Evaluation of Swedish youth labour market programmes

by

Laura Larsson*

Uppsala University

& Office of Labour Market Policy Evaluation

April 11, 2000

Abstract:

This paper evaluates and compares the direct effects of two Swedish active labour market programmes for youth, namely youth practice and labour market training. Effectiveness of the programmes is measured by subsequent annual earnings, re-employment probability, and probability of regular education. A non-parametric matching approach based on conditional independence assumption is applied to estimate the average program effects. Moreover, the results obtained by matching are compared to results from standard linear regression and probit models, and a polychotomous selectivity model with unobserved individual heterogeneity. The results indicate either zero or negative effects of both programmes in the short run, whereas the long-run effects are mainly zero or slightly positive. The results suggest furthermore that youth practice was better than labour training. However, there is much heterogeneity in the estimated treatment effects among the individuals.

Keywords: Active labour market policies, balancing score, evaluation, matching, propensity score, selection, treatment effects.
JEL classification: C14, C50, J38, J68.

---

* Laura Larsson, Office of Labour Market Policy Evaluation (IFAU), P.O. Box 513, S-751 20 Uppsala, Sweden. E-mail: laura.larsson@nek.uu.se. Previous versions of this paper have been presented at the Second Summer School in Labor Economics organised by IZA, May 10 – 16, 1999, at EALE conference in Regensburg, September 24 – 26, 1999, and at the IWH-workshop on Evaluation of Active Labor Market Policy and Welfare Programs in Halle, December 9 – 10, 1999. I am grateful for Denis Fougére for inviting me to an inspiring visit to CREST, Paris, and for the fruitful discussions there. Moreover, Per-Anders Edin, Bertil Holmlund, Per Johansson, Michael Lechner, and all the seminar participants at IFAU and the Department of Economics at Uppsala University deserve acknowledgements for their comments. I also want to thank Jochen Kluve and Winfried Koeniger, as well as other participants at the Summer School for valuable comments on a very early version of this paper. All remaining errors are mine.

1 Introduction

In the beginning of the 1990s, Sweden experienced a serious economic crisis and the unemployment figures rose drastically. The relative change in unemployment was approximately the same for all age groups, whereas the absolute change was largest for the youth; the unemployment rate for persons under 25 increased from 3 percent to 18 percent between 1990 and 1993. The corresponding increase for the adult population was approximately from 1 percent to 7 percent. Until the crisis in the 1990s, the high level of unemployment among the youth compared to the adults was not considered as a very serious problem, since youth unemployment typically was of a short-term nature. This pattern, however, changed during the crisis in the 1990s as the proportion of youth among the long-term unemployed grew rapidly.

As a response to the rising unemployment figures, the Swedish government increased the spending on active labour market policy in order to enhance the chances of the unemployed to return to regular employment. In 1992, a new large-scale program especially targeted at the unemployed youth, called youth practice, was introduced. Since participants in the active labour market programmes are defined either as employed or as being outside the labour force, the immediate effect of the programmes is that unemployment falls. This effect, however, is solely a matter of accounting. What the longer-term effects of the programmes are has been a matter of some controversy. Thus, the evaluation of these programmes has become an increasingly important issue.

This paper has two purposes. Firstly and most importantly, it is an evaluation of the two largest Swedish active labour market programmes directed to the youth in the first half of the 1990s, namely youth practice and labour market training. The aim is to determine the effects of the programmes on earnings, re-employment probability, and probability of studies provided by the regular educational system compared to what the outcome would have been had the person continued job searching as openly unemployed.[1] The focus is on the direct effects of the programmes, i.e. I make no attempt to assess the general equilibrium implications.[2]

[1] "Openly unemployed" refers to an unemployed who is not participating in any active labour market programmes.

[2] For a theoretical macroeconomic framework for studying both the direct and indirect effects, see Layard, Nickell and Jackman (1991) and Calmfors (1994).

Secondly, this paper is also a methodological study comparing some of the methods to obtain estimates for the expected ”treatment effects” proposed by the evaluation literature.[3] In the main part of the paper identification of the average treatment effects is based on the conditional independence assumption (CIA), i.e. an assumption that the participation in the various programmes is independent of the post-program outcomes conditional on observable exogenous factors. Given CIA, matching on the propensity score according to the multiple treatment approach introduced by Imbens (1999) and Lechner (1999b) can be applied to obtain unbiased estimates of the average treatment effects on both the treated and the population. Moreover, results from the matching analysis are compared to results obtained from standard linear regression and probit approach. Finally, a fully parametric selectivity model, due to Lee (1983), is applied to estimate the average earnings effects of the programmes.

Previous microeconomic studies of Swedish active labour market programmes have found mainly negative or zero effects of the programmes on the labour market outcomes of the youth (see e.g. Ackum 1991, Korpi 1994, Regnér 1997). Most of the studies, however, use data from the 1980s, and the quality of the methods as well as the data are not always as good as might be desired. Moreover, youth practice has not been evaluated. The contribution of this study to the thus far relatively thin literature of Swedish youth programmes is the introduction of new methods as well as the use of recent data of relatively good quality.

The paper is organised as follows: In Section 2, unemployment in Sweden during the 1990s and the youth programmes are described. The evaluation problem and the theoretical set-up are presented in Section 3. Section 4 describes the identification and estimation of the average treatment effects under the conditional independence assumption, and in Section 5 the data are described. Section 6 presents the econometric analysis based on the propensity score matching approach, while Section 7 shows results from the alternative evaluation models. Sensitivity analysis of the results is presented in Section 8, and, finally, Section 9 concludes.

[3] I use, yet reluctantly, the word ”treatment” to denote the various states including both the programmes and the ”no-program” state.

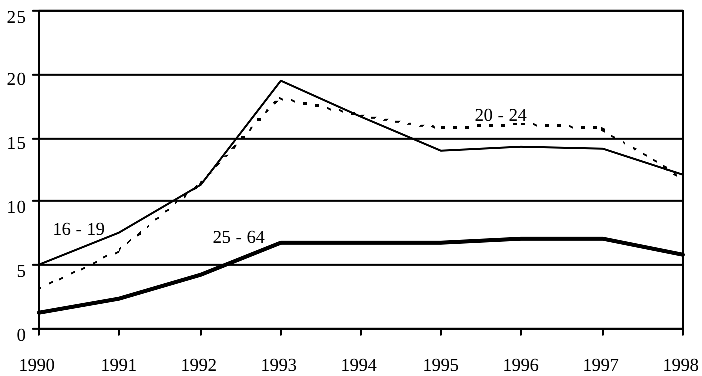
2 Youth unemployment and labour market programmes in Sweden

2.1 Youth employment and unemployment

It is a well-known fact in many European countries that unemployment among young people is more sensitive to fluctuations in the business cycle than adult unemployment. This has traditionally been the case in Sweden, too. Unemployment rates of the young labour force have been higher as well. Thus, the explosive rise in youth unemployment in the crisis of the 1990s is hardly a surprise: from being around 3 per cent in 1990, the unemployment rate for persons aged 20 – 24 rose to above 18 per cent in 1993, as shown by Figure 2.1. For the youngest age group the level unemployment rate was even higher until 1994. For adults (aged 25 – 64), the rise of the unemployment rate was from slightly more than 1 per cent to 7 per cent. After the peak in 1993, the situation has improved for the young cohorts, whereas the adult unemployment remained on the same level until 1997.

FIGURE 2.1

Unemployment rate by age 1990 – 1998.

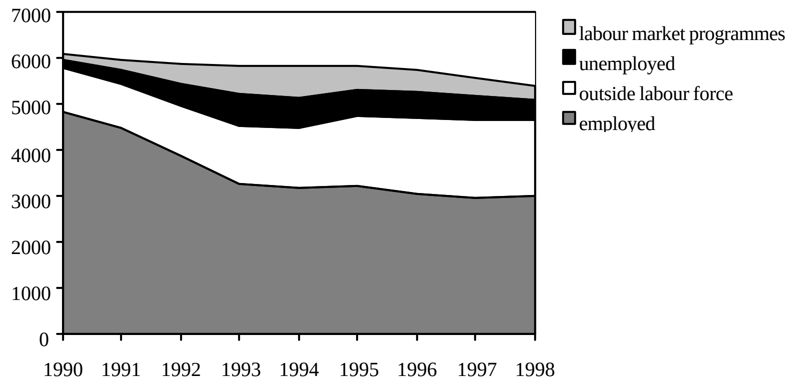


Source: Statistics Sweden, Labour Force Surveys

To explain the changes in the unemployment rate both before and after 1993, one has to look at the changes in both the number of unemployed and the labour force participation rate. Figure 2.2 shows the labour market status composition of the age group 20 – 24 during the 1990s. The rise in the unemployment rate until 1993 is due to both an increase in the number of unemployed and an even more drastic decrease in the number of employed. From 1993 and onward, the drop in employment declined considerably, and the number of unemployed started decreasing as well. Consequently, the share of 20 – 24 years olds outside the labour force (including the active labour market programmes), that had risen already before 1993 continued increasing. In 1994, approximately 12 per cent of the cohort participated in active labour market programmes. Thus, judged by the statistics, the drop in the employment rate after 1993 shown in Figure 2.1 seems to correlate with expansions in the active labour market programmes and the regular educational system rather than an increase in the number of unemployed.

FIGURE 2.2

Population aged 20 – 24 distinguished into employed, outside the labour force*, unemployed and in labour market programmes 1991 – 1998 (100 persons).



Source: Statistics Sweden, Labour Force Surveys; National labour market board; own calculations.
Note: The number of participants in labour market programmes in 1990 is an approximation based on the corresponding figure for person aged 18 – 24. *The correct expression is ”outside labour force for other reasons than labour market programmes”.

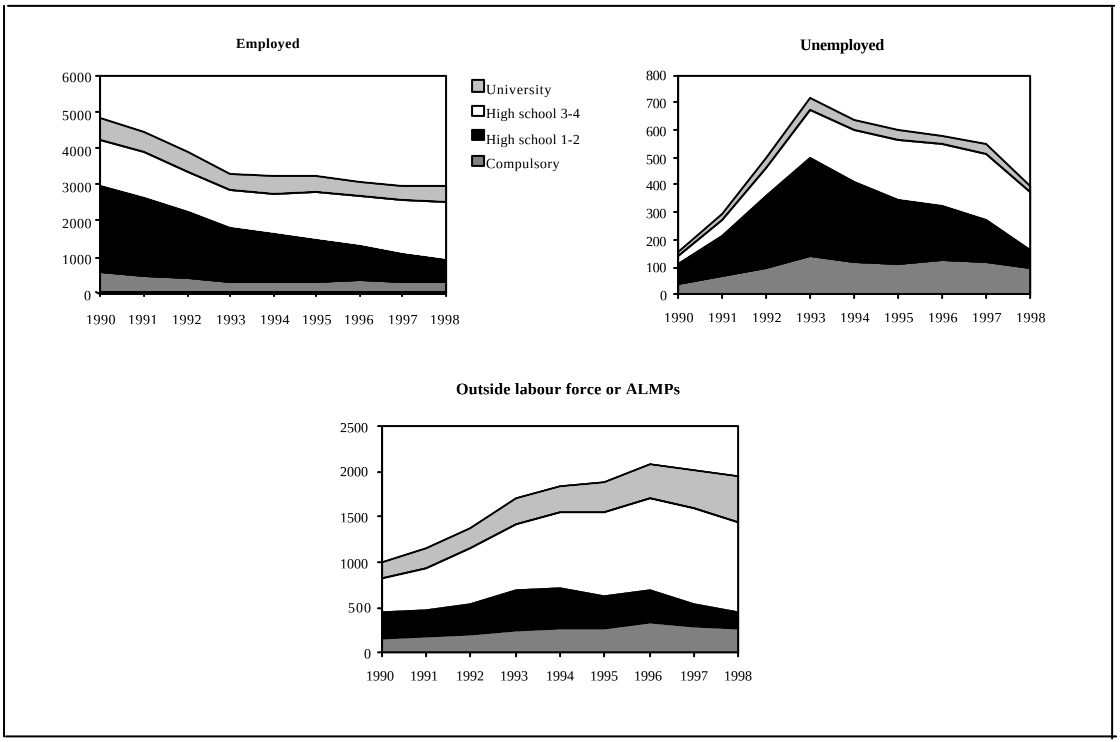
Furthermore, an examination of the educational level of the unemployed reveals that unemployment in Sweden has hit young people with various backgrounds. As shown by Figure 2.3, the unemployment rate rose significantly among all educational groups except the youth with university education. Thus, unemployment is not solely a problem of disadvantaged youth, i.e. youth that have not completed upper secondary school.[4] Consequently, the challenging task for labour market authorities has been to

[4] On the whole, the educational level in Sweden has gradually increased during the last decades. This is also illustrated in Figure 2.3.

construct programmes and other measures for a relatively heterogeneous group of young people.

FIGURE 2.3

Educational groups aged 20 – 24 distributed by labour market status, 1990 – 1998 (100 persons).



Source: Statistics Sweden, Labour Force Survey

2.2 Youth practice and labour market training

A large-scale youth program – youth practice (*ungdomspraktik*) – was launched in July 1992, during the worst period of rising unemployment. It was directed at unemployed youth aged 18 – 24. Youth practice was a subsidised work program[5], and the participants were placed in both private and public sectors. In order to minimise the potential displacement effects, they were supposed to perform tasks that otherwise would not have been done. Moreover, the participants were supposed to allocate 4 – 8 hours a week for job seeking activities. All unemployed young people aged 18 – 24 were eligible to apply for the program, the only formal restriction was that the unemployment period before enrolling into the program ought to be at least 4 months for

[5] Formally, youth practice was supposed to be a mixture of subsidised work and training in the sense that it would improve the participants' human capital. However, in practice the share of training was difficult to control for.

the youth aged 20 – 24 and 8 weeks for the young applicants. This restriction was supposed to assure that youth practice was used as ”the last resort” after all other alternatives were tested. However, empirical data reveals that the pre-program unemployment of the participants in youth practice varies remarkably from two or three days to several months.

The participants in youth practice received an allowance (*utbildningsbidrag*) below the going wage rate.[6] In the relatively rare cases, in which the participant was entitled to unemployment benefit he or she got an allowance equal to the benefit. Until 1994, the Employment Service covered all allowance costs, and from 1994 and onward, the employers paid 1,000 SEK a month. The program period was set to be six months, though it could in some cases be extended to another six months' period. As already pointed out in the previous passage concerning the pre-program unemployment, the Employment Service officials interpreted the rules often more as recommendations. After about three years, in October 1995, the active labour market programmes were reorganised in some respects, and new programmes replaced youth practice.

