Ministry of Employment and Social Policy. The usual length of an apprenticeship contract is two years, but it can vary between one and three years. The training is partly general, but it also comprises occupation-specific components. The apprentice is a wage-earner, and his/her wage is calculated as a fraction of the minimum wage level. At the end of the apprenticeship contract, the employee may be hired by the firm either under a fixed-term labor contract (FTC), or under a long-term labor contract (LTC).

The “Contrat de Qualification” is very similar to the apprenticeship contract. It is a fixed-term contract with length that may vary from 6 to 24 months. Every participant prepares himself/herself for a diploma as in apprenticeship contracts. This program is addressed to unskilled or long-term unemployed young adults. At least one-fourth of the contract period must be devoted to training. This training takes place during working hours and is approved by collective agreements. The participant is paid by the employer; the wage is equal to a fixed fraction of the monthly legal minimum wage, and this fraction varies according to the age of the participant and the seniority in the contract.

The “Contrat d'Adaptation” may be either a fixed-term labor contract with length that may vary from 6 to 12 months or a long-term labor contract. It is aimed to provide some specific training (adapted to the job). This program is addressed to skilled young people who have difficulties to find a job. Potential employers are all firms in craft, trade and industrial sectors. If the “adaptation contract” is a fixed-term labor contract, at least 200 hours must be devoted to training. If it is signed as a long-term labor contract, the amount of training depends both on the job and on the skill level of the applicant. The wage is paid by the firm; it is at least equal to the legal minimum wage. Firms signing “adaptation contracts” are exempted from paying the employer training tax but have to pay Social Security contributions.

“Courses for Preparation to the Working Life” (“Stages d'Initiation à la Vie Professionnelle”) are non renewable fixed-term labor contracts in the private sector, which are aimed to offer some general training to young people with no work experience or who are unemployed for more than one year. The training is provided either by the firm or by a government training center. Trainees receive a lump-sum from the State and a complementary allowance from the firm. Firms offering such courses are exempted to pay Social Security contributions.

The program called “Travaux d'Utilité Collective” (or “Community Jobs”) was set up in 1984 and suppressed in 1990. In these programs, hiring of low-educated jobless young adults and long-term unemployed in community service jobs is heavily subsidized; the objective being not only to give a job but also to increase employability. Employers are public institutions, local administrations and non-profit associations. The “community job” contract is a part-time (20 hours a week) fixed-term (from 3 to 12 months) employment contract. From 1987, contract length has be extended to 24 months for people with poor employment prospects. The hourly wage is the legal hourly minimum wage. It is entirely paid by the State. The employer is exempted from Social Security contributions but not from Unemployment Insurance contributions.

“Courses for 16 to 25 years old” (“Stages pour les 16-25 ans”) were training courses offered by State training centers. Their length varied from 6 to 9 months and the time devoted to training was between 550 and 700 hours. These courses were aimed to facilitate social and professional integration of young people leaving the educational system without any diploma or qualification. Trainees received a lump-sum from the State.

Table 1: Main programs for youth employment in France during the period 1986-1988

	Apprenticeship contracts	Qualification contracts	Adaptation contracts
Durations	Temporary employment contracts (between 1 and 3 years)	Temporary employment contracts (between 6 and 24 months)	Temporary employment contracts (from 6 to 12 months) or permanent employment contracts
Objectives	To provide a specific training giving a formal qualification or allowing to take examination for national diploma after completion	Idem	To provide a specific training (adapted to the job occupied)
Eligible workers	Young workers without any diploma or without certified skills	Idem	Young skilled workers who have difficulties to find a job
Potential employers	All private firms in craft, trade and industrial sectors	All private firms	Idem
Amount of training	At least 400 hours of training for non college graduates; at least 1500 hours of training for college graduates	At least one quarter of the contract duration	At least 200 hours in a temporary contract; for a permanent employment contract, it depends both on the job and on the worker qualification
Wage levels	The apprenticeship is paid by the firm, the wage depends on age and seniority in the contract (between 17 and 75% of the legal minimum wage)	Idem	The wage is paid by the firm; it is at least equal to the minimum wage
Employer incentives	Firms are exempted from paying social security contributions	Firms are exempted from paying social contributions and the employer training tax	Firms are exempted from paying the employer training tax, but have to pay social security contributions (July 87)

Table 1 (continued): Main programs for youth employment in France during the period 1986-1988

	Courses for preparation to the working life (SIVP)	Community jobs (TUC)	Training courses for 16 to 25 years-old
Durations	Non renewable temporary contracts	Non renewable temporary contracts (from 3 to 6 months)	Courses with a duration between 6 and 9 months
Objectives	To give a formal qualification (adapted to existing jobs)	To help young people to find a regular job	To facilitate social and professional integration
Eligible workers	Young people with no work experience or unemployed for more than one year	Young workers between 16 and 21 years old, long term unemployed between 22 and 25 years old	Young people leaving the educational system without any qualification
Potential employers	All private firms	State or local administrations, public institutions, non-profit making associations…	Courses take place in public training centers
Amount of training	Training provided either by the firm or by a public training center	No formal or specific training	Between 550 and 700 hours of training
Wage levels	Trainees receive a lump-sum from the State and a complementary allowance from the firm	Trainees are paid by the State, receive a fixed payment (1250FF), and sometimes an allowance from the firm	Trainees receive a lump-sum from the State
Employer incentives	Firms are exempted from paying social security contributions		

4 The Data

The data for our study are provided by the “Suivi des Chômeurs” (or “Histories of Unemployed”) survey collected by INSEE (Paris). The sample has been drawn randomly in August 1986 from the files of the public employment service (“Agence Nationale Pour l'Emploi” or ANPE[7]). About 8000 unemployed people were sampled but only 7450 could be reached at the first interview. Individuals were interviewed four times, in November 1986, May 1987, November 1987, and finally May 1988. At the first inquiry, respondents were asked to give information on their labor market status between August and November 1986, and in particular on the time already spent in the unemployment spell sampled in August 1986 and on their status before entry into that spell. The data record retrospectively month for month, between November 1986 and May 1988, the events corresponding to individual transitions in the labor market. For this study, we consider young unemployed who were less than 27 years old in August 1986 and for whom it is possible to observe an accurate and relevant date of registration in the ANPE files. The subsample includes 3160 individuals.[8] For each individual whose unemployment spell is not right censored, we observe either a transition to a regular job with a long-term duration labor contract (LTC) or with a fixed-term labor contract (FTC), either a transition to the out-of-labor-force (OLF) state, or a transition to one among the following employment programs:

- a workplace training program (an apprenticeship contract, a qualification contract, or an adaptation contract),
- a course for preparation to the working life (CPWL hereafter),
- a community job (CJ hereafter),
- or a training course “for 16 to 25 years old” (this category is also called “other programs” hereafter).

Table 2 gives the number and the destination states of transitions from the initial unemployment spell, given gender, age and educational level (below or above the “Baccalauréat”, which is the terminal high school diploma in France).The treatments we are interested in are these four types of employment programs. A job with a fixed-term labor contract (FTC) is considered as an additional

[7] These files include all unemployed people registered at the ANPE who were looking either for a full-time or part-time permanent job, or a full-time or part-time temporary job in August 1986. These requirements do not correspond to the definition of unemployment given by the International Labour Office.

[8] The selection rule for the chosen sample in our study differs to the one used in Bonnal, Fougère and Sérandon (1997) from two aspects; first we include men and women in our sample, while Bonnal, Fougère and Sérandon (1997) examined young males only; then, instead of younger than 26, the age limit is increased to younger than 27.

treatment. This allows us to compare each of the four programs with a reference treatment which is not strictly speaking the no-treatment case. This comparison makes sense because these programs and FTC jobs both offer temporary employment to participants. Moreover, empirical evidence shows that firms have substituted subsidized workplace training programs for FTC jobs after the introduction of such programs (DARES, 1996). Then the question is to know if, in comparison with FTC jobs, programs facilitate or postpone the access to stable jobs (with long-term labor contracts).

Table 2: Number of transitions from the initial unemployment spell

Transition to	Total	Women	Age≤23	High-school and above
LTC jobs	726	371	356	108
FTC jobs	703	358	380	108
OLF state	298	183	194	38
Workplace training	52	27	39	3
CPWL	194	96	139	24
CJ	244	153	211	25
Other programs	286	162	135	38
Right-censored[9]	657	436	340	63
Total	3160	1786	1794	487

5 Estimation of the Propensity Scores

In all evaluation studies using the propensity score methodology, specifying and estimating the conditional probabilities of receiving the different possible treatments (or transiting to the different programs) is the first and fundamental step. Nonparametric or semiparametric estimation of this conditional model (which may be specified for example as a multiple qualitative response model) is certainly the best strategy. But our data set is subject to right-censoring and to a stock sampling bias, and, at our knowledge, correcting for such problems is not possible without imposing some additional restrictions on the functional form of

9 The subset of right-censored observations includes the individuals who exited from the panel because of attrition in November 1986, May 1987, or November 1987, and the individuals who were still in their sampled unemployment spell at the end of the observation period (May 1988).