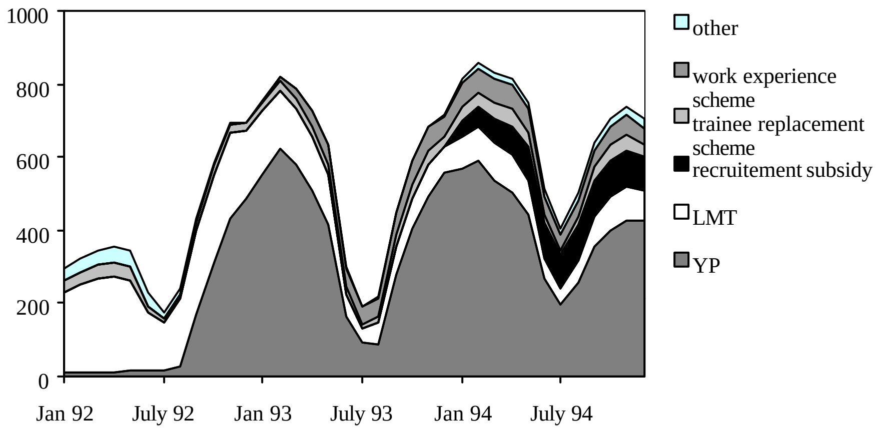
The other labour market program evaluated in this study is the traditional labour market training. Labour market training for unemployed has existed in various forms for a very long period. The main purpose of this program has been to improve the skills of the unemployed job seekers in such a way that they are better matched to the labour demand. The allowance for a participant in labour market training is the same as it was for a participant in youth practice, and as well as in youth practice, the participants in labour market training shall – at least formally – continue job searching during the program. In the 1990s, the age limit of labour market training was 20.

Arguments for the choice of programmes to be evaluated are illustrated by Figure 2.4 that shows the number and distribution of participants aged 20 – 24 in various active labour market programmes between 1992 and 1994.[7] Before the end of 1993, labour market training was clearly the second largest program, and the number of participants aged 20 – 24 in all other programmes was very low. From the second half of

[6] 338 SEK (approximately 39 EURO, January 2000) per day for participants aged 20 – 24 and 239 SEK (27.5 EURO) for the youngest participants.
[7] The large seasonal variation in the number of program participants in youth practice shown by Figure 2.4 is due to a rule that forbid assignment to the program over the summer months. This rule was supposed to prevent the employers from using youth practice participants as substitutes for regular summer workers.

1993, it could indeed be motivated to include some of the other programmes into the analysis, but due to restrictions in the data I concentrate on studying the early cohorts of youth practice and labour market training participants.[8] Moreover, the effects of youth practice on the young participants aged 18 – 19 are not evaluated in this study, because I want to examine a group of people who, at least formally, could have chosen any of the two programmes.

FIGURE 2.4
Number of participants aged 20 – 24 in various programmes, 1992 – 1994 (100 persons).



Source: Swedish national labour market board and own calculations

To understand the selection into the programmes, it is important to study the regulations and recommendations of the two programmes concerning their target group, since they are likely to affect the choice of the individual as well as the assignment decision done by the local employment office personnel. Allowance as well as rules for job search during the program were the same in both the programmes, whereas the main purposes of the programmes differed to some extent.[9] Labour market training was targeted mainly at persons with low skills in the field they were searching job, whereas the main aim with youth practice was to increase the young

[8] The data used in this study includes earnings information only until 1995.
[9] Concerning the allowance, it would be of interest to distinguish between those participants who were entitled to unemployment insurance (UI) from those who were not. In Sweden, the number of days an unemployed receives UI is restricted, but it is possible to extend the period by participation in active labour market programmes. Consequently, this possibility may affect the incentives to participate. However, there is no data on UI recipients for the first half of the 1990s. Moreover, most probably only a small share of persons under 25 were entitled to UI.

person’s working experience. Thus, the target groups of the programmes did formally differ concerning the level of education. However, in many cases both are needed in order to enhance the person’s chances of getting a job. As shown in the data section of this paper, there are similarities among the two participant groups, and thus I assume that many young unemployed considered both programmes as potential alternatives, which makes it relevant to examine whether the individual would have been better off by choosing the other program.

2.3 Measures of success

An explicit aim of the active labour market policy is to improve the employability of the unemployed. Hence, higher probability of future employment and higher earnings are natural measures of the programmes’ successfulness. However, when it comes to the youth, the picture might be more complicated. As discussed by e.g. Schröder and Sehlstedt (1989), there is a risk for especially young people of ending in ”vicious circles” of temporary unskilled jobs, unemployment and programmes. One positive way out might then be regular education. Thus, in this paper, probability of transition from unemployment to studies is considered as a third measure of success besides re-employment probability and earnings.

3 The Evaluation Problem

This study attempts to determine the outcomes of the three alternative strategies available for an unemployed youngster: to participate in either youth practice or labour market training or to continue job searching as openly unemployed. In other words, the aim is to determine the causal effect of a program compared to (*i*) the no-program state, and (*ii*) the other program. Following the language of among others Lechner (1999b), this multiple evaluation problem can be presented as follows.

Consider participation in $(M + 1)$ mutually exclusive treatments, denoted by an assignment indicator $T \in \{0, 1,\dots, M\}$. Let the ”0” category indicate the *no treatment* alternative. Moreover, denote variables unaffected by treatments, often called *attributes* (Holland, 1986) or *covariates*, by X. The outcomes of the treatments are denoted by $\{Y^0, Y^1, \ldots, Y^M\}$, and for any participant, only one of the components can be observed in the data. The remaining M outcomes are called counterfactuals.[10] The number of observations in the population is N, such that $N = \sum_{m=0}^{M} N^m$, where N^m is the number of participants in treatment m.

The evaluation problem is to define the effect of treatment m compared to treatment l, for all combinations of m, $l \in \{0, 1, \ldots, M\}$, $m \neq l$. More formally, the outcomes of interest in this study are presented by the following equations:

$$\text{(3.1)} \qquad q_0^{ml} = E\left(Y^m - Y^l \mid T = m\right) = E\left(Y^m \mid T = m\right) - E\left(Y^l \mid T = m\right),$$

$$\text{(3.2)} \qquad g_0^{ml} = E\left(Y^m - Y^l\right) = EY^m - EY^l .$$

q_0^{ml} in equation (3.1) denotes the expected average treatment effect of treatment m relative to treatment l for participants in treatment m (sample size N^m). In the binary case, where $m = 1$ and $l = 0$, this is usually called the ”treatment-on-the-treated” effect. g_0^{ml} in equation (3.2) is the corresponding expected effect for an individual drawn randomly from the whole population (N). Note that the latter expected effect is symmetric in the sense that $g_0^{ml} = -g_0^{lm}$, whereas the same is not valid for the treatment effect on the treated, i.e. $q_0^{ml} \neq -q_0^{lm}$, as long as the participants in treatments m and l differ in a non-random way.

The evaluation problem is a problem of missing data: one cannot observe the counterfactual $E\left(Y^l \mid T = m\right)$ for $m \neq l$, since it is impossible to observe the same individual in several states at the same time. Thus, the true causal effect of a treatment m relative to treatment l can never be identified. However, the *average* causal effects defined by equations (3.1) and (3.2) can be identified under the conditional independence assumption, presented in the following Section 4.

Moreover, to make causal analysis possible, the stable-unit-treatment-value assumption (SUTVA)[11] has to be satisfied for all individuals in the population. SUTVA

[10] Rubin (1974) first presented the word counterfactual in this context.
[11] The word ”stable-unit-treatment-value” refers to another implication of the assumption, namely that the treatment status of an individual (or ”unit”) is unrelated to the treatment status of other individuals. For a more detailed description and discussion of SUTVA, see e.g. Rubin (1978) and Angrist, Imbens and Rubin (1996).

has several consequences, out of which the most important in this context is that potential outcomes for an individual are independent of the treatment status of other individuals in the population. Thus, cross-effects and general equilibrium effects are excluded.

4 Matching as an Evaluation Estimator

In experiments, the participants are randomly assigned to the treatment(s) from a large group of eligible applicants. Thus, in a binary case, a comparison between the treated and the control group, which consists of the persons not assigned to the treatment, yields an unbiased estimate of the average treatment effect. Similarly, in a multiple case, an unbiased estimate of the average effect of one treatment compared to another is obtained by comparing the two randomly assigned treatment groups. In non-experimental studies this is not the case, since the various treatment groups most probably differ from each other in a non-random way. Hence, the object of a non-experimental evaluation study is to construct a comparison group as close as possible to the experimental control group. One method suggested to solve this problem is matching.

Matching methods have been developed and widely used in the statistics and medical literature (Rubin 1977; Rosenbaum and Rubin 1983, 1984, 1985; Rubin and Thomas 1992), but are relatively new to economics and the labour market policy evaluation methodology. In short, matching involves pairing together persons from various treatment groups who are similar in terms of their observable characteristics. When selection into the treatments is exclusively based on these observable characteristics, matching on them yields unbiased estimates of the average treatment effects.

4.1 Conditional Independence Assumption

The crucial assumption behind matching is that all the differences affecting the selection between the groups of participants in treatment m and treatment l are captured by (for the evaluator) observable characteristics X, and it is called conditional independence assumption by Rubin (1977). In the multiple case as presented in this paper, the assumption of conditional independence (CIA) is formalised as follows.[12]

$$\text{(4.1)} \qquad \left\{Y^0, Y^1, ..., Y^M\right\} \amalg T \mid X = x, \forall x \in \chi ,$$

where $\amalg$ is a symbol for independence. χ denotes the set of covariates for which the average treatment effect is defined. In words, CIA requires the treatment T to be independent of the entire set of outcomes. To understand the practical content of the assumption, consider the following: Given all the relevant observable characteristics (X), when choosing among the available treatments (including the no treatment alternative), the individual does not base this decision on the *actual* outcomes of the various treatments.[13] Moreover, for the average treatment effect to be identified, the probability of treatment m has to be strictly between zero and one, i.e.

$$\text{(4.2)} \qquad 0 < P^m(x) < 1 , \quad where \quad P^m(x) = E\left[P(T = m \mid X = x)\right], \quad \forall m = 0,1,...,M .$$

In the binary case of two treatments, Rosenbaum and Rubin (1983) show that if CIA is valid for X, it is also valid for a function of X called the *balancing score* $b(X)$, such that $X \amalg T \mid b(X)$.[14] The balancing score property holds even for the multiple case:

$$\text{(4.3)} \qquad \begin{aligned} &\left\{Y^0, Y^1, ..., Y^M\right\} \amalg T \mid X = x, \forall x \in \chi \quad if \\ &\rightarrow \quad \left\{Y^0, Y^1, ..., Y^M\right\} \amalg T \mid b(X) = b(x), \forall x \in \chi , \\ &E\left[P(T = m \mid X = x) \mid b(X) = b(x)\right] = P\left[T = m \mid X = x\right] = P^m(x), \\ &0 < P^m(x) < 1, \forall m = 0,1,...,M . \end{aligned}$$

[12] The significance and consequences of the conditional independence assumption (CIA) in the binary case of one treated and non-treated state have been explored and formalised by Rubin (1977) and Rosenbaum & Rubin (1983). The analysis of the multiple case presented in this paper follows closely the analyses presented by Lechner (1999) and Imbens (1999). Imbens calls the assumption in equation (4.1) strong unconfoundness.

[13] Of course, for the identification of a single treatment with fixed m and l it is sufficient to assume pair-wise independence $Y^l \amalg T = m, l \mid X = x, \forall x \in \chi$. Moreover, instead of conditional independence as presented in equation (4.1), it is sufficient to assume conditional *mean* independence, which is a somewhat weaker assumption. However, in practical applications, it is difficult to find a situation where the latter is fulfilled but not the first. For a thorough discussion on identifying assumptions, see Heckman, Ichimura and Todd (1998).

[14] The most trivial of balancing scores is the vector of X itself.

The main advantage of the balancing score property is the decrease in dimensionality: instead of conditioning on all the observable covariates, it is sufficient to condition on some function of the covariates. In the binary case of two treatments, the balancing score with lowest dimension is the propensity score $P^1(x) = E[P(T=1 \mid X = x)]$. In the case of multiple treatments, a potential and quite intuitive balancing score is the M-dimensional vector of propensity scores [$P^1(x)$, $P^2(x)$,…, $P^M(x)$].[15] Lechner (1999b) shows, however, that the dimension can be reduced further to two, or even to one. This is illustrated in the following section, where I discuss the identification of the average treatment effects defined by equations (3.1) – (3.2).

4.2 Identification

Let us first discuss the identification and estimation of the average treatment effect on the treated, q_0^{ml}. The mean outcome of treatment m for the participants in m, $E(Y^m \mid T = m)$, is identified and estimated by, for example, the sample mean. The conditional independence assumption implies that the latter part of the equation (3.1), i.e. the mean outcome of treatment l for the participants in m, $E(Y^l \mid T = m)$, is identified in large enough samples as:

$$\text{(4.4)} \qquad \begin{aligned} &E\lfloor E(Y^l \mid b(X), T = l) \mid T = m \rfloor = \\ &E\lfloor E(Y^l \mid b(X), T = m) \mid T = m \rfloor = E(Y^l \mid T = m). \end{aligned}$$

To estimate (4.4), Imbens (1999) and Lechner (1999b) show that instead of the M-dimensional balancing score the dimension of the condition set can be reduced to [$P^m(x)$, $P^l(x)$]. Thus,

$$\text{(4.5)} \qquad E(Y^l \mid T = m) = E\lfloor E(Y^l \mid P^m(X), P^l(X), T = l) \mid T = m \rfloor.$$

Lechner (1999b) shows that the dimension can be reduced further:

[15] By definition, $P^0(x) = 1 - [P^1(x) + P^2(x) +,\dots, + P^M(x)]$, and thus it is sufficient to condition on the M-dimensional vector instead of the complete (M+1) – dimensional vector of propensities.

$$(4.6) \qquad E\left(Y^{l} \mid T=m\right)=E\left[E\left(Y^{l} \mid P^{l|ml}(X), T=l\right) \mid T=m\right],$$

where $P^{l|ml}$ is the conditional choice probability of treatment l given either treatment m or l. In practice, however, it might be difficult to obtain the conditional choice probability, and thus both (4.5) and (4.6) are suggested for estimation of the average treatment effect on the treated.[16]

The identification and estimation of the average treatment effect for the whole population, g_0^{ml}, is possible in several ways. Lechner (1999b) suggests the following:

$$
\begin{aligned}
(4.7) \qquad g_0^{ml} &= E\left(Y^m \mid T=m\right)P\left(T=m\right) \\
&+ \underset{P^m(X)}{E}\left[E\left(Y^m \mid P^m(X), T=m\right) \mid T \neq m\right]P\left(T \neq m\right) \\
&- E\left(Y^l \mid T=l\right)P\left(T=l\right) + \underset{P^l(X)}{E}\left[E\left(Y^l \mid P^l(X), T=l\right) \mid T \neq l\right]P\left(T \neq l\right).
\end{aligned}
$$

In words, (4.7) implies that the average treatment effect on the population is identified by a weighted sum of the treatment effects on all sub-samples. For a more detailed description of the identification of q_0^{ml} and g_0^{ml} the reader should see Imbens (1999) and Lechner (1999b). The estimation procedures of the effects are described in the Appendix A.

5 The Data

5.1 Description of the data

The data used in this study is a random sample of approximately 200 000 individuals collected from the databases kept by Swedish national labour market board (AMS), and Statistics Sweden (SCB). The database at AMS includes records of all individuals who have been registered with the Employment Service, whereas SCB keeps records

[16] $P^{l|ml}$ is identified as $E\left[P^{l|ml}(X) \mid P^l(X), P^m(X)\right]=E\left[\frac{P^l(X)}{P^l(X)+P^m(X)} \mid P^l(X), P^m(X)\right]=P^{l|ml}(X)$.

of annual earnings for all persons registered in Sweden. For each person in the data used in this study, registration dates, labour market status, and individual characteristics between August 1991 and March 1997 are combined with information on annual earnings for the years 1985 to 1995. A more exact description of the variables used in the empirical analysis is given in Tables 5.1 – 5.4.

In the Employment Service records, each job seeker is registered under some ”job seeker category”, which defines his or her labour market status. Examples of the job seeker categories are openly unemployed, part-time unemployed, or participant in a labour market program. Persons signing up with Employment Service are also asked to fill in a so called search form, which contains questions about, for example, year of birth, citizenship, formal education, previous labour market experience, and what type of jobs the person is looking for. If a person wishes to apply for several jobs, she is asked to give each application either a high or a low priority. Moreover, the job seeker's home county and the code of the local employment office she visited are also recorded.

During a period in the Employment Service register the person may, and probably will, move from one category to another before de-registration. In other words, a person may enter in the register as openly unemployed, then participate in some labour market program, and be again openly unemployed before de-registration due to, for example, a transition to a regular job. All the relevant dates are provided in the data. Eventually, the reason for de-registration is recorded, as well.

The database at Statistics Sweden is constructed by using different administrative records, and it covers all persons who are registered in Sweden at the end of December each year. The information about earnings is based on firms' reports to the tax authorities. Earnings are measured on a yearly basis, and there is no information about the number of working hours. As a dependent variable in the empirical analysis of earnings, I use the annual sum of work-related income from various sources including the allowance for maternity or sickness leave and other work-related allowances from the social insurance. However, unemployment insurance and other unemployment-related income are not included in the variable. Variation in the dependent variable can thus reflect both changes in wage rates and changes in working hours.

5.2 Sample construction

From the database, I collect all individuals aged 20 to 24 who *registered with the Employment Service during 1992 and 1993 as openly unemployed*.[17] This procedure leaves me with 10,579 individuals. From this group I collect all individuals who, *after being openly unemployed, enrolled in youth practice or labour market training.* As a result, the groups consist of 1,657 youth practice participants and 606 labour market training participants.[18]

A potential comparison group consists of persons who entered in the register as openly unemployed during the same period 1992 – 93, and never participated in any of the programmes. There are slightly more than 5,000 such individuals. All of them could in principle be used as the third group of non-participants in the empirical analysis. However, there is one problem left to solve. As discussed in the evaluation literature (see e.g. Heckman and Smith, 1994, and Heckman, LaLonde and Smith, 1998), the pre-program unemployment history is very important in explaining the selection into a program. In explaining the choice among alternative programmes, the length of the unemployment period immediately before the program start is most presumably an important factor. Hence, in order to be able to use this information in the estimation of the propensities, I create a hypothetical program start date for the non-participants. The following procedure is similar to the *random* procedure suggested by Lechner (1999a).

First, both the group of participants (considered here as one) as well as the group of non-participants are divided into sub-groups by the month of registration with the Employment Service as openly unemployed. Second, each of the non-participants in a sub-group is randomly assigned an observation of ”length of pre-program unemployment” from the distribution of the contemporaneous sub-group of participants. In case the non-participant’s actual unemployment period is shorter than the assigned pre-program unemployment period, the individual is removed from the sample. This pro-

[17] Since I only have access to the year of birth, it is restricted to be 1967 – 72 for 1992 and 1968 – 73 for 1993. Thus, there are some persons both younger than 20 and older than 24 in the samples. For the mean ages, see Table 5.1.

[18] By restricting the program period to be a second ”seeking category” I exclude those program participants from the samples, who entered the program after several seeking periods. However, I consider it preferable in order to reduce heterogeneity in the participants’ unemployment history. Out of the complete sample of 10,579 persons, I have also removed weird observations with e.g. negative program periods or other curious dates.

cedure deletes approximately 60 % of the sample and thus leaves me with slightly more than 2,000 non-participants.

There are two important things to notice about this remaining group of non-participants. Firstly, it differs from the original, larger group of 5 000 individuals; when deleting those no longer openly unemployed at the drawn date the late program starts are deleted systematically. Consequently, the average length of pre-program unemployment is much shorter for the (remaining) group of non-participants than for the other groups, as shown by Table 5.1 in the following section. Moreover, non-participants with short actual unemployment periods are deleted, as well. However, this is desirable when the aim is to determine what would have been the outcome had the program participants not been assigned to their programmes at the time they actually were, since persons with very short unemployment periods probably never were considered as potential program participants.[19]

Secondly, and more importantly, this group of non-participants does not necessarily represent a world where no programmes exist; such a construction is possible only in a case where the probability of participating in the programmes *ever* is strictly below zero. In other words, it is possible only if the persons know that choosing not to participate now implies that they never will participate in that particular program. This is, however, not the case in Sweden, where the programmes continue existing and the unemployed who have not succeeded in finding a job (or for some other reason deregistered from the Employment Service) are offered new possibilities to participate. Thus, the 2 000 persons in the comparison group represent the alternative not to participate when the first chance is offered to them, but to *wait*. De facto we know, however, that these persons never participated in either of the programmes, and thus they are referred to as non-participant.

[19] As described in Section 2, the formal condition for participation in youth practice was four months of open unemployment. In practice, it was more considered as a recommendation, and thus the length of pre-program unemployment varies among the participants as it does for participants in labour market training, as well.

5.3 Descriptive sample statistics

Tables 5.1 – 5.2 present descriptive statistics of some selected variables for the three groups. There are clear differences in both the program characteristics as well as the individual characteristics among the participants in various states.

TABLE 5.1

Descriptive statistics of registration records and pre-program characteristics

	Non-participants (1)	Youth practice (2)	L.m. training (3)
Registration with ES			
Mean	November –92	December –92	July –92
Median	November –92	November –92	May –92
Assigned/true duration of pre-program unempl. in days (*mean*)	67.6	121.5	112.6
Pre-program unemployment at least four months (*mean*)	0.17	0.43	0.36
Annual earnings one year before registration (*mean*)	74,700	50,900	70,400
Assigned/true program start			
Mean	February –93	April –93	November –92
Median	January –93	March –93	September –92
Duration of program in days (*mean*)	–	146.6	131.3
Number of observations	2,024	1,657	606

Note: ”Program start” of the non-participants is a hypothetical date randomly assigned by a procedure described in Section 5.2. ”Duration of pre-program unemployment” of the non-participants is based on this hypothetical date.

In short, Table 5.1 shows that both the duration of pre-program unemployment as well as the duration of the program is shorter among the participants in labour market training compared with youth practice participants.[20] Secondly, the sample of labour market training participants consists of persons who were registered quite early with the Employment Service, and thus also started earlier in the program than the youth practice participants. Moreover, Table 5.2 shows relatively large differences in age, citizenship, education and experience among the three groups.

[20] For a discussion of the short pre-program unemployment period of the non-participants, see Section 5.2.

TABLE 5.2
Descriptive statistics of selected individual characteristics

	Non-participants (1)	Youth practice (2)	L.m. training (3)
Age (mean)	22.75	21.46	22.38
Female (%)	44	44	37
Non-Nordic (%)	4	5	13
Regional characteristics (%):			
Forest county	21	21	26
City county	41	29	35
Other county	39	50	39
Education (%):			
Compulsory	14	12	18
High School 1-2 years	41	41	40
High School 3-4 years	31	39	34
University	14	9	7
Specific education* (%):			
No	42	51	52
Yes	58	49	48
Experience* (%):			
None	34	45	40
Some	32	35	34
Good	34	21	26
Number of observations	2,024	1,657	606

Note: Age is an approximate for the age when registered with the Employment Service as openly unemployed. It is calculated as a difference between the year of registration and year of birth, since I do not have access to more precise data on the date of birth. The group with *compulsory education* includes even persons with a compulsory level education less than the legally required 9 to 10 years. *High school education* is divided into two groups depending on the length of the education.
* *Specific education* and *experience* refer to qualification required for the applied job, and the variables are based on information given by the job seekers when entering in the Employment Service records. For persons who have applied for several jobs, and thus have reported various levels of specific education and experience, I have used the observation with highest level of experience. Information on both specific education and experience is *missing* for approximately 16.1 % of the complete sample.

Finally, Table 5.3 presents some selected statistics from the local employment offices. The probability of being assigned into one of the three states is, among other things, assumed to be dependent on the proportion of all unemployed persons assigned to any program at the specific local employment office the month before actual assignment.[21] That proportion may be considered as a measure for how readily the office assigns persons to programmes and/or informs the applicants of the various alternatives. Furthermore, the decision between labour market training and youth practice is assumed to be dependent on the ratio between participants in these programmes.

[21] There are no records on the dates when persons are assigned to the programmes, and the period length between the decision and program start may vary among the programmes. I assume that, in case of youth practice and labour market training, the participants started in the program immediately after the decision was made, and thus I use data from the month before the persons entered the programmes. For non-participants, the shares are computed for the month before the hypothetical program start date.

TABLE 5.3

Descriptive statistics from the local employment offices, expressed as deviations from the contemporary country mean (percentage points).

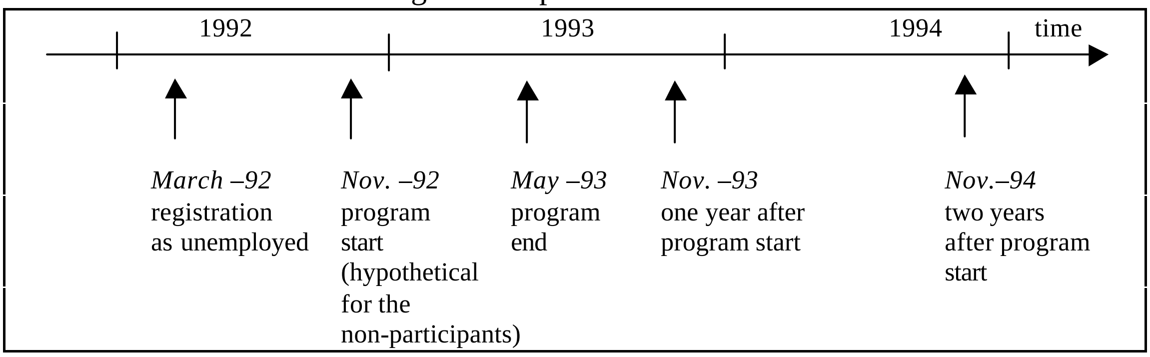
	Non-participants (1)	Youth practice (2)	L.m. training (3)
Share of program participants of all registered unemployed	-1.84	-0.77	-0.30
Share of youth practice of all program participants	-0,60	1.28	-0.99
Share of labour market training of all program participants	-0.05	-1.27	1.48
Number of observations	2,024	1,657	606

5.4 Measures of success

In this study, efficiency of the treatments is based on three criteria: annual earnings, re-employment probability, and probability of regular education. Earnings are measured by a continuous variable, whereas dummy variables – equal to unity if employed or student within 12 (or 24) months after program start, zero otherwise – are constructed for the two latter outcome measures. Figure 5.1 illustrates the way the various outcomes are defined for a hypothetical individual in the sample.

FIGURE 5.1

Illustration of a registration period and the outcome measures



This person signs up with the Employment Service in March 1992. Eight months later, in November 1992, she enrols into youth practice for a period of six months. Thus, she is defined as ”employed within one year (two years) after program start” if she is de-registered from the Employment Service due to regular employment by November 1993 (1994). Analogous definitions are used for the regular education.

Earnings one and two years after the program start, however, are measured in a somewhat less precise manner, since I only have access to an annual sum of earnings without any information on working hours. The time span between the program start and the moment the outcome is measured is now somewhat longer, since the earnings during and immediately after the program are almost by definition zero. For a person who enrolled into a program during the first half of a calendar year, ”earnings one year after the program start” are the annual sum of earnings for the following calendar year. Furthermore, for persons who started their program in July – December, I use the average of the two following calendar years. Thus, for the hypothetical person in Figure 5.1, ”earnings one year (two years) after the program start” are the average of the earnings for 1993 and 1994 (1994 and 1995). For non-participants I use the hypothetical date for program start, constructed as described in Section 5.2. Table 5.4 reports sample means of the various outcome measures for the three groups.[22]

TABLE 5.4
Sample means of the measures of success

	Non-participants (1)	Youth practice (2)	L.m. training (3)
Earnings one year after program start (SEK)	73,750	52,110	44,120
Earnings two years after program start (SEK)	89,300	74,770	66,700
Empl. within 12 months after program start	37 %	29 %	24 %
Empl.within 24 months after program start	42 %	41 %	39 %
Studies within 12 months after program start	11 %	10 %	5 %
Studies within 24 months after program start	12 %	13 %	9 %
Number of observations	2,024	1,657	606

Note: 100 SEK equals to 11.5 EURO (Jan. 2000). The low value of mean annual earnings is due to a large share of zero earnings.

6 Empirical Application

In this section, the empirical application of the matching methods on evaluation of Swedish youth programmes is presented. The matching algorithm is in many respects similar to the one by Lechner (1999b), and it is presented in detail in Appendix A.[23]

[22] Section 8 presents results for an analysis where the outcome variable for a participant is measured one or two years after the program end, instead.
[23] Heckman, Ichimura and Todd (1998) suggest other possible estimators. Estimators based on non-parametric kernel regressions have somewhat better asymptotic properties, whereas the main advantage of the estimators suggested by Lechner (1999b) is their computational simplicity.

6.1 Estimation of the Propensity

The discrete choice model to estimate the propensities is a multinomial logit model with three choice alternatives:

$$\text{(6.1)} \qquad \Pr(T_i = m) = \frac{\exp\left(X_i \boldsymbol{h}_m\right)}{\sum_{l=0}^{L} \exp\left(X_i \boldsymbol{h}_l\right)},$$

where *m* indexes the choice and *i* the individual. *X* is the vector of attributes. The choice alternatives are no treatment ($T = 0$), youth practice ($T = 1$), and labour market training ($T = 2$), and thus $L = 2$. The specification of the multinomial logit is based on likelihood-ratio tests for omitted variables in a binary framework.[24] The assumption of independence of irrelevant alternatives (IIA) underlying the multinomial logit model may be argued to be too restrictive in this context. To check whether this is the case I have applied binomial logit models to estimate the propensities for all three comparisons. The results are presented in Section 8 with other sensitivity analysis. The estimated coefficients of the binomial and multinomial models are similar to each other, and thus the IIA assumption is considered to be sufficiently valid.

The results presented in Table 6.1 show that the significance of various explanatory variables differs across the two programmes. However, the variables for the pre-program unemployment history, as well as the variables from the local employment offices seem in general to have high significance. The model's predictive power is presented in Table 6.2, and I consider it satisfactory: approximately six of ten observations are correctly predicted, when the highest of the propensities determine the prediction. Observations in the sub-samples of non-participants and youth practice participants are predicted correctly in at least seven cases of ten. Outcomes in the smallest sub-sample of labour market training, though, are predicted correctly in merely seven cases of hundred.

[24] The specification of the discrete choice model is also based on the match quality is produces: Results from the tests for balance of the covariates, described in Section 6.2, may suggest another specification of the model.