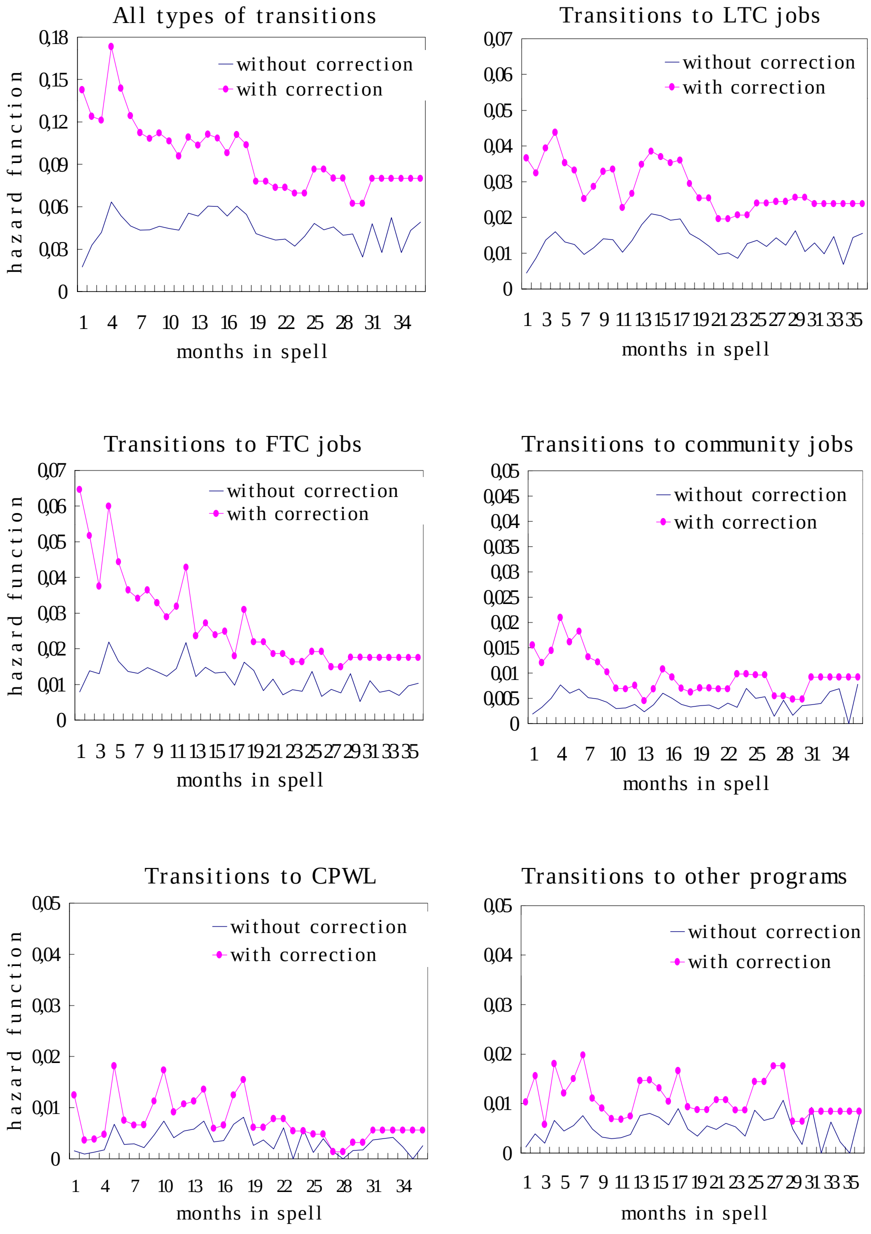
the transition probabilities. The potential effect of the unemployment duration on the process of assignment to treatments has naturally led us to derive the conditional probabilities of transiting from unemployment to the various treatments from a competing risks duration model.

5.1 The Competing-Risks Duration Model

Figure 1 illustrates the bias due to the fact that the sample has been extracted from the stock of unemployed people in August 1986. For example, on the first graph, we represent the Kaplan-Meier estimate of the overall hazard function of the duration spent in the sampled unemployment spell, which is obtained without any correction of the stock sampling bias, vs. the conditional maximum likelihood estimate of a piece wise constant hazard function taking into account some correction of the bias. This correction consists in weighting each observation by the inverse of the probability to be still unemployed at the sample date (August 1986), which is equal to the survivor function of the unemployment duration calculated as the difference between the sampling date and the date of entrance into the sampled unemployment spell.[10] The other graphs represent the same estimates for each specific hazard function associated with a given transition. These graphs show that the unemployment durations sampled from the stock underestimate seriously the hazard function and thus overestimate the average unemployment duration.

[10] This correction term is given, for instance, in Bonnal, Fougère and Sérandon (1997, p. 698-699).

Figure 1: Estimates of the hazard function of the first observed unemployment spell, with and without correction of the stock sampling bias



To estimate the propensity scores associated with the different programs and employment states, we make use of a competing-risks duration model whose estimation takes into account the stock sampling bias correction. More precisely, we assume that the rate of a transition from unemployment to a given state k (k=1,....7) has the following Weibull proportional hazard form

$$h_k\left(u \mid \beta_k, \alpha_k, X\right) = \alpha_k . u^{\alpha_k - 1} . \exp\left(\beta_k^{'} X\right), \alpha_k > 0, \qquad (2)$$

where u represents the duration of the unemployment spell, α_k is the (unknown) time-dependence parameter of the baseline intensity of transition from unemployment to state k, and β_k is a vector of unknown parameters associated with the fixed individual covariates X.[11] The survivor function for a duration in unemployment equal to u is

$$S\left(u \mid \alpha_1,...,\alpha_7, \beta_1,..., \beta_7, X\right) = \exp\left[-\sum_{l=1}^{7} u^{\alpha_l} \exp\left(\beta_l^{'} X\right)\right] \qquad (3)$$

and the propensity score, which is equal to the probability that the unemployment spell ends with a transition to a state k (k=1,...7), has the form

$$\begin{aligned} &\Pr\left(K = k \mid \alpha_1,...,\alpha_7, \beta_1,..., \beta_7, X\right) \\ &= \int_0^{\infty} \alpha_k u^{\alpha_k - 1} \exp\left(\beta_k^{'} X\right) \exp\left[-\sum_{l=1}^{7} u^{\alpha_l} \exp\left(\beta_l^{'} X\right)\right] du \end{aligned} \qquad (4)$$

Obviously, with a data set of unemployment spells sampled at a certain date (August 1986), the data is likely to overrepresent individuals with long unemployment durations. However, for the first step estimation of the balancing score, only the ratio of two participation probabilities is of interest. This last remark and the proportional hazards assumption could suggest to use a discrete choice model, because the conditional probability of a transition to state k, given that the unemployment duration is equal to u and greater than the difference s between the sampling date and the date of entrance into the sampled unemployment spell, is

11 In this version, we do not consider covariates whose value varies through the unemployment spell. Such covariates may have a significant effect on the selection process, but we ignore them for facilitating the estimation procedure. Bonnal, Fougère and Sérandon (1997) have found that one of the most important time-varying covariates, namely the qualification to unemployment insurance through the unemployment spell, has no significant effect on the transition rates from unemployment in this sample.

$$
\begin{aligned}
&\Pr\left(K = k \mid \alpha, \beta, X, u, u \geq s\right) \\
&= \frac{\Pr\left(K = k, D = u \mid \alpha, \beta, X, u \geq s\right)}{\Pr\left(D = u \mid \alpha, \beta, X, u \geq s\right)} \\
&= \frac{h_k\left(u \mid \beta_k, \alpha_k, X\right) \times S\left(u \mid \alpha, \beta, X\right) \big/ \Pr\left(D = u \mid \alpha, \beta, X, u \geq s\right)}{\sum_{j=1}^{K} h_j\left(u \mid \beta_j, \alpha_j, X\right) \times S\left(u \mid \alpha, \beta, X\right) \big/ \Pr\left(D = u \mid \alpha, \beta, X, u \geq s\right)} \qquad (5) \\
&= \frac{\alpha_k u^{\alpha_k - 1} \exp\left(\beta_k' X\right)}{\sum_{j=1}^{K} \alpha_j u^{\alpha_j - 1} \exp\left(\beta_j' X\right)} \\
&= \Pr\left(K = k \mid \alpha, \beta, X, u\right)
\end{aligned}
$$

Equation (5) shows that the competing-risks duration model has a logit multinomial representation in which the unemployment duration enters as a covariate, and whose specification does not depend on the stock sampling condition. However the conditional likelihood (5) is not valid for right-censored durations. Estimating the model (5) only for uncensored durations could give biased parameter estimates. Consequently, it is better to use the conditional likelihood derived from the competing-risks duration model (given the stock sampling condition) rather than the multinomial logit conditional model (5) in presence of right-censored data.

The competing-risks duration model also permits to estimate the probability to move from unemployment to a given treatment over a given subperiod of the unemployment spell, say between the thirteenth month and the twenty-fourth month spent in unemployment. This is of obvious interest because it allows to compare the training programmes with a long-term unemployment situation for those who did not leave unemployment over this time period. Such comparisons will be made in a further research.