TABLE 6.1

Results from the multinomial logit estimations

	Youth practice			Labour market training		
	Coefficient (1)	Std. error (2)	RRR (3)	Coefficient (4)	Std. error (5)	RRR (6)
Constant	**-57.7**	**8.45**	–	0.17	9.81	–
Personal characteristics:						
Female	0.15	0.08	1.17	-0.14	0.10	0.87
Age	**5.59**	**0.76**	**268**	-0.04	0.88	0.96
Age^2	**-0.14**	**0.02**	**0.87**	-0.00	0.02	1.00
Non-Nordic	0.24	0.18	1.27	**1.22**	**0.18**	**3.38**
Regional characteristics:						
Forest county	-0.14	0.11	0.87	**0.35**	**0.13**	**1.42**
City county	**-0.61**	**0.09**	**0.54**	-0.18	0.12	0.83
Education[1]:						
High School 1-2 years	**0.28**	**0.13**	**1.33**	-0.16	0.15	0.86
High School 3-4 years	0.23	0.13	1.25	-0.07	0.16	0.93
University	0.18	0.18	1.19	**-0.52**	**0.23**	**0.60**
Specific education[2]:						
Yes	**-0.27**	**0.09**	**0.76**	-0.15	0.12	0.86
Missing	-0.19	0.14	0.82	0.03	0.18	1.03
Experience[2]:						
Some	-0.11	0.11	0.90	0.00	0.14	1.00
Good	**-0.37**	**0.12**	**0.69**	**-0.40**	**0.16**	**0.67**
Pre-program labour market status:						
Duration of pre-program unemployment (days)	**0.01**	**0.00**	**1.01**	**0.01**	**0.00**	**1.01**
Earnings 1 year before reg. With ESR (10,000 SEK)*	**-0.04**	**0.01**	**0.96**	0.00	0.01	1.00
Local employment office variables[3]:						
Share of program part. of all registered unemployed	**1.94**	**0.47**	**6.98**	**2.57**	**0.59**	**13.0**
YP of all program part.	**0.88**	**0.37**	**2.40**	0.58	0.47	1.79
LMT of all program part.	0.77	0.21	0.93	**1.28**	**0.59**	**3.58**
Missing	**0.77**	**0.21**	**2.16**	**0.90**	**0.23**	**2.45**
Log likelihood: -3,559.3, LR chi2 (38): 1440.9, Pseudo R2: 0.1683						

Note: Non-participants are used as a reference category. Columns (1) and (4) report the coefficients β^{YP} and β^{LMT}, and columns (2) and (5) show the standard errors of the estimated coefficients. **Bold type** indicates statistical significance at the 5 % level. Relative risk ratios (RRR) in columns (3) and (6) report the exponentiated value of the coefficient, $\exp(\beta^{YP})$. It is interpreted as the relative probability (or risk) ratio for one unit change in the corresponding variable, when risk is measured as the risk of the category relative to the reference category. *Age* is an approximate for the age when registered with the Employment Service as openly unemployed. **1)** Compulsory education is the refereerence level. **2)** *Specific education* and *experience* refer to qualification required for the applied job, and the variables are based on information given by the job seekers when entering in the Employment Service records. For persons who have applied several various jobs, and thus have reported various levels of specific education and experience, I have collected the observation with highest level of experience. The dummy variable *Missing* indicates the observations for which both ”specific education” and ”experience” is missing (approximately 16.1 % of the complete sample). Reference level is no specific education or experience. **3)** The variables from the local employment offices are computed as deviations from the contemporaneous country mean. Missing observations are set to zero, and denoted by the dummy variable *Missing* equal to one.* ESR stands for Employment Service Register.

TABLE 6.2
Predictive power of the multinomial logit model

Predicted outcome:	True outcome: No participation	Youth practice	L. m. Training	Total:
No participation	1,541 (76.1 %)	491 (29.6 %)	332 (54.8 %)	2,364 (55.1 %)
Youth practice	461 (22.8 %)	1,153 (69.6 %)	233 (38.5 %)	1,847 (43.1 %)
L. m. training	22 (1.1 %)	13 (0.8 %)	41 (6.8 %)	76 (1.8 %)
Total:	2,024 (100 %)	1,657 (100 %)	606 (100 %)	4,287 (100 %)

Distributions of the predicted propensities are presented in Figures B1 – B3 in the Appendix. In broad outline, a good model produces large differences of the mean predicted propensities across the various groups. This is the case for propensities to participate in youth practice and to not participate, whereas the distributions of propensities to participate in labour market training, presented by Figure B3, look very similar. Once again, this may be a result of the small size of the sub-sample compared to the other sub-samples.

A correct estimation of the average treatment effects q_0^{ml} and g_0^{ml} requires a common support for the treatment and the comparison group, i.e. $0 < P^m(x) < 1$ for all m = 0,1,…, M. In practice, this implies that some of the observations are excluded from the sample, if the propensity distributions do not cover the exact same interval. A minimum requirement is that all observations in sub-sample m for which there does not exist a comparison observation in sub-sample l (m, $l \in \{0, 1,\ldots, M\}$, $m \neq l$) are removed from the sample. In a multiple treatment analysis, comparisons are done between all groups, and thus the common support restriction is expressed as follows:

For an observation in sub-sample k with $P(T = j \mid X)_{T=k} = p$ to be included in the analysis, the following has to be valid:

(6.2) $$P\left(P(T = j \mid X)_{T=k} = p, P(T = j \mid X)_{T=h} = p\right) > 0,$$
$$\forall\, h, k, j = 0,1,\ldots, M, \text{ and } p > 0.$$

The vertical lines in Figures B1 – B3 illustrate this common support restriction; the tails outside the common support area are excluded from the analysis. As a result, approximately 200 observations are deleted leaving a sample size of 4,084.

6.2 Matching

In the binary case of two treatments, the sub-sample of non-participants consists generally of a large number of observations, and thus each comparison unit can be used only once. In the multiple case this is not meaningful, since pair-wise comparisons are done across all sub-samples.[25] Thus, matching is done with replacement, i.e. each comparison unit may be used more than once given that it is the nearest match for several treated units. The covariance matrix for the estimates of average effects, suggested by and presented in Lechner (1999b), pays regard to the risk of ”over-using” some of the comparison units: the more times each comparison is used, the larger the standard error of the estimated average effect.

A detailed description of the matching algorithm is presented in the Appendix A. The pair-wise matching procedure is carried through altogether six times. Each individual in the treated sub-sample m is matched with a comparison in the sub-sample l, and the criteria for finding the nearest possible match is to minimise the Mahalanobis distance of $[P^m(X), P^l(X)]$ between the two units.[26]

Furthermore, covariates in the matched samples ought to be balanced according to the condition $X \coprod T \mid b(X)$. Table 6.3 reports the absolute standardised biases for each covariate and pair-wise comparison. In general, the match quality is satisfactory, and thus I consider the condition $X \coprod T \mid b(X)$ to be sufficiently fulfilled.

[25] For example, to estimate the effect of youth practice (N = 1,592 within common support) compared to labour market training (N = 580), one has to find a match for each youth practice participant from the latter sub-sample. If matching was done without replacement, one could only make use of at most 580 observations in the sub-sample of youth practice, and thus a lot of information would be lost.

[26] Results using the ”naive” conditional probability $P^{l|ml}(X)$ obtained by binomial logit analysis are presented in Chapter 8.

TABLE 6.3

Balance of the covariates measured by absolute standardised bias (ASB).

	YP – Non (1)	Non – YP (2)	LMT – Non (3)	Non – LMT (4)	YP – LMT (5)	LMT – YP (6)
Female	2.25	1.2	1.4	0.2	4.7	11
Age	2.2	1.9	1.4	4.4	4.7	1.7
Non-Nordic	4.9	4.5	2.7	1.7	2.1	13
Regional characteristics:						
Forest county	4.5	2.1	5.1	8.4	3.8	3.6
City county	9.4	5.9	0.7	4.0	6.3	2.2
Other county	4.7	4.2	3.9	2.9	2.5	1.1
Education[1]:						
Compulsory	6.6	3.8	0.0	9.8	0.2	8.3
High School 1-2 years	5.9	1.2	6.6	1.0	11	0.4
High School 3-4 years	4.0	5.9	6.3	3.5	3.7	7.0
University	4.5	2.3	1.4	7.6	14	0.7
Specific education[2]:						
Yes	0.6	0.8	4.1	3.5	0.5	7.3
Experience[2]:						
None	2.0	0.2	4.2	1.0	7.5	9.1
Some	5.5	3.0	5.0	1.2	0.4	7.3
Good	9.4	2.8	0.8	0.2	8.5	2.4
Pre-program labour market status:						
Duration of pre-program unemployment (days)	7.0	3.1	0.2	1.2	1.5	7.5
Earnings 1 year before registration with ESR	2.7	6.6	10	2.8	3.5	9.7
Local employment office variables[3]:						
Share of program participants of all registered unemployed	6.2	1.2	6.1	0.7	7.0	6.5
YP of all program part.	5.7	5.4	6.9	0.6	7.9	5.6
LMT of all program part.	3.6	6.9	8.4	0.5	3.3	0.6
Median ASB	4.5	2.8	4.1	2.8	4.7	6.5
No. of observations, $m - l$	1,592 – 711	1,912 – 722	580 – 439	1,852 – 459	1,592 – 425	580 – 388

Note: The absolute standardised bias (ASB) is defined as $\frac{abs(\bar{x}_m - \bar{x}_l)}{\sqrt{\{s_m^2(x_{mi}) + s_l^2(x_{li})\}/2}}$, where $\bar{x}_j$ and $s_j^2(x_{ji})$ ($j = m$, l) are the sample mean and variance of each covariate x_{ji}. The last row reports the number of treated and comparisons in each matched sample: for example, as reported in column (1), 711 non-participants are used as matches for 1,592 youth practice participants. ”Non” indicates the “no program” state.

6.3 Results

For each matched pair, the differences in earnings, employment and studies between the two units is an estimate for the treatment effect for the treated. Aggregation of the pair-wise differences over the common support obtains an estimate of the average treatment effects on the treated, q_0^{ml}. Finally, average treatment effects on the whole population (within the common support), g_0^{ml}, are obtained by taking weighted sums of the treatment effects on the treated.[27] The exact expressions for q_0^{ml} and g_0^{ml} are found in Lechner (1999b).

6.3.1 Heterogeneity in the pair-wise differences

Figures C1.1 – C3.3 in the Appendix illustrate individual heterogeneity in the treatment effects on the treated in respect to the propensity of a treatment. The heterogeneity in earnings effects is presented for each matched couple, whereas the pair-wise differences in probability of re-employment and studies are aggregated over intervals of one percentage point. The reason for this is that for individual matched couples the difference is always by definition –1, 0 or 1, and illustrating that in a figure does not show much. Moreover, all the figures next to each other present the effects ”in the same direction”. For example, Figure C1.1(i) shows the short term earnings effect of youth practice compared to non-participation for those who actually participated in youth practice, and Figure C1.1(ii) next to it presents what the effect of youth practice would have been for those who actually did not participate in anything. The propensity on the x-axis is the same, as well. Thus, similar patterns in figures next to each other indicate little heterogeneity in the treatment effect *among* the treatment and comparison group, whereas large dispersion or other patterns of the dots in a figure implies heterogeneity *within* the group.

Figures C1.1, C2.1 and C3.1 illustrate earnings effects for all treatment comparisons, and look relatively similar to each other. There is a great deal of heterogeneity within each sample, but the extent of the variation and the size of the effects do not

[27] The weights to calculate the average population effect of treatment *m* compared to treatment *l* are based on the number of times each unit is used in all various comparisons, i.e. not only the comparisons between treatments *m* and *l*. Consequently, the average population effect may differ quite considerably from the average of the treatment effects on the treated, $\left(q_0^{ml} + (-q_0^{lm})\right)/2$.

seem to depend on the propensity of being assigned to the treatment on the x-axis. In other words, the average effects of the programmes compared to non-participation and to each other appear to be approximately the same irrespective of the propensity of being assigned to the programmes.

Turning to the employment effects, Figures C1.2, C2.2 and C3.2 show more distinct patterns of heterogeneity. Employment probability as dependent variable, the effect of youth practice compared to non-participation appears to be worse for those with very low or very high propensity of youth practice than for persons with medium-sized propensity. Especially non-participants with very high propensity of youth practice would have been much worse of had they participated in the program. For labour market training compared to non-participation the opposite is valid: Figure C2.2 shows that the employment effect seems to be more positive for persons with high propensity of labour market training. Moreover, when compared with labour market training the effect of youth practice on employment probability seems to be worse for those with highest probability of being assigned to youth practice, though the pattern is not all clear. Finally, Figures C1.3, C2.3 and C3.3 reveal little heterogeneity in the treatment effects on the probability of studies.

6.3.2 Average treatment effect on the treated

Table 6.4 reports the results for the six various treatment on the treated effects. Each estimated effect is reported both in absolute and relative terms. Presentation of the absolute size of the effects makes it possible to compare the magnitude of the effects between the treatment and comparison groups, i.e. column (1) and (2), (3) and (4), and (5) and (6). The relative effects indicate how considerable the size of the effect is, and helps to analyse the changes in the results due to changes in the model specification, presented in Section 8.

TABLE 6.4
Results for the mean treatment effect on the treated: $\theta_0^{ml} = E(Y^m \mid T = m) - E(Y^l \mid T = m)$.
t-values in parentheses, *relative effects in italics*.

	YP – Non (1)	Non – YP (2)	LMT – Non (3)	Non – LMT (4)	YP – LMT (5)	LMT – YP (6)
Earnings one year after program start (SEK)	**-14,565**	**16,380**	**-23,440**	**27,100**	**15,560**	-6,690
	(- 3.82)	**(4.03)**	**(-5.22)**	**(7.16)**	**(3.92)**	(-1.50)
	-22 %	***29 %***	***-35 %***	***59 %***	***42 %***	*-13 %*
Earnings two years after program start (SEK)	-3,330	5,060	**-14,080**	**14,450**	11,480	-2,170
	(-0.50)	(0.76)	**(-2.33)**	**(2.22)**	(1.45)	(-0.34)
	-4 %	*6 %*	***-17 %***	***20 %***	*18 %*	*-3 %*
Employment within 12 months after program start (percentage points)	**-0.07**	**0.10**	**-0.10**	**0.11**	**0.06**	-0.01
	(-2.46)	**(3.25)**	**(-3.30)**	**(3.77)**	**(2.03)**	(-0.16)
	-18 %	***37 %***	***-30 %***	***41 %***	***27 %***	*-2 %*
Employment within 24 months after program start (percentage points)	0.02	0.03	-0.01	-0.02	0.03	0.00
	(0.82)	(1.00)	(-0.31)	(-0.64)	(0.71)	(-0.05)
	6 %	*8 %*	*-3 %*	*-5 %*	*6 %*	*0 %*
Studies within 12 months after program start (percentage points)	-0.01	0.00	-0.03	**0.06**	**0.06**	**-0.04**
	(-0.42)	(0.05)	(-1.69)	**(3.77)**	**(3.20)**	**(-2.00)**
	-7 %	*1 %*	*-33 %*	***102 %***	***127%***	***-42 %***
Studies within 24 months after program start (percentage points)	0.01	-0.00	0	**0.05**	**0.04**	-0.02
	(0.64)	(0.21)	(0)	**(2.40)**	**(2.06)**	(0.96)
	10%	*-4 %*	*0 %*	***62 %***	***51 %***	*-20 %*
No. of observations *m – l**	1,592 – 711	1,912 – 722	580 – 439	1,852 – 459	1,592 – 425	580 – 388

Note: **Bold type** indicates statistical significance at the 5 % level. For a description of the dependent variables, see Figure 5.1). * The number of observed earnings two years after is somewhat lower than for the other outcome variables.

First, let us compare the programmes to the state of no participation presented in columns (1) – (4). The short-term effects on both earnings and employment are through out significantly negative for both programmes and all groups. However, after two years from the program start they have become more positive and the only significantly negative effects are found for the effects of labour market training on earnings. Youth practice does not seem to have any effect on the probability of studies, whereas participation in labour market training would have significantly decreased the study probability of the non-participants, as presented in column (4).