5.2 Estimates

Table 3 gives parameter estimates of this competing-risks duration model with correction of the stock sampling bias.[12] For sake of brevity, we do not report the

12 Because unemployment spell durations are observed on a monthly scale in our data set, a model with grouped durations would have been more appropriate. In such a model, the likelihood contribution for an uncensored spell has the generic form:

estimates of parameters coresponding to intensities of transition from unemployment to long-term contract jobs, to the OLF state and to workplace training programs. The small number of transitions to workplace training programs (see Table 2) does not permit us to make inference on their relative effectiveness using kernel matching estimators. Let us remark that the estimated baseline intensities of transition from unemployment to the program called "Courses for Preparation to the Working Life" (CPWL hereafter) or to the category called "other programs" are constant through the unemployment spell (α is not significantly different from 1), while it is slightly but significantly decreasing for transitions from unemployment to jobs with fixed-term labor contracts (FTC hereafter) and to community jobs (CJ hereafter). These results are in line with the results obtained from the estimation of the piecewise constant hazard model without covariates, but with correction of the stock sampling bias (see Figure 1). Various covariates such as age, diploma, gender, marital status, health, type of housing, car ownership, regional dummies and previous labor market experience appear to have statistically significant but sometimes opposite effects on the intensities of transition from unemployment. For example, previous experience increases the intensity of transition from unemployment to FTC jobs but reduces very significantly the intensity of transition to community jobs; it has a smaller negative impact on the intensity of transition to "courses for preparation to the working life". Intensities of transition from unemployment to fixed-term labor contracts or to programs are lower for women and low-educated individuals; they decrease with age, with the exception of the category called "other programs".

Figure 2 presents nonparametric kernel estimates of the distributions of the balancing scores $\Pi^{k/k'}(X_i)$ for each pair (k , k') of treatments (programs) of interest. For example, the graph in the first window plots the distribution of the ratio of the conditional probability to move from unemployment to a job under a fixed-term labor contract (FTC hereafter) over the sum of this probability and the conditional probability to move from unemployment to a community job, for individuals who transited from unemployment to an FTC job (solid line) and for unemployed who effectively moved to to a community job (dotted line).

$$\Pr\left(U \in \left]S, S+1\right], T = k\right) = \int_S^{S+1} \alpha_k u^{\alpha_k - 1} \exp\left(\beta_k' X\right) \exp\left[-\sum_{l=1}^{7} \exp\left(\beta_l' X\right)\right] du$$

The parameter estimates of this model, which are not reproduced here, are very similar to the parameter estimates of the continuous time model specified in equations (2)-(3).

Table 3: Estimates of the parameters of the unemployment duration model

	LTC jobs	FTC	CJ	CPWL	Other Program	Appren- ticeship	OLF
Alpha	1.024 (0.038)	0.860 (0.035)	0.860 (0.057)	1.004 (0.070)	1.062 (0.062)	0.691 (0.108)	1.020 (0.058)
Intercept	-4.144 (0.205)	-3.507 (0.178)	-3.397 (0.259)	-4.716 (0.285)	-4.928 (0.286)	-3.963 (0.475)	-3.963 (0.266)
Women	-0.334 (0.086)	-0.262 (0.085)	-	-0.486 (0.156)	-0.148 (0.130)	-0.346 (0.282)	-0.580 (0.143)
Married men	0.229 (0.169)	0.418 (0.164)	-	-	-	-	-1.285 (0.719)
Married women	-0.491 (0.127)	-0.732 (0.137)	-1.473 (0.346)	-0.644 (0.282)	-0.678 (0.195)	-	0.957 (0.163)
Age ≤ 18	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Age 19-21	0.493 (0.126)	0.188 (0.089)	-	-	0.229 (0.189)	-0.594 (0.322)	-
Age 22-23	-	0.212 (0.101)	-0.947 (0.236)	-0.380 (0.196)	0.524 (0.204)	-0.888 (0.420)	-0.278 (0.166)
Age 24-25	0.544 (0.152)	-	-1.276 (0.334)	-0.759 (0.265)	0.672 (0.214)	-1.648 (0.619)	-0.702 (0.212)
Age 26-27	0.515 (0.170)	-	-2.328 (0.718)	-2.077 (0.590)	0.568 (0.254)	-1.704 (0.744)	-0.568 (0.231)
Dip1	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Dip2	-	0.271 (0.138)	-	-	0.393 (0.185)	-1.772 (1.018)	-
Dip3	0.362 (0.099)	0.352 (0.105)	-	0.238 (0.186)	-	-0.651 (0.409)	-0.368 (0.172)
Dip4	0.425 (0.126)	0.597 (0.122)	0.552 (0.186)	0.428 (0.240)	0.341 (0.200)	-1.811 (1.018)	-

Remarks: between parentheses are the standard errors; (-) if not included; educational levels are indicated by dip1 (elementary school), dip2 (junior high school only), dip3 (basic vocational technical shool), dip4 (elementary school and junior high school), dip5 (high school only), dip6 (advanced vocational technical school), dip7 (technical college and undergraduate), dip8 (graduate school and other post secondary education)

Table 3 (continued): Estimates of the parameters of the unemployment duration model

	LTC jobs	FTC	CJ	CPWL	Other Program	Appren-ticeship	OLF
Dip5	-	0.407 (0.208)	0.664 (0.332)	0.797 (0.398)	0.337 (0.284)	-	0.409 (0.280)
Dip6	0.396 (0.194)	0.508 (0.190)	0.480 (0.331)	0.965 (0.343)	0.475 (0.292)	-	-
Dip7	0.790 (0.204)	1.115 (0.190)	-	0.908 (0.472)	0.701 (0.334)	-	-
Dip8	1.138 (0.205)	0.611 (0.272)	-	-	-1.288 (1.012)	-	-
Foreigner	-	-	-	-	-	-	-0.671 (0.311)
Poor Health	-0.299 (0.097)	-0.219 (0.097)	-	-0.228 (0.188)	-	-	-
Having a car	0.341 (0.093)	0.288 (0.094)	0.185 (0.149)	-	-	-	0.306 (0.149)
Living with parents	-	-0.125 (0.110)	-	-	-	-	-0.263 (0.190)
Collective housing	-	0.391 (0.194)	-0.601 (0.515)	-	-	-	-
Regions:							
Nord	-0.504 (0.160)	-0.615 (0.178)	-	0.759 (0.211)	-0.541 (0.255)	-	-0.374 (0.217)
Picardie	-0.456 (0.194)	-	-0.572 (0.326)	0.539 (0.290)	-	-	-
Lorraine	-0.378 (0.178)	-0.773 (0.230)	-	-	-	-	-
Basse Normandie	-1.258 (0.713)	-	1.021 (0.467)	1.068 (0.596)	-	-	-

Remarks: between parentheses are the standard errors; (-) if not included; educational levels are indicated by dip1 (elementary school), dip2 (junior high school only), dip3 (basic vocational technical shool), dip4 (elementary school and junior high school), dip5 (high school only), dip6 (advanced vocational technical school), dip7 (technical college and undergraduate), dip8 (graduate school and other post secondary education)

Table 3 (continued): Estimates of the parameters of the unemployment duration model

	LTC jobs	FTC	CJ	CPWL	Other Program	Appren-ticeship	OLF
Regions:							
Bretagne	-0.556 (0.196)	-	-	0.710 (0.291)	-	-	-
Auvergne	-	-	-	1.251 (0.299)	0.590 (0.298)	-	-
Loire	-0.298 (0.168)	-	-	-	-	-	-
Bourgogne	0.460 (0.323)	-	-	-	-	-	-
Rhône Alpes	-0.305 (0.141)	-	-0.547 (0.270)	-	-	-	-
Poitou Charentes	0.654 (0.142)	-0.313 (0.238)	-1.074 (0.456)	-	-	-	-
Limousin	-1.190 (0.358)	-0.425 (0.288)	-	-	0.446 (0.287)	-	-0.579 (0.459)
Languedoc	-0.315 (0.216)	-0.757 (0.273)	-	-	0.575 (0.241)	-	-
Ile de France	-	0.135 (0.129)	-0.767 (0.329)	-	-	-	-0.541 (0.310)
Centre	-	0.682 (0.149)	-	-	-	-	-
Haute Normandie	-	0.337 (0.182)	-	0.821 (0.322)	-	-	-
Midi Pyrénées	-	-0.321 (0.181)	-	0.759 (0.275)	-	-	-
Franche Comté	-	-	1.244 (0.269)	-	-	-	-
Provence	-	-	-1.176 (0.507)	-0.864 (0.588)	-0.872 (0.415)	-	-
Corse	-	-	-	-	0.856 (0.509)	-	-

Remarks: between parentheses are the standard errors; (-) if not included

Table 3 (continued): Estimates of the parameters of the unemployment duration model

	LTC jobs	FTC	CJ	CPWL	Other Program	Appren- ticeship	OLF
Previous state:							
OLF	Ref.	Ref.	Ref.	Ref.	Ref.	-	Ref.
Temp. Job (≤3)	-	0.645 (0.170)	-0.909 (0.509)	-	-	-	-0.510 (0.388)
Temp. Job (>3)	-	0.551 (0.244)	-1.075 (1.003)	-	-0.865 (0.713)	-	-
App contract	0.350 (0.180)	0.452 (0.182)	-0.390 (0.302)	-	-	-	-
Program (≤3)	-	-	-	0.601 (0.292)	0.498 (0.290)	-	-0.455 (0.386)
Program (>6)	-	-	-0.510 (0.328)	-	0.379 (0.291)	-	-
FTC job (≤3)	0.215 (0.104)	0.399 (0.100)	-0.590 (0.211)	-	-0.214 (0.173)	-	-0.538 (0.191)
FTC job (3-6)	-	0.533 (0.140)	-0.835 (0.344)	-	-	-	-0.717 (0.290)
FTC job (7-12)	-	0.407 (0.153)	-0.648 (0.336)	-1.091 (0.717)	-0.624 (0.343)	-	-
LTC job (≤6)	0.766 (0.178)	-	-0.756 (0.583)	0.534 (0.389)	-	-	-
LTC job (7-12)	0.393 (0.167)	-	-0.529 (0.458)	-	-	-	-0.621 (0.365)
LTC job (13-24)	0.149 (0.147)	-	-1.063 (0.461)	-0.404 (0.392)	-0.587 (0.290)	-	-0.550 (0.275)
LTC job (>24)	0.145 (0.125)	-	-1.946 (0.589)	-	-	-	-0.513 (0.216)

Remarks: the standard errors are given besides the estimates and between parentheses; (-) if not included. The previous state is the state just before the first observed unemployment spell; OLF means “out-of-the-labor-force”, Temp. job means “temporary job”, App. Contract means “apprenticeship contract”; between parentheses we indicate the duration of the previous state spell, which is in months.

Several points have to be emphasized. For each pair of programs (treatments) to be compared, it appears that the common supports of the balancing scores are wide enough, and these common supports differ between pairs of treatments. Moreover, for some pairs, the shapes of the balancing score distributions significantly differ. For example, when comparing the relative probabilities of entering a fixed-term contract (FTC) job for individuals who have effectively accepted an FTC job and a community job (see the first graph in Figure 2), we observe that the distribution of the balancing score is more concentrated in the higher part of the support for individuals who have entered an FTC job, while it is more concentrated in the middle for young people who entered a community job. A similar pattern appears when comparing FTC jobs and "courses for preparation to the working life", or "community jobs" and "other programs" (see Figure 2). Here is a potential source of selectivity bias for the naive estimator and a challenging situation for the matching estimator. Under the conditional independence assumption, Figure 2 provides a graphical representation of the upper bound of the naive estimator bias. Indeed this bias is equal to:

$$\begin{aligned}
& E\left(Y_{k,i} \mid T_i = k\right) - E\left(Y_{k,i} \mid T_i = k'\right) \\
&= E\Big(E\left(Y_{k,i} \mid \Pi^{k/k'}(X_i), T_i = k\right) \Big| T_i = k \Big) \\
&\qquad - E\Big(E\left(Y_{k,i} \mid \Pi^{k/k'}(X_i), T_i = k'\right) \Big| T_i = k' \Big) \\
&= E\Big(E\left(Y_{k,i} \mid \Pi^{k/k'}(X_i)\right) \Big| T_i = k \Big) \qquad\qquad (6) \\
&\qquad - E\Big(E\left(Y_{k,i} \mid \Pi^{k/k'}(X_i)\right) \Big| T_i = k' \Big) \\
&= \int_{Support(X_i)} E\left(Y_{k,i} \mid \Pi^{k/k'}(X_i)\right) \\
&\qquad \times \left[f\left(\Pi^{k/k'}(X_i) \mid T_i = k\right) - f\left(\Pi^{k/k'}(X_i) \mid T_i = k'\right) \right] dX_i
\end{aligned}$$

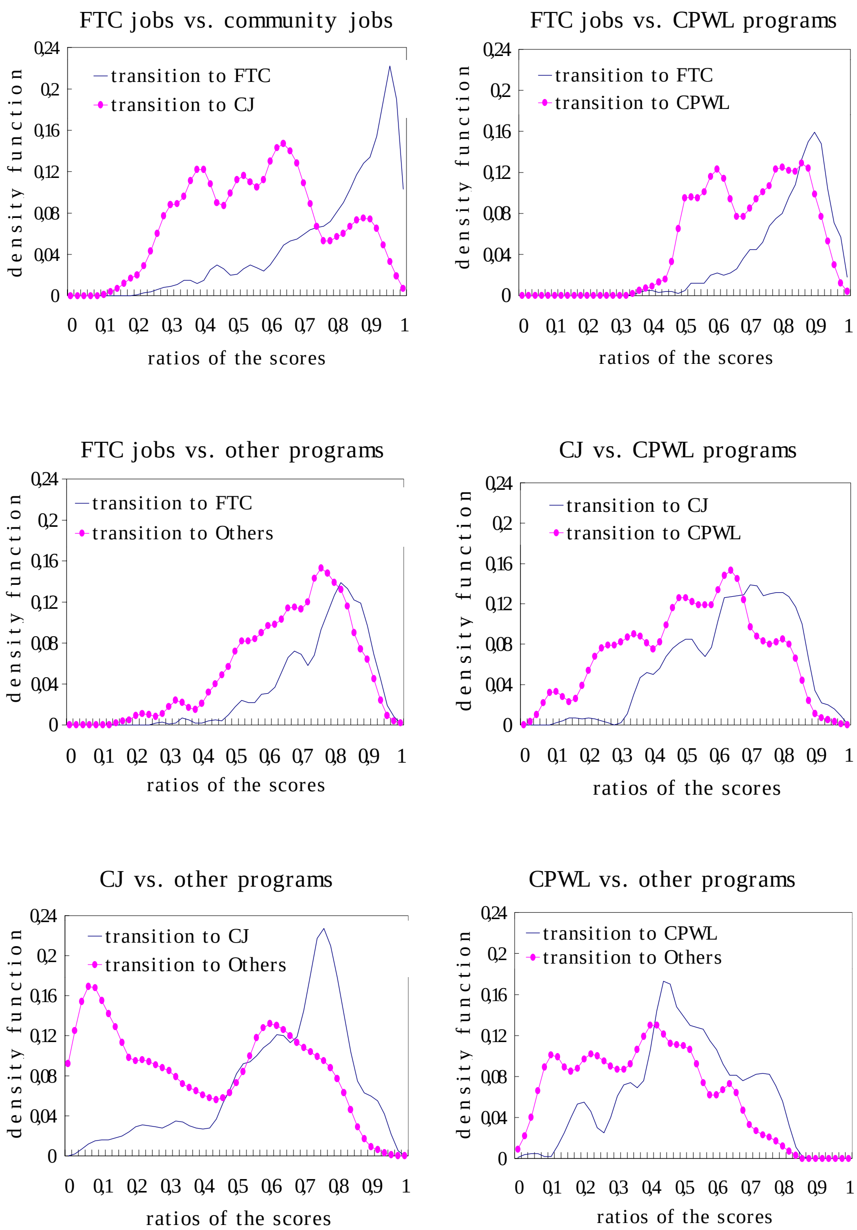
From that equality, we deduce that

$$\begin{aligned}
\left|Bias\right| \le {} & \underset{X_i}{Max}\, E\left(Y_{k,i} \mid \Pi^{k/k'}(X_i)\right) \\
& \times \int_{Support(X_i)} \left| f\left(\Pi^{k/k'}(X_i) \mid T_i = k\right) - f\left(\Pi^{k/k'}(X_i) \mid T_i = k'\right) \right| .dX_i
\end{aligned}$$

When $Y_{k,i}$ is a dummy variable with possible values 0 or 1, then we have :

$$\left|Bias\right| \le \int_{Support(X_i)} \left| f\left(\Pi^{k/k'}(X_i) \mid T_i = k\right) - f\left(\Pi^{k/k'}(X_i) \mid T_i = k'\right) \right| .dX_i$$

Figure 2: Nonparametric estimates of the density functions of the balancing scores for various pairs of treatments



Thus, the absolute value of the bias associated with the naive estimator is bounded by the surface lying between the two distributions shown in each window of Figure 2.

6 Matching Estimates

6.1 The Response Variables and the Matching Algorithm

For evaluating the impact of training programs, we use various response variables. The first one is a dummy variable representing the state occupied by the individual just after the treatment. In our application to French data, this variable has two alternative definitions:

- it is set equal to 1 if this state is an LTC job or an FTC job, 0 otherwise,
- alternatively, it is equal to 1 if this state is an LTC job only, 0 otherwise.

We also consider the same variables 3 months and 6 months after the end of the treatment, which enables us to consider temporal effects. The two others response variables are count data:

- the total number of months spent in LTC jobs during the 6 months following the end of the treatment,
- the total number of months spent in LTC or FTC jobs over the same period.[13]

Obviously, for different individuals, the program may start and end at different points in time. Thus the post-training calendar period for individuals in treatment *l* is generally different from the post-training calendar period for individuals in treatment *m*. Neglecting this point means that we do not take into account possible different labor market environments for the treatments *l* and *m*. But this shortcoming is mitigated by the relatively short time interval over which observations are made.

Investigations will be conducted on the full common supports of the ratios of propensity scores, but also on their lower and higher parts to point out potential score effects.

---

[13] Considering the whole transition process after the end of the program as an outcome vector is a much more difficult task.

To estimate the average conditional effect of treatment k with respect to treatment k given that individual i is assigned to treatment k, we use a kernel matching estimator such as the ones studied by Heckman, Ichimura, Smith and Todd (1998). Remember that the counterfactual parameter of interest is

$$E\left(Y_{k',i} \middle| T_i = k\right) = E\left(E\left(Y_{k',i} \middle| \Pi^{k/k'}(X_i), T_i = k'\right) \middle| T_i = k\right)$$

First the inner expectation is estimated from a Nadaraya-Watson kernel regression as

$$y_{k',i}(k) = \frac{\sum_{j=1}^{N} y_{k',j}(k) \times 1(T_j = k') \times K\left(\frac{\Pi^{k/k'}(X_i) - \Pi^{k/k'}(X_j)}{h_{N_{k'}}}\right)}{\sum_{j=1}^{N} 1(T_j = k') \times K\left(\frac{\Pi^{k/k'}(X_i) - \Pi^{k/k'}(X_j)}{h_{N_{k'}}}\right)} \quad (7)$$

where $\Pi^{k/k'}(X_j)$ is the balancing score for a covariate vector X_j, $K(.)$ is a kernel function,[14] and $h_{N_{k'}}$ is the "rule-of-thumb" bandwith parameter calculated on the support of the ratio $\Pi^{k/k'}$ for the individuals assigned to treatment k'. Then the outer expectation $E(Y_{k',i} \mid T_i=k)$ is computed as the sample average over the participants in treatment k. We also calculate the naive estimator (the simple mean difference) in order to detect the presence of a selectivity bias in our data.