Second, comparison of the two programmes indicates that youth practice was better for those who actually participated in it in terms of all outcome measures. All effects reported in the column (5) of Table 6.4, except for the long-term employment effect, are statistically significant and positive. For the group of participants in labour market training the difference between the programmes seems to be less significant though in the same direction as for youth practice participants.

6.3.3 Average treatment effect for the population

Table 6.5 presents the estimated average treatment effects on the population. In short, the results confirm the impression given by Table 6.4: In the short run, both the programmes result in lower earnings, as well as lower probability of employment, compared to what the outcome would have been without participation in any program. Similar to the prior results, the negative effects more or less disappear in course of time. Youth practice does not seem to have any effect on the probability of studies, while the effect of labour market training is significantly negative. Moreover, youth practice seems to have been ”less harmful” than labour market training, except for the effect on employment probability for which the effect is statistically insignificant.

TABLE 6.5

Results for the average treatment effect on the population:

$$g_0^{ml} = E\left(Y^m - Y^l\right) = EY^m - EY^l .$$

t-values in parentheses, *relative effects in italics.*

	YP – Non (1)	LMT – Non (2)	YP – LMT (3)
Earnings one year after program start (SEK)	**- 15,740** **(-4.12)** ***-23 %***	**-27,760** **(-7.46)** ***-39 %***	**12,020** **(3.73)** ***30 %***
Earnings two years after program start (SEK)	- 2,320 (-0.49) *-3 %*	2,900 (0.64) *5 %*	-5,220 (-1.36) *-7 %*
Employment within 12 months after program start (percentage points)	**-0.09** **(-3.00)** ***-23 %***	**-0.12** **(-4.34)** ***-33 %***	0.03 (1.26) *11 %*
Employment within 24 months after program start (percentage points)	-0.01 (-0.26) *-2 %*	0.01 (0.24) *3 %*	-0.02 (-0.62) *-5 %*
Studies within 12 months after program start (percentage points)	0.00 (0.10) *0 %*	**-0.06** **(-3.75)** ***-50 %***	**0.06** **(3.56)** ***150%***
Studies within 24 months after program start (percentage points)	0.01 (0.51) *8 %*	-0.03 (-1.93) *-25 %*	**0.04** **(2.64)** ***44 %***

See the notes under Table 6.4.

7 Alternative methods to estimate the average effects

An often-referred advantage of matching methods is that average treatment effects can be obtained without a parametric specification. This is, of course, not completely true when matching is based on the propensity score, since a parametric, or at least a semi-parametric, model is generally used to estimate the propensities. Nevertheless, matching is a relatively flexible and above all intuitive method to compare the effects of various treatments and to explore the extent of heterogeneity in the treatment effect among the individuals. Yet, the cost of using matching is not unimportant. Firstly, the assumption of conditional independence is not only very strong, but also impossible to test. There is always some uncertainty left whether or not one has managed to control for all the variables affecting the selection into the various states. Secondly, even though one does not have to specify the outcome model, there are several other decisions to make concerning, among other things, the specification of the discrete choice model, the criterion of matching, and the definition of common support.

Hence, in this second part of the paper I introduce two other approaches for determining the average treatment effect on the population, and relate them to the method of propensity score matching and the results presented in the previous section. The first approach involves the standard OLS regression for continuous dependent variables, and probit model for discrete dependent variables. As in the matching approach, identification of the average treatment effect in these models requires that the conditional independence assumption is valid. Moreover, I apply the polychotomous selectivity model introduced by Lee (1983) to investigate the existence of unobserved heterogeneity.

7.1 Linear regression and probit

The question this section examines whether the results obtained by matching on the propensity score differ from results from a standard linear regression – for continuous outcome variables – and a probit model for discrete outcome variables. Briefly, the difference between matching and regression is the underlying weighing scheme used to aggregate the estimates at different values of the covariates. As shown by a simple example in Angrist (1998), the weights of the matching estimator are proportional to the probability of being treated at each value of the covariates, whereas the weights underlying the regression estimator are proportional to the variance of treatment status at each value of the covariates.[28] Similarly, the weights underlying a probit model are likely to differ from the matching weights.

The regression model can be described as follows: Let us assume that the post-program earnings of individual *i* are given by:

$$(7.1) \qquad Y_i = X_i \boldsymbol{b} + D_{1i}\boldsymbol{a}_1 + D_{2i}\boldsymbol{a}_2 + u_i \ ,$$

where D_{1i} is a dummy taking the value of one if the individual has participated in youth practice and zero otherwise; D_{2i} is a dummy taking the value of one if the individual has participated in labour market training and zero otherwise; $\boldsymbol{a}_1$ and $\boldsymbol{a}_2$ are the average effects of the programmes on earnings compared to the state of no participation. The error term u_i is assumed to be independently and identically distributed across individuals with $E[u_i] = 0$. X_i is a vector of covariates assumed to affect the earnings. Selection bias in equation (7.1) arises because of a stochastic relationship between D_{1i}, D_{2i} and u_i, that is

$$(7.2) \qquad E[u_i | D_{1i}, D_{2i}, X_i] \neq 0 \ .$$

Moreover, the treatment assignment is modelled by a latent variable D^*_{ji} as follows:

$$(7.3) \qquad D^*_{ji} = Z_i \boldsymbol{h}_j + \boldsymbol{e}_i \ ,$$

where Z_i are covariates assumed to affect the selection into a treatment, and $j = 0, 1, 2$ indexes the various treatments. Enrolment into the treatments is thus defined by the following:

[28] However, I find the example somewhat misleading, since it compares the estimate of treatment effect on the treated with an average treatment effect for the population.

(7.4) $$D_{1i} = D_{2i} = 0 \quad iff \quad D_{0i}^{*} = \max\left(D_{0i}^{*}, D_{1i}^{*}, D_{2i}^{*}\right),$$

(7.5) $$D_{1i} = 1, D_{2i} = 0 \quad iff \quad D_{1i}^{*} = \max\left(D_{0i}^{*}, D_{1i}^{*}, D_{2i}^{*}\right),$$

(7.6) $$D_{1i} = 0, D_{2i} = 1 \quad iff \quad D_{2i}^{*} = \max\left(D_{0i}^{*}, D_{1i}^{*}, D_{2i}^{*}\right).$$

Selection bias as describes above can arise from two sources. First, the dependence between Z_i and u_i is the case described in the previous section, i.e. the selection is *on observables*. Second, there may be a dependence between ε_i and u_i, usually referred to as selection *on unobservables*. If selection is assumed to be on observables, and, furthermore, the dependence between Z_i and u_i is assumed to be linear, the vector Z_i may be included into the equation of outcome (7.1) to obtain unbiased estimates of the average treatment effects.[29] Thus, to obtain unbiased estimates of the treatment effects the following equation is estimated:

(7.7) $$Y_i = X_i \beta + Z_i \gamma + D_{1i}\alpha_1 + D_{2i}\alpha_2 + v_i \,.$$

Based on similar arguments, all covariates affecting the selection process as well as the outcome may be included into a probit model for re-employment probability or probability of studies. Formally, let us denote the post-program employment by a dummy variable E_i, such that $E_i = 1$ if the person is employed, and zero otherwise. The value of the dummy variable is defined by an unobserved latent variable as follows:

(7.8) $$E_i^{*} = X_i \beta + D_{1i}\alpha_1 + D_{2i}\alpha_2 + u_i \,,$$

$$\text{such that} \quad \begin{array}{l} E_i = 1 \quad iff \quad E_i^{*} > 0, \\ E_i = 0 \quad iff \quad E_i^{*} \le 0. \end{array}$$

Furthermore, let the probability of employment be characterised by the normal cumulative distribution function $\Phi(\bullet)$ as follows:

[29] This is called the linear control function approach, see e.g. Heckman and Robb (1985).

(7.9) $$P(E_i = 1) = P(X_i \boldsymbol{b} + D_{1i}\boldsymbol{a}_1 + D_{2i}\boldsymbol{a}_2 > u_i) = \Phi(X_i \boldsymbol{b} + D_{1i}\boldsymbol{a}_1 + D_{2i}\boldsymbol{a}_2),$$

As in the case of earnings presented above, if selection is assumed to be on observables Z_i , and, furthermore, the dependence between Z_i and u_i to be linear, the vector Z_i may be included into equation (7.9) to obtain unbiased estimates of the average treatment effects. Formally, the equation estimated is

(7.10) $$P(E_i = 1) = \Phi(X_i \boldsymbol{b} + Z_i \boldsymbol{g} + D_{1i}\boldsymbol{a}_1 + D_{2i}\boldsymbol{a}_2).$$

The probability of studies (S_i) is defined in a similar way. Results for the regression and probit estimations are presented in Table 7.1. The sample is identical to the one used in the previous section with sample size of 4,084. The combined set of X and Z covariates includes the variables presented in Table 6.3 and used to estimate the propensity scores.

TABLE 7.1

Results from a linear regression / probit analysis. Results for the probit model are reported as marginal changes *dF/dx*.* *t*-values in parentheses.

	YP – Non (1)	LMT – Non (2)	YP – LMT (3)
		OLS Regression	
Earnings one year after program start (SEK)	**-10,350** **(-4.43)**	**-23,830** **(-8.03)**	**13,480** **(4.43)**
Earnings two years after program start (SEK)	90 (0.03)	**-11,680** **(-3.05)**	**11,770** **(2.94)**
		Probit	
Employment within 12 months after program start (percentage points)	-0.03 (-1.89)	**-0.10** **(-4.29)**	**0.07** **(2.77)**
Employment within 24 months after program start (percentage points)	**0.04** **(2.32)**	-0.00 (-0.09)	0.05 (1.87)
Studies within 12 months after program start (percentage points)	**-0.02** **(-2.15)**	**-0.05** **(-4.04)**	**0.04** **(2.53)**
Studies within 24 months after program start (percentage points)	-0.01 (-0.88)	**-0.03** **(-2.11)**	0.02 (1.44)

Note: **Bold type** indicates statistical significance at the 5 % level. Other explanatory variables included in the model are gender, age, age^2, citizenship, regional characteristics, education, specific education, experience, pre-program labour market status and local employment office variables (see e.g. Table 6.3). * The marginal change is defined as a change in probability due to a one-unit change in the covariate, dProb(E=1)/dx or dProb(S=1)/dx. Thus, -0.01 on the last row in column (1) should be interpreted as follows: A change in the dummy variable for youth practice from 0 to 1 implies a 1 percentage point decrease in the probability of studies within 24 months after program start.

Comparison of the results in Table 7.1 with results in Table 6.5 shows that, in this specific case, OLS and probit on the one hand, and matching on the other produce fairly similar estimates of the average treatment effects on the population. One considerable difference compared to the results obtained by matching, however, is the improvement in the employment effects of youth practice. Table 7.1 above reports a practically zero short-term effect and a significantly positive effect in the long run, whereas the effects obtained from the matching framework are clearly more negative. Consequently, the difference between the employment effects of youth practice and labour market training is more obvious in Table 7.1. Moreover, the long-term earnings effect of labour market training is estimated significantly negative by OLS, whereas matching obtains an effect. The other estimated effects are approximately the same as in the previous section.

7.2 Polychotomous selectivity model

The model presented by Lee (1983) is in particular designed for dealing with selectivity bias in the polychotomous case when the dependent variable is continuous. The idea with this approach is largely the same as in the approach introduced by Dubin and McFadden (1984), which in turn is a multinomial generalisation of Heckman's two-stage method.[30] Like all these selectivity models, the Lee model is designed to adjust for both observed and unobserved selection bias. Thus, it does not require the conditional independence assumption to be valid. However, it rests on other strong assumptions, among them linearity in the outcome variable and joint normality in the error terms.

Let us now assume that the dependence between the treatment dummies and the error term in equation (7.1) is due to a dependence between the error terms e_i and u_i. Thus, simply controlling for observable characteristics is not sufficient to eliminate the selection bias. However, if the error terms are assumed to be jointly normally distributed the treatment effects can be identified and estimated by the following procedure suggested by Lee (1983).

[30] The main shortcoming of the Lee approach compared to the one presented by Dubin and McFadden is that it contains relatively restrictive assumptions on the covariance between the error terms e and u. For a discussion and comparison of the two approaches, see e.g. Schmertmann (1994).

As in the matching framework, the probability of choosing alternative j is assumed to be based on a multinomial logit as follows

$$P_{ji} = \Pr(D_i = j) = \frac{\exp(Z_i \eta_j)}{\sum_{l=1}^{L} \exp(Z_i \eta_l)}, \tag{7.11}$$

where j indexes the choice and i indexes the individual. As previously, the choice alternatives are continued unemployment, youth practice and labour market training. Thus, $j = 0, 1, 2$ and $L = 2$. First, equation (7.11) is estimated by maximum likelihood. Retain coefficients (η_j), covariance matrix of these estimates (Σ) and the full set of predicted probabilities (P_{ji}) for $D_i = \{0, 1, 2\}$. Then, calculate

$$H_{ji} = \Phi^{-1}(P_{ji}), \tag{7.12}$$

$$\lambda_{ji} = \frac{\phi(H_{ji})}{\Phi(H_{ji})}. \tag{7.13}$$

The second step then is to estimate the modified outcome equation (7.1) with adjustment for selection. To be precise, earnings are estimated with the following equation by using OLS:

$$Y_i = X_i \beta + D_{1i}\alpha_1 + D_{2i}\alpha_2 + \lambda_{1i} D_{1i}(\rho_1 \sigma_u) + \lambda_{2i} D_{2i}(\rho_2 \sigma_u) + v_i . \tag{7.14}$$

Statistically significant estimates of $\rho_1 \sigma_u$ and $\rho_2 \sigma_u$ indicate that the selection process in fact is based on some unobserved heterogeneity.

Results from the empirical application are presented in Table 7.2. The sample is again the same as in Section 6, and the multinomial logit model underlying the inverse Mill's ratios is exactly the same as the one used for the estimation of the propensity scores. The set of X variables in the earnings equation (7.14) is limited to gender, age, age^2, citizenship, regional characteristics, education, specific education, experience and pre-program labour market status. The local employment office variables are thus assumed to affect the selection into the programmes but not the earnings.

TABLE 7.2

Results from the estimation of Lee's selectivity model. *t*-values in parentheses.

	Earnings one year after program start	Earnings two years after program start
Youth practice – No participation	-4,310	2,040
	(-0.76)	(0.26)
Labour market training – No participation	-12,940	-21,350
	(-0.86)	(-1.02)
Youth practice – Labour market training	8,640	23,380
	(0.56)	(1.09)
<u>Selection adjustment terms</u>:		
λ_1	-6.340	-2,050
	(-1.14)	(-0.28)
λ_2	-6,870	6,250
	(-0.73)	(0.48)

Note: To calculate the standard errors, I have used White heteroscedasticity robust variance estimator.

The results in Table 7.2 show that including the selection adjustment terms in the equation for earnings produces less negative effects of the programmes. The difference between the effects of youth practice and labour market training diminishes, as well. Somewhat surprisingly, the long-term effect of labour market training is estimated to be worse than in the short run. It is not straightforward, however, to draw conclusions about the existence of unobserved heterogeneity, because the parameter estimates for selection adjustment terms are not statistically significant at the 5 % level. The precision of the estimates of the treatment effects is low, as well. Thus, I do not consider the results in Table 7.2 to be any strong evidence for the existence of unobserved heterogeneity.

8 Sensitivity Analysis

This section examines the robustness of the results presented in Section 6, referred to as the ”main analysis” or the ”main results”. Firstly, the sensitivity of the results to the specification and estimation of the propensities is investigated. Then, various sources of heterogeneity are explored, namely variation between the sexes and the cohorts of program participants, as well as between various types of labour market training. Finally, the definition of the outcome variables is changed in order to examine whether the negative program effects are simply a result of decreased search activity during the program instead of some real human capital deteriorating impact of the programmes.

8.1 Binomial propensity as the matching criterion

The multinomial logit model requires the Independence of Irrelevant Alternatives (IIA) assumption to hold, i.e. that inclusion of new alternatives – or exclusion of some of the existing alternatives – does not alter the relative probability of a choice alternative to another. For example, let us assume that a person initially has three alternatives: no program, program 1 and program 2, with respective probabilities 3/10, 6/10 and 1/10. IIA implies that if program 2 is laid down, the relative probability of program 1 to no program, 6/3 = 2, must remain the same such that the new probabilities of no program and program 1 are 1/3 and 2/3. However, if the programmes are at least partly substitutes to each other, the new probabilities may be expected to be nearer to 3/10 and 7/10.

As described in Section 2, the target groups of youth practice and labour market training were partially the same. By the end of 1992 youth practice had become a massive program that extended over a very large and also heterogeneous group of participants. Concurrently with the expansion of youth practice the number of participants in labour market training diminished. Hence, there might be reason to question the validity of IIA assumption in this context. In order to analyse the sensitivity of the results obtained from the multinomial logit model and presented in Section 6 I have applied a matching procedure based on propensities obtained from binomial logit models. If the estimated coefficients in the binomial model are similar to the coefficients in the multinomial model the IIA assumption may be considered as valid.[31] In general, the advantage of binomial logit compared to its multinomial counterpart is that it does not require validity of the IIA assumption. Nevertheless, a binomial model does not pay any regard to other alternatives and may thus be misleading in case there are several programmes to choose among.[32] Henceforth, the analysis presented in

[31] To be more exact, one could apply the Hausman test to check whether the estimated coefficients differ from each other significantly.

[32] Lechner (1999b) evaluates Swiss active labour market programmes, and by applying both multinomial and binomial model specifications finds the results to be relatively sensitive to whether the existence of other programmes is considered in the analysis or not. Besides the approaches presented in this paper, Lechner (1999b) applies a third possible matching procedure based on conditional propensity $P^{l|ml}(X)$ calculated by means of the marginal propensities $[P^m(X), P^l(X)]$ obtained from the multinomial model. Using conditional propensity as matching criteria produces results very similar to the ones obtained by matching on the Mahalanobis distance between the marginal propensities $[P^m(X), P^l(X)]$ as described in Section 6. The changes that actually take place are of same character as when matching on the binomial propensities, presented in this section. Thus, the results are not reported in the paper, but they are accessible from the author by request.

Section 6 is referred to as matching on multinomial propensities, whereas matching on binomial propensities indicates the procedure described below.

Table D1.1 in the Appendix presents the results from the binomial logit estimations. The vector of explanatory variables is identical to the one included in the multinomial model in Section 6. The binomial model is estimated for all three choice combinations: youth practice and no participation, labour market training and no participation, and youth practice and labour market training. The estimated coefficients shown in columns (1) and (4) are very similar to the coefficients in the multinomial model presented in columns (1) and (4) in Table 6.1. Thus, the IIA assumption appears to be sufficiently fulfilled. The matching procedure is based on one-dimensional nearest-match criterion, i.e. each individual in sample m is matched with a comparison in sample l with the same, or nearest probability of treatment m, $P(T = m| X)$. Like before, observations outside the common support are excluded from the samples.[33] The match quality measured by the absolute standardised bias of the samples is in general similar to the match quality presented in Table 6.3.[34]

Table D1.2 presents results for the average treatment effect on the treated. Qualitatively the results do not differ from the main results obtained by matching on multinomial propensities and presented in Table 6.4. However, matching on binomial propensities produces less negative earnings and short-term employment effects of both programmes compared to no participation. The long-term employment effects of the programmes compared to no participation are approximately unaltered, as well as the effects on probability of studies. There are other minor differences between Tables D1.2 and 6.4, as well, but all in all they look very alike.

The average treatment effects are presented in Table D1.3, and they confirm the results described above. The estimated effects of both programmes are less negative than, or the same as the effects obtained by matching on the multinomial propensities. Qualitatively the results remain the same. All in all, the analysis presented in this section does not give reason to doubt the validity of the IIA assumption.

[33] Due to a different distribution of the estimated propensities the sample sizes are slightly different than in Section 6.
[34] The results of the match quality are accessible from the author by request.

8.2 Separate analysis of women and men

In order to examine whether there is some heterogeneity in the treatment effects between women and men, the sample is divided by sex, and the matching procedure from section 6 is applied to analyse the average treatment effects conditional on sex. The results are presented in Tables D2.1 and D2.2 in the Appendix.

In brief, there is considerable heterogeneity between the sexes, which is discovered by comparing the relative treatment effects expressed in percentage. To begin with the average effects on the treated in Table D2.1, the precision of the estimates is in general better for men than women, which might be explained by the larger sample size for men. The earnings effects are similar for both men and women, whereas the effects on both study and employment probability differ significantly between the sexes. Both programmes, but especially labour market training, have more negative short-term effects on employment for men than for women. Consequently, youth practice appears as significantly better in terms of employment probability for men. Concerning the probability of studies, the opposite holds: youth practice seems to be clearly more favourable for women than for men. This is due to the significantly negative effect of youth practice on study probability for men and of labour market training for women. To sum up with, youth practice seems to have been a better choice for women aiming at studies and men with employment as a goal. Concerning post-program earnings, youth practice was a better choice for both sexes.

The effects on population presented in Table D2.2 confirm the conclusions drawn from Table D2.1. Earnings effects of the programmes are approximately the same for both men and women, whereas particularly the short-term effect of labour market training on re-employment probability is significantly more negative for men than women. Finally, participation in youth practice instead of labour market training yields a significantly higher probability of post-program studies for women, whereas the corresponding effect on men is indistinct.

8.3 Time variation

The sample of fully 4,000 individuals consists of persons who registered with the Employment Service during a two years' period of 1992 – 93. Consequently, as shown in Table 5.1, the dates for program start vary much among the persons, as well. In the main analysis presented in section 6 I have not considered the time variation, i.e. the fact that ”one year after program start” may imply early 1993 for one person and early 1995 for another. If the labour demand or the study opportunities vary over the period, the results may be very sensitive to the registration dates of the individuals. For example, let us assume that the state of the labour market was better 1995 than 1993. Comparing the outcome of a person who participated in labour market training during 1992 with a non-participant with a hypothetical program period during 1994 results in negatively biased estimate of the treatment effect. As long as the distributions of the program start dates, or at least the average start dates are approximately the same this is not a problem for the average treatment effect. But if average start dates differ among the samples, as they do in this study, the results may be biased.

Table 5.1 shows that the participants in labour market training started their program first, while the sample of non-participants has the latest average (hypothetical) program start. In order to examine whether the variation in the start dates influences the results, I have carried through separate analyses for persons enrolling into the programmes (and the no program state) during 1992, 1993 and 1994. Given that the business cycle turned into better during the period of the study, one would expect more positive results for both programmes, and also the difference between youth practice and labour market training to shrink.

Tables D3.1 – D3.2 in the Appendix report results for the treatment effects on the treated and on population. The rows denoted by ”Program start 1992” present results for an analysis in which persons who enrolled in the programmes during 1992 are matched with one another according to the Mahalanobis distance matching procedure described in Section 6. Once again, observations outside the common support are deleted from the samples.[35] In sum, controlling for the date of program start seems to matter. For the samples of 1992, there is more heterogeneity in the treatment effects on the treated, as shown by Table D3.1, than there is for the aggregated samples. Especially the effect of youth practice varies considerably among the column (1) and (2), as well as (5) and (6). However, the population effects for the 1992 samples reported

[35] Especially for the samples with start 1994 the common support requirement implies that relatively many observations are deleted.

in Table D3.2 are quite similar to the main results, except for that the difference between youth practice and labour market training is diminished as expected.

For the samples of 1993 and 1994 the change in the results is more obvious, though most of the estimates have relatively low precision. As shown by Table D3.2, for the 1993 sample all the average earnings and employment effects of both programmes are more positive than in the main analysis in Section 6, whereas the effects on study probability are clearly worse. Moreover, all results except for the short-term earnings effects are clearly positive, though statistically insignificant, for the 1994 sample. However, there is much heterogeneity among all the sub-samples, as shown by the Table D3.2.

In sum, some of the negative effects reported by Section 6 may be explained by the variation of the program start dates together with changes in the business cycle. Nevertheless, for the early cohorts of program participants, the negative results seem quite robust. Youth practice does not appear to be as superior to labour market training as presented by the main results; all the estimates in column (3) in Table D3.2 are statistically insignificant. The results presented in this section should, however, be considered as preliminary. More sophisticated modelling of the time variation is subjects for future research.

8.4 Various types of labour market training

Labour market training is a relatively heterogeneous program that consists of courses of various length and content. In broad outline, the courses are divided into vocational and non-vocational. Non-vocational courses are often preparatory in the sense that the participants have already *ex ante* plans to participate in further programmes. Examples of such courses are e.g. Swedish for foreigners. Consequently, participants in these courses are not expected to de-register from the Employment Service as quickly as participants in the vocational courses or other programmes. Thus, the effects of the non-vocational courses on at least future earnings and employment may be worse than the effects of vocational courses.

In the sample of 606 labour market training participants used in this study, 518 observations include information on the course type. Approximately 34 per cent of these individuals participated in a non-vocational course. To see whether this explains the strongly negative effects of the aggregated labour market training, i.e. whether the effects differ between the two types of training, I have applied an analysis where vocational and non-vocational training are treated as separate programmes.[36] In short, matching is based on propensities obtained from a multinomial logit model with four instead of three alternatives. Otherwise the procedure is identical to the one described in Section 6.[37]

The results for the average effects on the treated and the population are presented in Tables D4.1 and D4.2 in the Appendix. On the whole, the difference between the effects of vocational and non-vocational training is relatively small. As expected, the estimated earnings and employment effects of vocational training are higher (i.e. less negative) than, whereas the effects of non-vocational training are lower than the effects of aggregated labour market training reported in Tables 6.4 and 6.5. The average population effects in Table D4.2 indicate that the short-term earnings effect of vocational and non-vocational training compared to non-participation were – 31 % and – 54%, while the aggregated effect reported in Section 6 was – 39 %. The short-term employment effects are of similar size. The short-term study probability, on the contrary, is slightly higher in percentage for participants in non-vocational training. However, all the differences between the two types of training are statistically insignificant. Once again, the long-term effects are all statistically insignificant and near zero.

Moreover, the results for the average treatment effect on the treated in Table D4.1 reveal more heterogeneity between the various groups than what is reported in Table 6.4. The variation is largest for non-vocational training and no participation: the actual earnings and employment probability of the non-participants were approximately 70 % higher, whereas the study probability was almost 200 % higher compared to what they would have been had the persons participated in non-vocational training. The corresponding effects are considerably smaller for the participants in the training.

In sum, the strongly negative effects of labour market training obtained in Section 6 remain robust even when the various types of training are regarded. However,

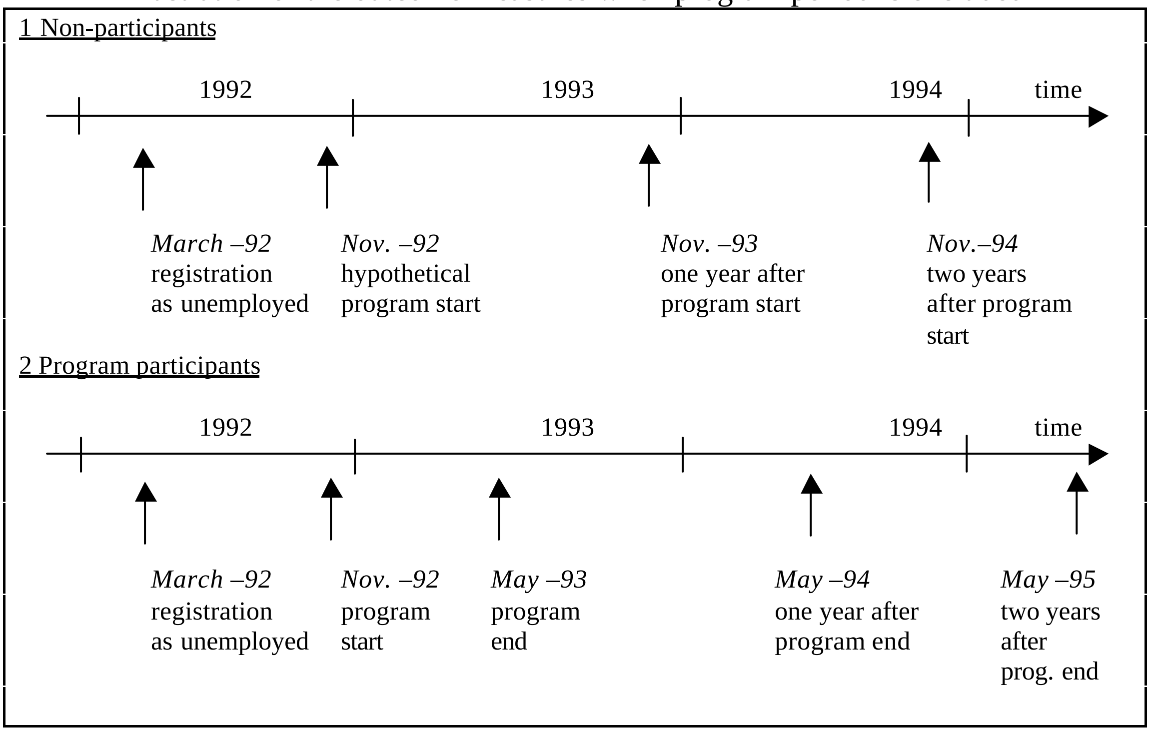
[36] The observations that lack information on course type are excluded from the sample.
[37] The results from the various stages of the matching procedure are accessible from the author by request.

there seems to be considerable heterogeneity in the treatment effects among the individuals implying that the effects are not necessarily quite as negative for everybody.

8.5 Exclusion of the program period

The main analysis in Section 6 is based on the assumption that persons participating in the programmes continued job searching, which was required by the program regulations. However, in practice the search activity may diminish considerably during the participation (For evidence, see e.g. Ackum Agell, 1996, or Edin and Holmlund, 1991). Thus, one could argue for not counting for the program period when defining the outcome variables, as illustrated in Figure 8.1.

FIGURE 8.1
Illustration of the outcome measures when program period is excluded



For the non-participants, the time span ”within one or two years after” starts as before from the hypothetical program start, while for the program participants it starts *from the end of the program* instead. Earnings after the program start are in this analysis defined as follows: For all program participants with program *end*, and for all non-participants with a hypothetical program *start* during e.g. 1992, earnings one year after program *end/start* are the annual sum of earnings for the calendar year 1993. Consequently, one would expect more positive effects of the programmes compared to the state of no participation. Moreover, since in this sample the average participation period in labour market training is shorter than the average period in youth practice (see Table 5.1), one would also expect more positive average effects of youth practice compared to labour market training.