6.2 Results

6.2.1 Relative Effects of the Programs

Tables 4a and 5 present the estimates obtained with the kernel matching procedure for the different response variables we have considered. Those results are given for the whole common support and have to be read as follows. For example, consider the first row and the first column in Table 4a, that is the probability gain to be in an LTC job or an FTC job just after the treatment; for a person who was previously in a community job (CJ), the average gain from not having participated in a CPWL program is estimated as 0.014 (s.e. 0.057). The reading is the same for all the remaining tables. Tables 4a and 5 help us to compare the relative effectiveness of the various programs.

When the output variable is the probability to be employed in an LTC job or in an FTC job (Table 4a), there is a positive effect of CJ and CPWL programs vs. “other programs” just after and 3 months after the program, but these effects clearly

[14] In our application, K is chosen to be the quartic kernel function.

disappear six months after. The CPWL program is the most effective program when compared with FTC jobs, since there is no significant negative effect six months after for people who effectively participated in a CPWL program, while such effects exist when FTC jobs are compared with CJ or "other programs":

$$E\left(Y_{CJ} - Y_{FTC} \mid T = CJ\right) = -0.136 \quad (se.\ 0.049),$$

$$E\left(Y_{Others} - Y_{FTC} \mid T = Others\right) = -0.143 \quad (se.\ 0.041)$$

but

$$E\left(Y_{CPWL} - Y_{FTC} \mid T = CPWL\right) = -0.078 \quad (se.\ 0.049).$$

There is an asymmetry between $E(Y_k - Y_{k'} \mid T=k)$ and $E(Y_{k'} - Y_k \mid T=k')$ when comparing FTC jobs and CPWL programs: one is significant while the other is not. For people who were effectively employed in an FTC job, the benefit from being hired in an FTC job rather than participating in a CPWL program is positive; at the opposite, for people who effectively participated in the CPWL program, there is no significant loss from not having found an FTC job. There is no such asymmetry for community jobs and "other programs" (the loss from not having found an FTC job is significantly negative).

When the output variable is the probability to be employed in an LTC job, i.e. in a stable employment state, there are no significant differences between programs (see Table 4a). But employment in an FTC job is still more effective than all types of programs, whatever the date is. However, it must be noticed that these effects are stable through time after a CJ job, but are clearly decreasing after a CPWL program or after an "other program". When comparing programs with FTC jobs, we find that "other programs" display the lowest loss six months after the end of the "treatment".

When the output variable is the time spent in the two employment states over the six months period after the program (Table 5, columns S), we find that there are no significant differences between the programs. However, employment in an FTC job is associated with significant effects which vary from 0.5 to 0.9: this corresponds to a gain (or a loss for program participants) varying from 2 weeks to one month in employment.

To summarize these first results, we can say that an FTC job is more effective than the employment programs. Among these programs, the most effective one seems to be the CPWL program; the less effective is the CJ program, especially when the output variable is employment in an LTC job or an FTC job. Thus, on-the-job training programs in the private sector (associated with higher amounts of vocational and specific training) give better results than the programs in the public sector. It is also interesting to notice that significant differences between programs appear when the output variable is the probability to be employed in an LTC job or an FTC job, but none is significant when the output variable is the probability to be employed in an LTC job. This result shows that there exists a gap between stable and unstable employment states, and that employment programs are not designed to increase the probability of finding an LTC job but simply to increase the probability of leaving unemployment. Finally, the gain associated with non participation in a program for people who are hired in an FTC job is generally higher (in absolute value) than the loss of not getting an FTC job for people who participate in a program.

To test for the robustness of our results, and to follow the suggestion of a referee, we have considered another outcome variable, namely the probability to be employed 6, 12 and 16 months after the beginning of the treatment (given the treatment has ended). Results are reported in Table 4b, Subsection 8.4. They show that the CPWL program is more effective than the CJ program if its duration is greater than 12 months, and more effective than an "other program" if its duration is less than 6 months. Training programs in public centers ("other programs") are more effective than the CJ programs when their durations are greater than one year. Finally, FTC jobs are more effective than the programs in the public sector (CJ and "other programs"): their mean outcome is not statistically different from the one associated with CPWL programs. So, on the whole, these results confirm the ones obtained when the outcome variable is the probability to be employed 1, 3 or 6 months after the end of the program.

6.2.2 Selection Bias

Comparisons between the naive and kernel matching estimates show the presence of some selectivity bias in our data. As it is suggested by the conditional propensity scores distributions in Figure 2, a selectivity bias is present when the score distributions estimated for two subgroups of individuals (for example, the ones who participated in a CPWL program and the ones who were employed in a community job) exhibit significant differences. For instance, consider the output variable "probability to be employed in an LTC job or in an FTC job six months after the treatment" (see Table 4c, Subsection 8.4). When estimating the difference between the conditional probability to be employed six months after the end of the treatment for people who participated in a community job (CJ) and the conditional probability would they have been employed in an FTC job (row 3 in Table 4c), the naive estimator gives -0.224 (s.e. 0.042) whereas the kernel matching estimator gives -0.136 (s.e. 0.049), that is half the first one. Another example is the comparison between the CPWL program and an FTC job given a participation in the CPWL program for the same output variable (row 6 in Table 4c). Whatever the date is (just after the treatment, three or six months after the treatment), the naive estimator gives a significantly negative effect whose absolute value increases over time, whereas the kernel matching estimator shows that the difference is not statistically significant.

Table 4a: Kernel matching estimates of the mean differences for two different outputs: employment in a LTC job or in an FTC job, employment in a LTC job

		Just after the Program		3 months after		6 months after	
		LTC+FTC	LTC	LTC+FTC	LTC	LTC+FTC	LTC
Reference: CJ	CPWL	0.014	0.009	0.011	-0.012	-0.038	-0.006
		(0.057)	(0.049)	(0.057)	(0.050)	(0.058)	(0.051)
	OTHER	0.103**	0.046	0.114**	0.012	-0.011	-0.029
		(0.051)	(0.045)	(0.057)	(0.045)	(0.056)	(0.051)
	FTC	0.004	-0.116**	-0.017	-0.088**	-0.136**	-0.111**
		(0.044)	(0.045)	(0.049)	(0.041)	(0.049)	(0.045)
Reference: CPWL	CJ	-0.010	-0.001	0.009	0.025	0.051	0.016
		(0.050)	(0.045)	(0.062)	(0.047)	(0.051)	(0.045)
	OTHER	0.091**	0.038	0.089*	0.016	0.052	-0.029
		(0.047)	(0.036)	(0.051)	(0.044)	(0.055)	(0.044)
	FTC	-0.022	-0.117**	-0.053	-0.087**	-0.078	-0.102**
		(0.041)	(0.041)	(0.052)	(0.043)	(0.049)	(0.040)
Reference: OTHER	CJ	-0.080*	-0.011	-0.059	0.031	0.055	0.047
		(0.049)	(0.042)	(0.052)	(0.044)	(0.049)	(0.044)
	CPWL	-0.059	-0.012	-0.066	-0.005	-0.059	0.006
		(0.057)	(0.041)	(0.051)	(0.046)	(0.061)	(0.053)
	FTC	-0.092**	-0.168**	-0.131**	-0.112**	-0.143**	-0.094**
		(0.038)	(0.032)	(0.038)	(0.039)	(0.041)	(0.038)
Reference: FTC	CJ	-0.014	0.158**	0.056	0.129**	0.228**	0.164**
		(0.056)	(0.044)	(0.059)	(0.043)	(0.053)	(0.044)
	CPWL	0.036	0.163**	0.074	0.103**	0.143**	0.119**
		(0.051)	(0.046)	(0.050)	(0.043)	(0.054)	(0.048)
	OTHER	0.096**	0.172**	0.122**	0.091**	0.145**	0.080**
		(0.038)	(0.035)	(0.039)	(0.035)	(0.044)	(0.041)

Remarks: * means that the estimated mean difference is significant at the 10% level and ** that it is significant at the 5%. Between parentheses we report the bootstrapped standard errors

Table 5: Kernel matching estimates of the mean differences on the whole support, on the higher part and on the lower part of the support (output: number of months in employment)