Table D5.1 reports the results of average treatment effects on the treated according to the alternative way of defining outcome measures. Firstly, the significance of the estimates is generally lower than in the main analysis. Secondly, as expected, the earnings and employment effects of the programmes compared to not participating are clearly more positive, whereas there is almost no difference in the effect on the probability of studies. Finally, the superiority of youth practice over labour market training does not decrease to any notable extent, except perhaps for the short-term earnings effect.

The average treatment effects on the population are presented in Table D5.2. Again, the significance of the estimates is decreased considerably from the corresponding results in section 6. Thus, it is difficult to draw any robust conclusions. Nevertheless, the short-term earnings effects of the programmes with respect to not participating are more positive, especially the effect of youth practice. All in all, it seems that the negative effects of youth practice presented in Section 6 may be explained by decreased search activity among the participants during the program. The results in this section do not, however, offer an explanation for the relative large deleterious effects of labour market training on the participants' future earnings, employment and studies opportunities.

9 Conclusions

The main purpose of this study was to evaluate Swedish youth programmes using three various measures of effectiveness: Post-program annual earnings, re-employment probability and probability of regular education. More precisely, the programmes evaluated were youth practice and labour market training, and the age group examined was 20 – 24. Furthermore, another purpose was to compare some well-known methods proposed by the evaluation literature to estimate the average treatment effects.

The main analysis is based on the assumption that the participation in the various treatments, including the no-treatment state, is independent of the post-program outcomes conditional on observable exogenous factors, called the *conditional independence assumption* (CIA). Given that CIA holds, unbiased estimators of the average treatment effects can be obtained by matching on the treatment propensities. The results from the main analysis suggest that both youth practice and labour market training have negative short-term effects on earnings and employment, where ”short-term” refers to one year after the program start. Two years after the program start, however, the effects are no longer as obvious; most of the estimates for employment and earnings are statistically insignificant at the 5 % level. Concerning the third measure of effectiveness, the probability of regular education, the results show no significant effects of youth practice, whereas labour market training may have had a negative effect at least in the short run. Finally, comparison of the two programmes suggests that youth practice was better – or less harmful – than labour market training.

How robust are these results? Firstly, matching on the conditional propensities – either obtained from binomial logit estimations or calculated by means of the marginal propensities from the multinomial model – yield very similar results as those described above. Secondly, the results from standard OLS regression and probit analyses applied on the same data are almost identical, as well, except maybe for the fact that the employment effects of youth practice are zero in the short run and slightly positive in the long run. Results from the polychotomous selectivity model are not considered to undermine the main results, either. However, the sensitivity analysis suggests some factors that might explain the in part strongly negative effects obtained by the main analysis. Moreover, the effects seem to be heterogeneous for various types of individuals.

To begin with heterogeneity in the treatment effects, plotting the pair-wise differences between a treated and a comparison person against the treatment propensity suggests that the earnings and employment effects of the programmes vary considerably among the individuals. For the most part, the effects vary as much for all values of estimated propensity. The employment effect of labour market training, however, seems to be more positive the higher the estimated probability of being assigned to training, whereas for youth practice, the opposite seems to be the case. Furthermore, separate analyses of the sexes suggest that, in general, the programmes have a more negative effect on men than on women. Comparison of the two programmes reveals even more heterogeneity between the sexes. In short, youth practice seems to have been a better choice for women aiming at higher earnings or regular education and men with employment or higher earnings as a goal. Finally, the effect of labour training on earnings and employment seems to have been somewhat less negative for those who took a vocational course than for participants in non-vocational courses of a more preparatory nature.

A preliminary attempt to control for variation in the program entry date suggests an additional source of heterogeneity. Separate analyses of the persons enrolling into the programmes during 1992, 1993, and 1994 are carried through in order to control for possible business cycle effects. The results show that the entry date does matter; relatively many statistically significant negative estimates become either insignificant or even significant positive. This holds mainly for the latter cohorts of 1993 and 1994, while the results for the 1992 sample do not remarkably differ from the previously presented results. However, more sophisticated modelling of the time variation is subjects for future research.

Hence, the results from the sensitivity and heterogeneity analysis suggest that in the total sample of fully 4,000 individuals there are sub-samples for which the effects are not as negative as they are on average for all. Moreover, a plausible explanation for the negative or non-existing earnings and employment effects of youth practice, provided by the sensitivity analysis, is that the participants put less or no effort on finding a job during the program, despite of the program regulations that required active job search even during participation.

All in all, neither of the youth programmes seem to work in the sense they are supposed to. In an international perspective this is not a surprising result; A survey on existing evaluation studies by Martin (1998) shows that most of the OECD countries have failed in active labour market programmes for the youth. The previous Swedish studies on labour market training have not found any positive effects either. If this is the case, what is the explanation for the non-existing or negative effects?

The results concerning youth practice might be explained by insufficient planning and follow-up, as well as low-qualified tasks that did not provide any human capital accumulation. The implementation studies of youth practice by Hallström (1994) and Schröder (1994, 1995) reveal that this was the case for many participants. Moreover, the results from the analysis of time variation may suggest that these problems were more severe when the program was relatively new. Given that the search activity was very low during the program participation, it seems more or less expected that the effect turned out not positive.

To explain the negative results for labour market training require, however, more than what is suggested for youth practice. The program has existed for decades, and thus ”start-problems” do not provide an explanation. Furthermore, exclusion of the program period does still produce significantly negative effects. One potential explanation is that the courses do not fit the employers requirements for labour, and that training thus has both professional and regional “lock-in” effects on the participants.

What is the policy conclusion drawn from these results? In order to find the answer, the interpretation of the non-treatment state described in Section 5 ought to be recalled. Due to the institutional settings in Sweden the group of non-participants collected from the database *does not* represent a world without active labour market programme; when deciding not to participate they know that they can potentially enter the programmes at a later stage. Thus, *it is wrong to draw the conclusion that the participants would have been better off had there been no programmes at all*. Instead, the results may suggest that it is better to wait than to participate at an early stage. Moreover, the results also suggest that workplace practice is more effective than pure training.

Finally, is it likely that the observable characteristics in the data do not provide all information needed to explain the selection into the programmes? Even though the results from the selectivity model in Section 7 do clearly indicate that, the possibility may not be totally ignored.[38] Are there some important variables omitted from the analysis?

[38] Identification of the average treatment effects rely on different assumptions in the matching approach and the selectivity model, and thus they cannot be used as formal tests for each other. The fact that the selectivity model does not suggest unobserved heterogeneity may as well depend on that the structural restrictions of the model are incorrect.

The data applied in this study provides information on the persons' annual earnings before registration with the Employment Service and the pre-program unemployment history after the first visit at the local employment office. Moreover, the level of formal education, as well as the individual's own judgement on how well his or her education and previous work experience fit the type of the job applied is included in the estimation of the propensities. Whether the person chooses to fill in the search form or not is also controlled for in order to have a further measure for "motivation" or other personal characteristics that may affect the selection into the programmes. However, more detailed data on the activities before registration with Employment Service could provide relevant information on what determines the selection into the various states and the outcome variables. Moreover, whether or not the person was entitled to unemployment insurance or not, as well as more qualitative measures of the human capital – e.g. the marks in the school-leaving certificate – might reveal heterogeneity among the various groups that is not controlled for in this study.

References

Ackum, S (1991), Youth Unemployment, Labor Market Programs and Subsequent Earnings, *The Scandinavian Journal of Economics, 93(4)*, 531 – 541.

Ackum Agell, S (1996), Arbetslösas sökaktivitet, FIEF Reprint Series No. 106, reprinted from Aktiv Arbetsmarknadspolitik, SOU 1996:34.

Angrist, J (1998), Estimating the Labor Market Impact of Voluntary Military Service Using Social Security Data on Military Applicants, *Econometrica 66(2)*, 249 – 288.

Angrist, J, G Imbens, and D Rubin (1996), Causal Effects and Instrumental Variables, *Journal of the American Statistical Association*, 91, 444 – 455.

Dubin, J and D McFadden (1984), An econometric analysis of residential electrical appliance holdings and consumption, *Econometrica* (52), 345 – 362.

Edin, P-A, A Forslund and B Holmlund (1998*), The Swedish Youth Labor Market in Boom and Depression*, Department of Economics working paper, Uppsala University.

Edin, P-A and B Holmlund (1991), Unemployment, Vacancies and Labour Market Programmes: Swedish Evidence, in Padoa-Schioppa, F. (Ed.), Mismatch and Labour Mobility, Cambridge University Press, Cambridge.

Heckman, J (1979). Sample selection bias as a specification error, *Econometrica* 47(1), 153 – 161.

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

Heckman, J, R J LaLonde and J A Smith (1998), The Economics and Econometrics of Active Labor Market Programmes, in Aschenfelter O and D Card, eds., *Handbook of Labor Economics*, Volume III.

Heckman, J and R Robb (1985), Alternative methods for evaluating the impact of interventions, in J. Heckman and B. Singer, eds., *Longitudinal analysis of labor market data*. Cambridge University Press, Cambridge.

Heckman, J and J Smith (1995), Assessing the case for social experiments, *Journal of Economic Perspective 9(2)*, 85 – 110.

Holland, P W (1986), Statistics and Causal Inference, *Journal of the American Statistical Association*, 81, 945 – 970, with discussion.

Imbens, G (1999), The Role of Propensity Score in Estimating Dose-Response Functions, *NBER working paper TO237.*

Korpi, T (1994), Escaping Unemployment. Studies in the Individual Consequences of Unemployment and Labor Market Policy, Ph.D. Thesis, Swedish Institute for Social Research, 24.

Layard, R, S Nickell and R Jackman (1991), *Unemployment, macroeconomic performance and the labor market*. Oxford University Press, Oxford, UK.

Lechner, M (1999a), Earnings and Employment Effects of Continuous Off-the-Job Training in East Germany After Unification, *Journal of Business & Economic Statistics*, 17, 74 – 90.

– (1999b), Identification and estimation of causal effects of multiple treatments under the conditional independence assumption, *Discussion paper 9908*, University of St. Gallen.

Lee, L F (1983), Generalized econometric models with selectivity, *Econometrica* 51, 507 – 512.

Martin, J P (1998), What works among active labour market policies: Evidence from OECD countries' experiences, *Labour market and social policy – occational papers* no. 35, OECD, Paris.

Regnér, H (1997), *Training at the Job and Training for a New Job: Two Swedish Studies.* Ph.D. Thesis, Swedish Institute for Social Research, 29.

Rosenbaum, P and D Rubin (1983), The Central Role of the Propensity Score in Observational Studies for Causal Effects, *Biometrika 70*, 41 – 55.

– (1984), Reducing Bias in Observational Studies Using Subclassification on the Propensity Score, *Journal of the American Statistical Association* 79, 516 – 524.

– (1985), Constructing a Control Group Using Multivariate Matched Sampling Methods that Incorporate the Propensity Score, *The American Statistician 39*, 33 – 38.

Rubin, D (1973), The use of Matched Sampling and Regression Adjustment to Remove Bias in Observational Studies, *Biometrics 29*, 185 – 203.

– (1974), Estimating Causal Effects of Treatments in Randomized and Nonrandomized Studies, *Journal of Educational Psychology*, 66, 688 – 701.

– (1977), Assignment to Treatment Group on the Basis of a Covariate, *Journal of Educational Statistics*, 2, 1 – 26.

Schmertmann, C (1994), Selectivity bias correction methods in polychotomous sample selection models, *Journal of Econometrics* 60, 101 –132

Schröder L (1994) Kompetenshöjning och substitution – arbetsmarknadspolitiska åtgärder för arbetslösa ungdomar i Sverige 1934–1993, *TemaNord* 1994:557.

Schröder L (1995), Ungdomars etablering på arbetsmarknaden – från femtiotalet till nit-tiotalet, EFA rapport 38, Arbetsmarknadsdepartementet

Schröder, L (1996), *Programmes for unemployed youth Sweden*. Unpublished Manuscript, Swedish Institute for Social research.

Sehlstedt, K and L Schröder (1989), *Språngbräda till arbete? En utvärdering av beredskaps-arbeten, rekryteringstöd och ungdomsarbete*. EFA, Dept. of Labour, Stockholm.

Appendix A: Matching algorithm

The matching algorithm applied in this paper is similar to Lechner (1999b). Thus, for the estimators and their covariance matrixes, the reader is asked to turn to that paper.

Matching algorithm

1. Collect the participant samples and the largest possible sample of non-participants, and randomly assign the program start dates for the non-participants from the distribution of the participants (by month). Eliminate all the non-participants who are assigned a date after their actual de-registration from open unemployment.

2. Specify and estimate a multinomial discrete choice model to obtain the (estimated) propensities $P(T = 0|X)$, $P(T = 1|X)$, $P(T = 2|X)$. Tests for omitted variables in a binomial framework. Compute the conditional probabilities $P^{m|ml}(X)$.

3. Common support: Eliminate all observations outside the defined common support.

4. Apply the following procedure to match each observation in group $T = m$ with an observation in the comparison group $T = l$:

   (i) Choose an observation from the group *m*, and remove it from that pool.
   (ii) Find an observation in the group *l* that is as close as possible to the one collected in step (i) in terms of the predicted probabilities. The distance can be measured by a Mahalanobis distance metric. Alternatively, base the closeness on the conditional probability $P^{m|ml}(X)$. Do not remove that observation so that it can be used again.
   (iii) Repeat (i) and (ii) so that there is no observation left in the group *m*.
   (iv) Repeat (i) – (iii) for all combinations of *m* and *l*.

5. Test for balance of the covariates. In case that the covariates are not balanced, refine the specification of the discrete choice model, and go through steps 2 – 4 again.

6. Use the comparison groups formed in 4(iv) to compute the respective conditional expectations by the sample mean. Note that the same observation may appear several times in the sample.

7. Compute the estimates of the treatment effects using the results of step 6, and compute their covariance matrix.

Appendix B: Estimated propensities

Figures B1 – B3 present the distributions of the estimated propensities to be assigned into the three states. First, the density of each propensity is estimated by a Epanechnikov kernel with bandwidth 0.02. The y-axis is then by approximation transformed into frequencies. The vertical lines in the Figures indicate the common support restriction $0 < \Pr(T = t \mid X) < 1 \;\forall\; t, X$. Observations to the left of the left-hand vertical line or to the right of the right-hand vertical line are excluded from the sample.