	State:	LTC + FTC			LTC only		
		S	S_-	S^+	S	S_-	S^+
Reference: CJ	CPWL	-0.029	-0.077	-0.006	-0.031	0.006	-0.049
		(0.270)	(0.412)	(0.360)	(0.256)	(0.352)	(0.339)
	OTHER	0.370	-0.352	0.570**	0.022	-0.707**	0.224
		(0.276)	(0.393)	(0.314)	(0.233)	(0.298)	(0.289)
	FTC	-0.405	-0.531*	-0.272	-0.644**	-0.876**	-0.401
		(0.252)	(0.288)	(0.386)	(0.230)	(0.264)	(0.351)
Reference: CPWL	CJ	0.090	0.241	-0.069	0.093	0.194	-0.015
		(0.282)	(0.396)	(0.395)	(0.223)	(0.320)	(0.356)
	OTHER	0.412	0.228	0.620*	0.008	-0.082	0.110
		(0.261)	(0.325)	(0.365)	(0.220)	(0.276)	(0.359)
	FTC	-0.457*	-0.676**	-0.121	-0.643**	-0.845**	-0.330
		(0.253)	(0.306)	(0.446)	(0.226)	(0.240)	(0.351)
Reference: OTHER	CJ	-0.204	-0.584**	0.325	0.155	-0.226	0.685**
		(0.253)	(0.297)	(0.502)	(0.190)	(0.274)	(0.317)
	CPWL	-0.390	-0.636	-0.275	-0.023	-0.162	0.042
		(0.281)	(0.397)	(0.334)	(0.223)	(0.399)	(0.299)
	FTC	-0.831**	-0.795**	-0.876**	-0.758**	-0.665**	-0.875**
		(0.214)	(0.238)	(0.320)	(0.188)	(0.227)	(0.305)
Reference: FTC	CJ	0.596**	0.103	0.693**	0.932**	0.279	1.061**
		(0.291)	(0.384)	(0.322)	(0.213)	(0.375)	(0.255)
	CPWL	0.666**	0.298	0.728**	0.804**	0.603**	0.837**
		(0.265)	(0.383)	(0.299)	(0.226)	(0.365)	(0.264)
	OTHER	0.826**	0.843**	0.819**	0.673**	0.776**	0.635**
		(0.189)	(0.298)	(0.275)	(0.188)	(0.303)	(0.255)

Remarks: * means that the estimated mean difference is significant at the 10% level and ** that it is significant at the 5%. Between parentheses we report the bootstrapped standard errors; S denotes the whole support; S^+ (respectively, S_-) denotes the higher (respectively, the lower) part of the common support.

6.2.3 Heterogeneity Across Participants in a Program

We have previously estimated the effects of the different treatments by averaging over all conditional probabilities lying in the common support of each pair of treatments. It is also interesting to study these effects on subintervals of the common support, that is for particular values of the conditional probabilities. This exercise allows us to emphasize the variability of the effects of a program for recipients who have very different conditional probabilities to participate.

We estimate the average effects over two subintervals, namely the lower and higher parts of the common supports of conditional propnsity scores. For each pair of treatments, we divide the common S=[S_- ,S^+] into two equal intervals around the value (S_- + S^+)/2. Thus, comparisons are conducted for subpopulations that have not necessarily the same size, and, as a consequence, results on the whole common supports cannot be considered as the simple avereage of the results on the two subintervals we have constructed.

Such comparisons produce results that lead us to revise our first classification of the programs (see Tables 5-7). Except for “other programs”, comparisons between various treatments show that positive effects on the whole common support are usually associated with significant positive effects on the higher part of the support and no significant effect on the lower part; at the opposite, negative effects on the whole common support are usually associated with significant negative effects on the lower part of the support and no significant effect on the higher part. Positive effects on the higher part of the support suggest that the highest effectiveness is obtained for individuals who have the highest conditional probability to participate; for example, the positive effects of FTC jobs vs. CPWL and CJ programs are obtained for people who have a higher probability to be employed in an FTC job and who are effectively hired in an FTC job. Negative effects on the lower part of the support suggest that costs of misallocation are paid by people who have the lowest probability to enter the treatment they have effectively received. That is the case when we compare CPWL and CJ programs vs. FTC jobs for individuals who participated in CPWL or CJ programs but who had a lower conditional probability to do so (notice that people with a higher probability to enter treatment *k* conditionally on treatments *k* or *k’* are those who have a lower probability to enter treatment *k’* conditionally on treatments *k* or *k’*. Thus there is a cost of misallocation. Moreover, our results give a partial idea of what could be a way of improving such an allocation, which is a question of special interest for policy recommendations. For example, when the output variable we consider is the probability to be in a stable (LTC) job six months after the treatment, the loss from not having participated in other programs for people who participated in a CJ program and who had a lower conditional probability to do so is -0.156 (s.e. 0.064), whereas for people who had a higher probability to do so, there is neither loss or gain because the estimate difference is 0.007 (s.e. 0.054). Thus, one way to improve the allocation could be to offer “other programs" than CJ programs to people whose observable characteristics are associated with a lower conditional probability of getting a CJ program. Finally, it should be noticed that due to the fact that our results are pairwise comparisons, different improvements may be proposed to the same person.

As we noticed above, "other programs" seem to be an exception, especially when compared to FTC jobs. Surprisingly, for that pair of treatments, positive effects on the whole common support are associated with positive effects on the lower part of the support, whereas negative effects on the whole support are associated with negative effects on the higher part of the support. More precisely, for people who have a high conditional probability to participate in other programs,

$$E\left(Y_{Others} - Y_{FTC} \,\middle|\, T = Others, \Pi^{Others/FTC} > (S_{-} + S^{+})/2\right) = -0.14 \text{ (se. 0.061)}$$

whereas for people who have a low probability to participate in other programs,

$$E\left(Y_{Others} - Y_{FTC} \,\middle|\, T = Others, \Pi^{Others/FTC} < (S_{-} + S^{+})/2\right) = -0.058 \text{ (se. 0.052)}$$

where Y is 1 if the individual is employed in an LTC job six months after the treatment, 0 otherwise. Thus, "other programs" could have also a negative signalling effect with respect to FTC jobs. Moreover, this effect is revealed from the first date (just after the treatment), and seems to be constant through time.

7 Conclusions

In this paper we have applied the statistical framework developed by Imbens (1999) and Lechner (1999) to identify and to estimate the causal effects of multiple treatments under the conditional independence assumption. In particular, we have shown that, under this assumption, matching with respect to the ratio of the scores $\Pr(T=k|X)$ and $\Pr(T=k'|X)$ allows to estimate nonparametrically the average conditional treatment effect $E(Y_k - Y_{k'} \mid T=k)$ for a pair of treatments k and $k' \neq k$. In our application we have considered youth employment programs which were set up in France during the eighties to improve the labor market prospects of the most disadvantaged and unskilled young workers. Using data from INSEE previously analyzed by Bonnal, Fougère and Sérandon (1997), we have re-examined the impact of these programs on the subsequent employment status by implementing matching estimators introduced by Heckman, Ichimura, Smith and Todd (1998) and Heckman, Ichimura and Todd (1998).

Due to the fact that our sample is extracted from the stock of unemployed people at a given date (August 1986), we derived the propensity scores from a competing-risks duration model. This specification allowed us to take rigorously into account the potential endogenous effect of the unemployment duration on the process of

Table 6: Kernel matching estimates on the higher and lower parts of the support (output: employment in an LTC or an FTC job)

		Just after the program		3 months after		6 months after	
		S_-	S^+	S_-	S^+	S_-	S^+
Reference: CJ	CPWL	-0.011	0.026	-0.031	0.031	-0.085	-0.015
		(0.065)	(0.072)	(0.086)	(0.066)	(0.071)	(0.078)
	OTHER	0.002	0.131**	-0.078	0.167**	-0.215**	0.045
		(0.098)	(0.054)	(0.091)	(0.057)	(0.082)	(0.059)
	FTC	0.030	-0.023	-0.037	0.003	-0.185**	-0.085
		(0.057)	(0.067)	(0.056)	(0.072)	(0.059)	(0.069)
Reference: CPWL	CJ	-0.003	-0.017	0.017	0.001	0.048	0.054
		(0.074)	(0.076)	(0.075)	(0.084)	(0.077)	(0.072)
	OTHER	0.066	0.120*	0.076	0.103	0.014	0.096
		(0.055)	(0.066)	(0.067)	(0.083)	(0.063)	(0.082)
	FTC	-0.037	0.001	-0.073	-0.021	-0.145**	0.025
		(0.054)	(0.076)	(0.060)	(0.078)	(0.058)	(0.072)
Reference: OTHER	CJ	-0.137**	-0.000	-0.175**	0.102	-0.031	0.175**
		(0.054)	(0.099)	(0.056)	(0.101)	(0.059)	(0.082)
	CPWL	-0.088	-0.046	-0.112	-0.046	-0.098	-0.041
		(0.077)	(0.064)	(0.080)	(0.069)	(0.085)	(0.068)
	FTC	-0.120**	-0.055	-0.123**	-0.142**	-0.131**	-0.160**
		(0.051)	(0.059)	(0.047)	(0.056)	(0.049)	(0.069)
Reference: FTC	CJ	0.014	-0.020	-0.057	0.078	0.038	0.265**
		(0.072)	(0.062)	(0.074)	(0.068)	(0.080)	(0.066)
	CPWL	0.025	0.038	0.010	0.084	0.001	0.167**
		(0.071)	(0.055)	(0.064)	(0.052)	(0.075)	(0.057)
	OTHER	0.026	0.122**	0.114*	0.125**	0.183**	0.131**
		(0.059)	(0.049)	(0.060)	(0.053)	(0.059)	(0.051)

Remarks: * means that the estimated mean difference is significant at the 10% level and ** that it is significant at the 5%. Between parentheses we report the bootstrapped standard errors; S^+ (respectively, S_-) denotes the higher (respectively, the lower) part of the common support.

Table 7: Kernel matching estimates on the higher and lower parts of the support (output: employment in an LTC job)

		Just after the program		3 months after		6 months after	
		S_-	S^+	S_-	S^+	S_-	S^+
Reference: CJ	CPWL	0.002	0.012	-0.001	-0.017	-0.017	-0.001
		(0.072)	(0.062)	(0.073)	(0.065)	(0.065)	(0.061)
	OTHER	-0.092	0.084	-0.143**	0.055	-0.156**	0.007
		(0.065)	(0.054)	(0.068)	(0.056)	(0.064)	(0.054)
	FTC	-0.123**	-0.109	-0.130**	-0.044	-0.130**	-0.090
		(0.052)	(0.070)	(0.050)	(0.065)	(0.054)	(0.068)
Reference: CPWL	CJ	0.001	-0.004	0.040	0.009	0.029	0.002
		(0.061)	(0.060)	(0.067)	(0.062)	(0.060)	(0.062)
	OTHER	0.005	0.075	0.038	-0.010	-0.045	-0.012
		(0.051)	(0.066)	(0.049)	(0.076)	(0.056)	(0.071)
	FTC	-0.173**	-0.031	-0.105**	-0.060	-0.123**	-0.070
		(0.045)	(0.076)	(0.048)	(0.074)	(0.046)	(0.064)
Reference: OTHER	CJ	-0.086*	0.094	-0.043	0.133**	-0.005	0.121*
		(0.050)	(0.061)	(0.049)	(0.072)	(0.056)	(0.072)
	CPWL	-0.058	0.010	0.013	-0.013	-0.020	0.018
		(0.061)	(0.053)	(0.074)	(0.063)	(0.080)	(0.069)
	FTC	-0.197**	-0.131**	-0.110**	-0.115**	-0.058	-0.140**
		(0.041)	(0.057)	(0.045)	(0.059)	(0.052)	(0.061)
Reference: FTC	CJ	0.112	0.167**	0.023	0.150**	0.053	0.186**
		(0.070)	(0.053)	(0.065)	(0.053)	(0.066)	(0.052)
	CPWL	0.088	0.176**	0.060	0.111**	0.104	0.121**
		(0.075)	(0.049)	(0.068)	(0.052)	(0.065)	(0.049)
	OTHER	0.106**	0.196**	0.088*	0.092**	0.149**	0.055
		(0.052)	(0.044)	(0.050)	(0.045)	(0.055)	(0.048)

Remarks: * means that the estimated mean difference is significant at the 10% level and ** that it is significant at the 5%. Between parentheses we report the bootstrapped standard errors; S^+ (respectively, S_-) denotes the higher (respectively, the lower) part of the common support.

assignment to treatments. The nonparametric kernel estimates of the distributions of the balancing scores $\Pi^{k/k'}(X_i)$ show that, for each pair of programs (treatments) to be compared, the common supports of the ratios are wide enough. Moreover, these common supports differ between pairs of treatments.

The kernel matching estimates of the mean output differences show the variability of program effects, both between programs and among recipients of the same program. For instance, if the output variable is the probability to be employed in an LTC job, i.e. in a stable employment state, or the time spent in each of the employment states over the six months period after the program, there are no significant differences between programs. On the whole, it appears that an FTC job is more effective than the employment programs. Among these programs, the most effective one seems to be the CPWL program; the least effective is the CJ program, especially when the output variable is employment in an LTC job or an FTC job. Thus, on-the-job training programs in the private sector (associated with higher amounts of vocational and specific training) give better results than the programs in the public sector. This general result confirms the conclusions of the paper written by Bonnal, Fougère and Sérandon (1997), which were deduced from a very different approach.

But our paper contains further results. We have also studied the relative effects of the different programs on subintervals of the common support, that is for particular values of the conditional probabilities. This exercise allowed us to emphasize the variability of the effects of a program for recipients who have very different conditional probabilities to participate. We found that, in general, comparisons between various treatments show that positive effects on the whole common support are usually associated with significant positive effects on the highest part of the support and no significant effect on the lower part; at the opposite, negative effects on the whole common support are usually associated with significant negative effects on the lower part of the support and no significant effect on the highest part. Positive effects on the higher part of the support suggest that the highest effectiveness is obtained for individuals who have the highest conditional probability to participate; for example, the positive effects of FTC jobs vs. CPWL and CJ programs are obtained for people who have a higher probability to be employed in an FTC job and who are effectively hired in an FTC job. Negative effects on the lower part of the support suggest that costs of misallocation are paid by people who have the lower probability to enter the treatment they have effectively received. That is the case when we compare CPWL and CJ programs vs. FTC jobs for individuals who participated in CPWL or CJ programs but who had a lower conditional probability to do so. Thus our results give an idea of what could be a way of improving the assignment of applicants through treatments.

Finally, let us remark that the comparison of the participants in programs with those unemployed who did not receive any program in a comparable time period would be of obvious interest. This is made possible by the competing-risks duration model, because it permits directly to estimate the probability to move from unemployment to a given treatment over a given subperiod of the unemployment spell. Such comparisons will be conducted in a further research.

8 Appendix

8.1 Proof of Proposition 1

First let us recall the following usual relation

$$\Pr\left(T_i = k \mid X_i, T_i \in \{k, k'\}\right) = \frac{\Pr\left(T_i = k \mid X_i\right)}{\Pr\left(T_i \in \{k, k'\} \mid X_i\right)}$$

$$= \frac{\Pi^k(X_i)}{\Pi^k(X_i) + \Pi^{k'}(X_i)}$$

$$= \Pi^{k/k'}(X_i)$$

Similarly we have

$$\Pr\left(T_i = k \mid X_i, Y_{k,i}, Y_{k',i}, T_i \in \{k, k'\}\right)$$

$$= \frac{\Pr\left(T_i = k \mid X_i, Y_{k,i}, Y_{k',i}\right)}{\Pr\left(T_i = k \mid X_i, Y_{k,i}, Y_{k',i}\right) + \Pr\left(T_i = k' \mid X_i, Y_{k,i}, Y_{k',i}\right)}$$

$$= \Pi^{k/k'}(X_i)$$

The last equality derives directly from the conditional independence assumption:

$$\Pr\left(T_i = k \mid X_i, Y_{k,i}, Y_{k',i}\right) = \Pr\left(T_i = k \mid X_i\right).$$

Then

$$
\begin{aligned}
&\Pr\left(T_i = k \,\middle|\, \Pi^{k/k'}(X_i), Y_{k,i}, Y_{k',i}, T_i \in \{k, k'\}\right) \\
&= E\left(1(T_i = k) \,\middle|\, \Pi^{k/k'}(X_i), Y_{k,i}, Y_{k',i}, T_i \in \{k, k'\}\right) \\
&= E\left(E\left(1(T_i = k) \,\middle|\, X_i, Y_{k,i}, Y_{k',i}, T_i \in \{k, k'\}\right) \,\middle|\, \Pi^{k/k'}(X_i), Y_{k,i}, Y_{k',i}, T_i \in \{k, k'\}\right) \\
&= E\left(\Pr\left(1(T_i = k) \,\middle|\, X_i, Y_{k,i}, Y_{k',i}, T_i \in \{k, k'\}\right) \,\middle|\, \Pi^{k/k'}(X_i), Y_{k,i}, Y_{k',i}, T_i \in \{k, k'\}\right) \\
&= E\left(\Pi^{k/k'}(X_i) \,\middle|\, \Pi^{k/k'}(X_i), Y_{k,i}, Y_{k',i}, T_i \in \{k, k'\}\right) \\
&= \Pi^{k/k'}(X_i)
\end{aligned}
$$

Similarly we have

$$\Pr\left(T_i = k \,\middle|\, \Pi^{k/k'}(X_i), T_i \in \{k, k'\}\right) = \Pi^{k/k'}(X_i)$$

Thus

$$\Pr\left(T_i = k \,\middle|\, \Pi^{k/k'}(X_i), Y_{k,i}, Y_{k',i}, T_i \in \{k, k'\}\right) = \Pr\left(T_i = k \,\middle|\, \Pi^{k/k'}(X_i), T_i \in \{k, k'\}\right)$$

from which it follows that:

$$(Y_{k,i}, Y_{k',i}) \perp T_i \,\middle|\, \Pi^{k/k'}(X_i), T_i \in \{k, k'\} \quad \blacklozenge$$

8.2 Proof of Proposition 2

Using the independence assumption, we get

$$
\begin{aligned}
E\left(Y_{k',i} \mid T_i = k\right) &= E\left(E\left(Y_{k',i} \mid X_i, T_i = k\right) \mid T_i = k\right) \\
&= E\left(E\left(Y_{k',i} \mid X_i\right) \mid T_i = k\right)
\end{aligned}
$$

Considering the decomposition of the joint density h of the covariates and the treatments

$$h(X_i, T_i) = f_1(X_i \mid T_i) \times g_2(T_i) = f_2(X_i) \times g_1(T_i \mid X_i)$$

we obtain

$$\frac{f_1(X_i \mid T_i) \times g_2(T_i)}{g_1(T_i \mid X_i)} = f_2(X_i)$$

which implies

$$f_1(X_i \mid T_i = k) = f_1(X_i \mid T_i = k') \times \frac{\Pi^k(X_i) \times \Pr(T_i = k')}{\Pi^{k'}(X_i) \times \Pr(T_i = k)}$$

Thus

$$\begin{aligned}
E(Y_{k',i} \mid T_i = k) &= E\left(E(Y_{k',i} \mid X_i) \times \frac{\Pi^k(X_i) \times \Pr(T_i = k')}{\Pi^{k'}(X_i) \times \Pr(T_i = k)} \mid T_i = k' \right) \\
&= E\left(E\left(Y_{k',i} \times \frac{\Pi^k(X_i) \times \Pr(T_i = k')}{\Pi^{k'}(X_i) \times \Pr(T_i = k)} \mid X_i \right) \mid T_i = k' \right) \\
&= E\left(Y_i \times \frac{\Pi^k(X_i) \times \Pr(T_i = k')}{\Pi^{k'}(X_i) \times \Pr(T_i = k)} \mid T_i = k' \right) \quad \blacklozenge
\end{aligned}$$

8.3 Proof of Proposition 3

$$\begin{aligned}
&E(Y_{k,i} - Y_{k',i} \mid T_i = k) \\
&= E(Y_{k,i} - Y_{k',i} \mid T_i = k, T_i \in \{k, k'\}) \\
&= E\left(Y_{k,i} - E\left(Y_{k',i} \mid \Pi^{k/k'}(X_i), T_i = k, T_i \in \{k, k'\}\right) \mid T_i = k, T_i \in \{k, k'\}\right) \\
&= E\left(Y_{k,i} - E\left(Y_{k',i} \mid \Pi^{k/k'}(X_i), T_i = k', T_i \in \{k, k'\}\right) \mid T_i = k, T_i \in \{k, k'\}\right) \\
&= E\left(Y_{k,i} - E\left(Y_{k',i} \mid \Pi^{k/k'}(X_i), T_i = k'\right) \mid T_i = k\right) \quad \blacklozenge
\end{aligned}$$

8.4 Tables

Table 4b: Kernel matching estimates of the mean differences for two different outputs: employment in a LTC job or in an FTC job, employment in a LTC job; 6 months, 12 months and 16 months after the beginning of the program.

		6 months after		12 months after		16 months after	
		LTC+FTC	LTC	LTC+FTC	LTC	LTC+FTC	LTC
Reference: CJ	CPWL	-0.032	-0.042	-0.129	-0.061	-0.225**	-0.175
		(0.095)	(0.092)	(0.083)	(0.077)	(0.106)	(0.110)
	OTHER	0.153*	0.089	-0.066	-0.026	-0.017	-0.064
		(0.085)	(0.069)	(0.085)	(0.059)	(0.093)	(0.093)
	FTC	-0.012	-0.022	-0.247**	-0.067	-0.287**	-0.171*
		(0.100)	(0.072)	(0.070)	(0.062)	(0.100)	(0.089)
Reference: CPWL	CJ	0.020	0.028	0.143**	0.089	0.209*	0.221**
		(0.103)	(0.093)	(0.073)	(0.059)	(0.107)	(0.093)
	OTHER	0.159**	0.107**	0.040	-0.023	0.187*	0.118
		(0.059)	(0.053)	(0.065)	(0.063)	(0.109)	(0.096)
	FTC	-0.025	0.045	-0.039	0.006	-0.077	0.096
		(0.056)	(0.049)	(0.063)	(0.055)	(0.091)	(0.097)
Reference: OTHER	CJ	-0.094	-0.014	0.185**	0.146**	0.054	0.095
		(0.110)	(0.071)	(0.080)	(0.059)	(0.107)	(0.074)
	CPWL	-0.106	-0.040	-0.015	0.043	-0.139	-0.098
		(0.077)	(0.064)	(0.067)	(0.065)	(0.103)	(0.110)
	FTC	-0.212**	-0.037	-0.143**	-0.053	-0.180**	-0.054
		(0.051)	(0.048)	(0.066)	(0.059)	(0.085)	(0.078)
Reference: FTC	CJ	0.200**	0.033	0.257**	0.110*	0.375**	0.282**
		(0.084)	(0.065)	(0.083)	(0.057)	(0.111)	(0.062)
	CPWL	0.140**	-0.005	0.096	0.034	0.018	-0.072
		(0.071)	(0.053)	(0.069)	(0.060)	(0.089)	(0.097)
	OTHER	0.231**	0.042	0.125*	0.032	0.137*	0.065
		(0.057)	(0.048)	(0.068)	(0.063)	(0.083)	(0.079)

Remarks: * means that the estimated mean difference is significant at the 10% level and ** that it is significant at the 5%. Between parentheses we report the bootstrapped standard errors. For each duration (6, 12 or 16 months), we only consider recipients that have completed their programs.

Table 4c: Kernel matching vs. naive estimates of the mean differences (output: employment in a LTC job or in an FTC job)

		Just after the Program		3 months after		6 months after	
		Matching	Naive	Matching	Naive	Matching	Naive
Reference: CJ	CPWL	0.014	0.023	0.011	0.008	-0.038	-0.035
		(0.057)	(0.054)	(0.057)	(0.051)	(0.058)	(0.055)
	OTHER	0.103**	0.087*	0.114**	0.095**	-0.011	-0.005
		(0.051)	(0.047)	(0.057)	(0.046)	(0.056)	(0.050)
	FTC	0.004	-0.031	-0.017	-0.087**	-0.136**	-0.224**
		(0.044)	(0.037)	(0.049)	(0.043)	(0.049)	(0.042)
Reference: CPWL	CJ	-0.010	-0.023	0.009	-0.008	0.051	0.035
		(0.050)	(0.046)	(0.062)	(0.059)	(0.051)	(0.054)
	OTHER	0.091**	0.070	0.089*	0.073	0.052	0.050
		(0.047)	(0.046)	(0.051)	(0.051)	(0.055)	(0.048)
	FTC	-0.022	-0.063*	-0.053	-0.103**	-0.078	-0.149**
		(0.041)	(0.038)	(0.052)	(0.046)	(0.049)	(0.047)
Reference: OTHER	CJ	-0.080*	-0.087*	-0.059	-0.095*	0.055	0.005
		(0.049)	(0.049)	(0.052)	(0.050)	(0.049)	(0.052)
	CPWL	-0.059	-0.070	-0.066	-0.073	-0.059	-0.050
		(0.057)	(0.047)	(0.051)	(0.049)	(0.061)	(0.045)
	FTC	-0.092**	-0.098**	-0.131**	-0.144**	-0.143**	-0.161**
		(0.038)	(0.035)	(0.038)	(0.038)	(0.041)	(0.040)
Reference: FTC	CJ	-0.014	0.031	0.056	0.087**	0.228**	0.224**
		(0.056)	(0.041)	(0.059)	(0.044)	(0.053)	(0.045)
	CPWL	0.036	0.063*	0.074	0.103**	0.143**	0.149**
		(0.051)	(0.038)	(0.050)	(0.042)	(0.054)	(0.047)
	OTHER	0.096**	0.098**	0.122**	0.144**	0.145**	0.161**
		(0.038)	(0.032)	(0.039)	(0.041)	(0.044)	(0.041)

Remarks: * means that the estimated mean difference is significant at the 10% level and ** that it is significant at the 5%. Between parentheses we report the bootstrapped standard errors

9 References

Bonnal, L., Fougère, D., Sérandon, A. (1997): "Evaluating the Impact of French Employment Policies on Individual labor Market Histories". *Review of Economic Studies*, 64, 683-713.

DARES (1996). *40 ans de politiques de l'emploi*. Paris: La Documentation Française.

Dehejia, R., Wahba, S. (2000): "Causal Effects in Non-Experimental Studies: Re-Evaluating the Evaluation of Training Programs". *Journal of the American Statistical Association*, 94, 1053-1062.

Fougère, D., Kramarz, F., Magnac, T. (2000): "Youth Employment Policies in France". Forthcoming in *European Economic Review*.

Heckman, J. J., Hotz, J. V. (1989): "Choosing Among Alternative Nonexperimental Methods for Estimating the Impact of Social Programs: The Case of Manpower Training". *Journal of the American Statistical Association*, 84, 862-874.

Heckman, J. J., Ichimura, H., Todd, P. (1998): "Matching As An Econometric Evaluation Estimator". *Review of Economic Studies*, 65, 261-294.

Heckman, J. J., Ichimura, H., Smith, J. A., Todd, P. (1998): "Characterizing Selection Bias Using Experimental Data". *Econometrica*, 66, 1017-1098.

Heckman, J. J, Robb, R. (1985): "Alternative Methods for Evaluating the Impact of Interventions", in *Longitudinal Analysis of Labor Market Data*, J.J. Heckman and B. Singer (eds.), Cambridge University Press: New York.

Heckman, J. J., Smith, J. A. (1995): "Assessing the Case for Social Experiments". *Journal of Economic Perspectives*, 9, 85-100.

Heckman, J. J., Smith, J. A. (1999): "The Pre-Programme Earnings Dip and the Determinants of Participation in a Social Programme. Implications for Simple Programme Evaluation Strategies". *The Economic Journal*, 109, 313-348.

Imbens, G. W. (1999): "The Role of the Propensity Score in Estimating Dose-Response Functions". NBER Technical Working Paper No T0237.

Lechner, M. (1999): "Identification and Estimation of Causal Effects of Multiple Treatments under the Conditional Independence Assumption". Discussion Paper 9908, Volkswirtschaftliche Abteilung, Universität St. Gallen.

Magnac, T. (1997): "State Dependence and Heterogeneity in Youth Employment Histories". CREST Working Paper No 9747.

Rosenbaum, P. R., Rubin, D. (1983): "The Central Role of the Propensity Score in Observational Studies for Causal Effects". *Biometrica*, 70, 41-55.

Rubin, D. (1974): “Estimating Causal Effects of Treatments in Randomized and Nonrandomized Studies”. *Journal of Educational Psychology*, 66, 688-701.

Rubin, D. (1977): “Assignment to Treatment Group on the Basis of a Covariate”. *Journal of Educational Statistics*, 2, 1-26.

Scarpetta, S. (1993): “Assessing the Role of Labour Market Policies and Institutional Settings on Unemployment: A Cross-Country Study”. *OECD Economic Studies*, 26, 53-113.