FIGURE B1

Estimates for propensity to not participate

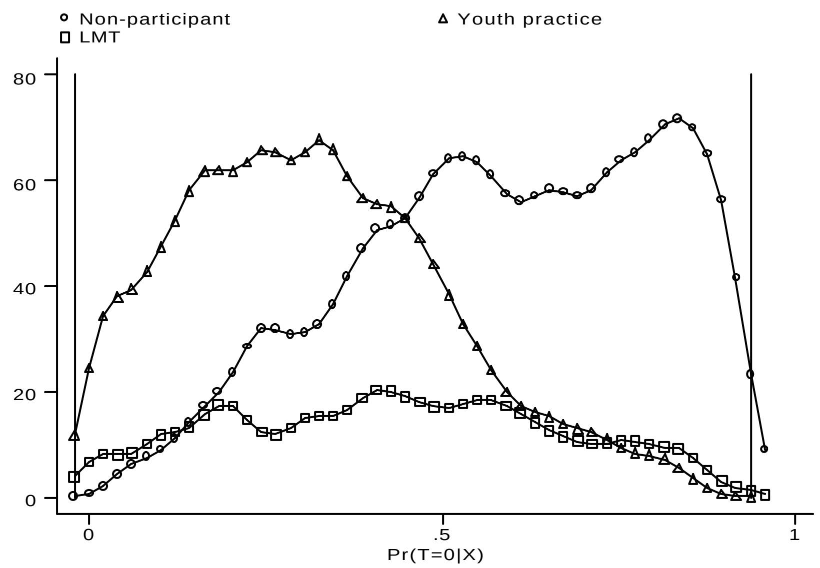


FIGURE B2

Estimates for propensity to participate in youth practice

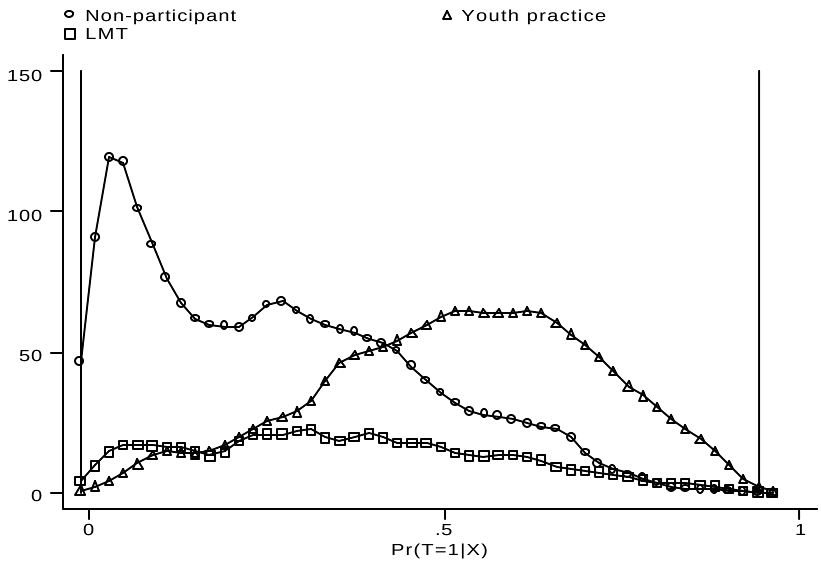
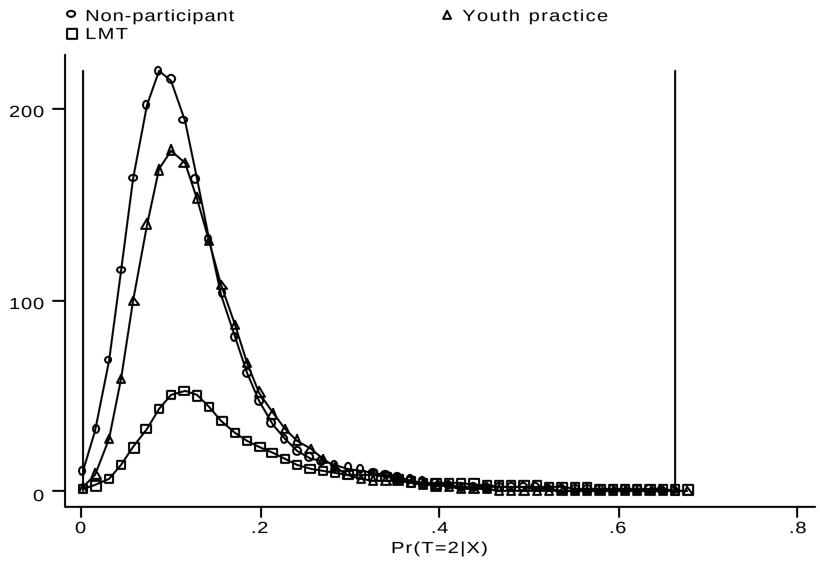


FIGURE B3

Estimates for propensity to participate in labour market training

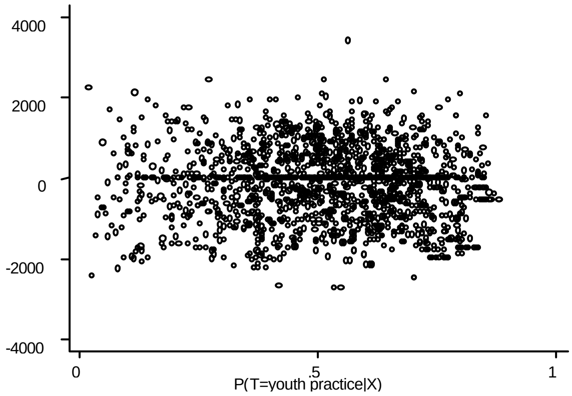


Appendix C: Heterogeneity

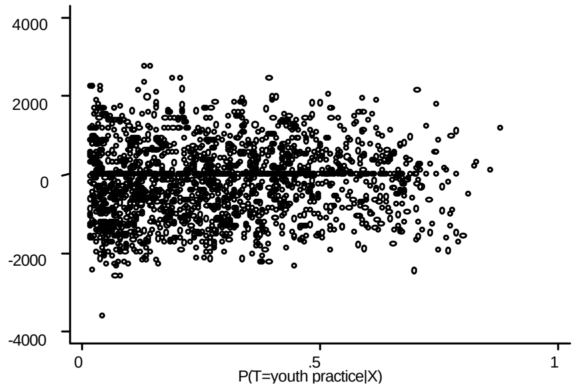
C1 Youth practice compared to non-participation

C1.1 Earnings effects of youth practice compared to non-participation (difference in SEK 100)

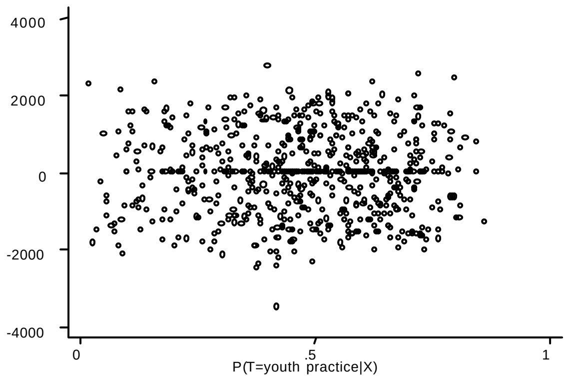
(i) on participants in youth practice (1 year after)



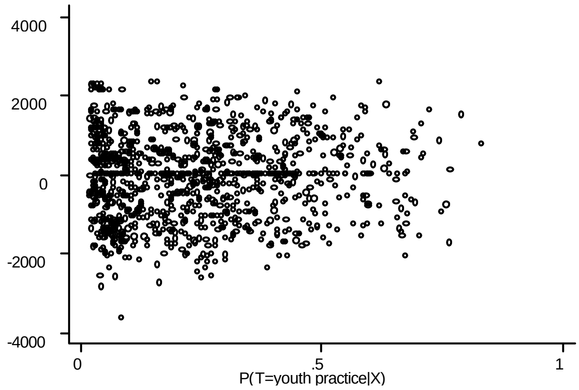
(ii) on non-participants (1 year after)



(iii) on participants in youth practice (2 years after)

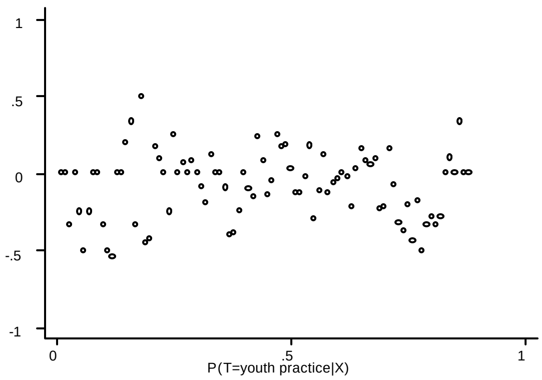


(iv) on non-participants (2 years after)

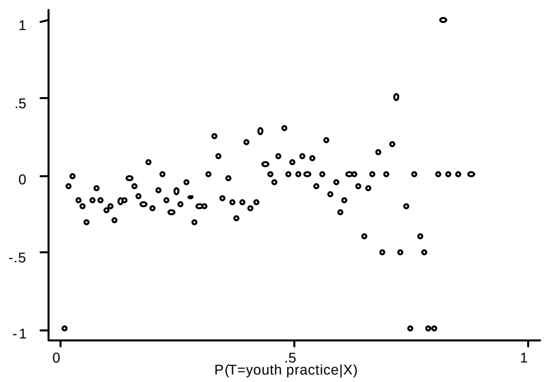


C1.2 Employment effects of youth practice compared to non-participation

(i) on participants in youth practice (within 12 months)

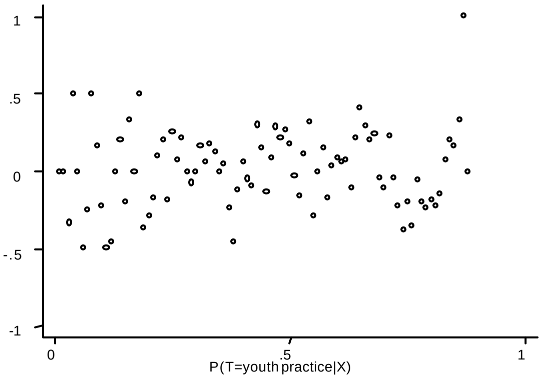


(ii) on non-participants (within 12 months)

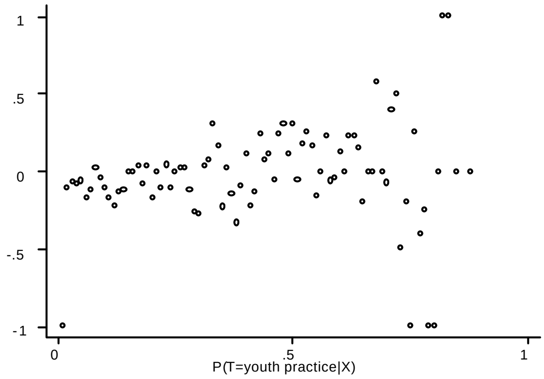


Heterogeneity, cont.

(iii) on participants in youth practice (within 24 months)

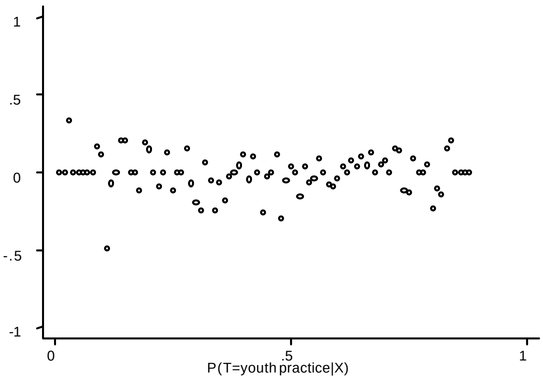


(iv) on non-participants (within 24 months)

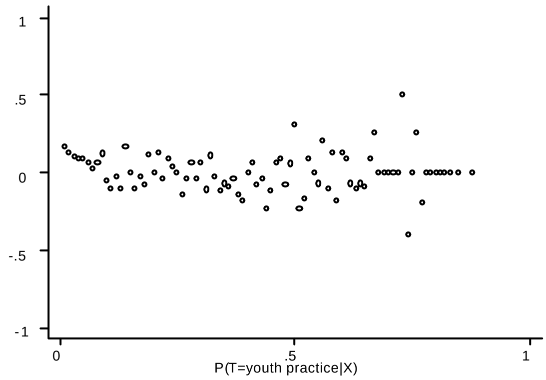


C1.3 Study effects of youth practice compared to non-participation

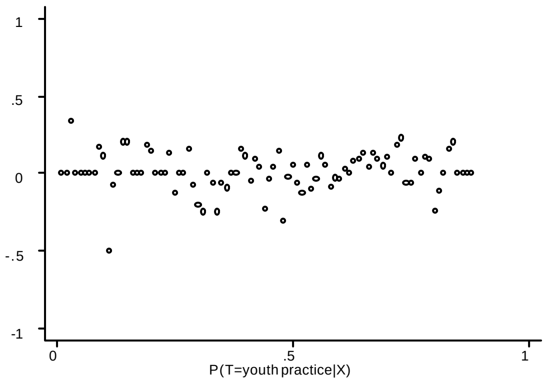
(i) on participants in youth practice (within 12 months)



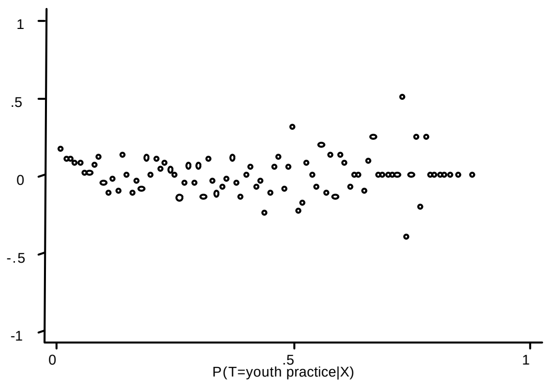
(ii) on non-participants (within 12 months)



(iii) on participants in youth practice (within 24 months)



(iv) on non-participants (within 24 months)

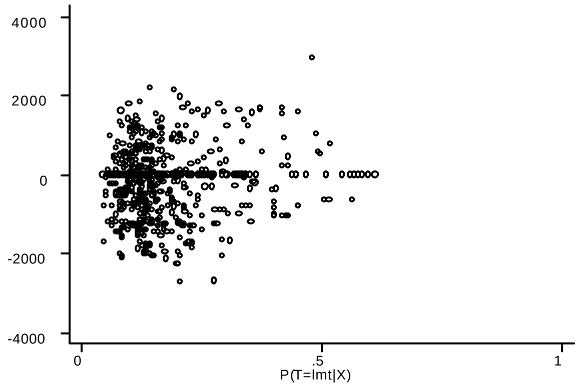


Heterogeneity, cont.

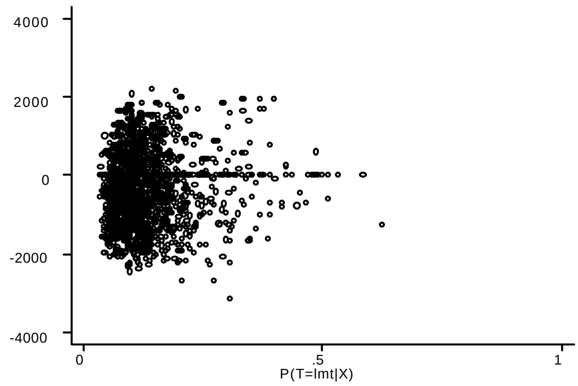
C2 Labour market training compared to non-participation

C2.1 Earnings effects of labour market training compared to non-participation (SEK 100)

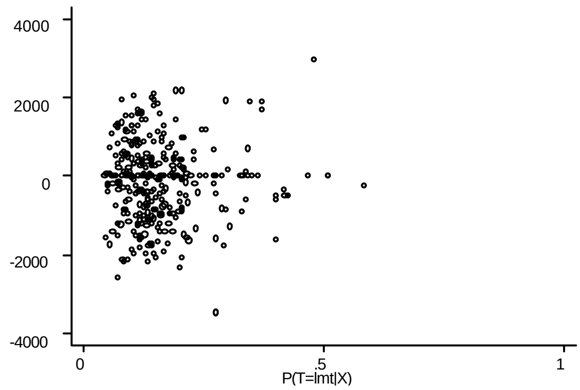
(i) on participants in labour market training (1 year after)



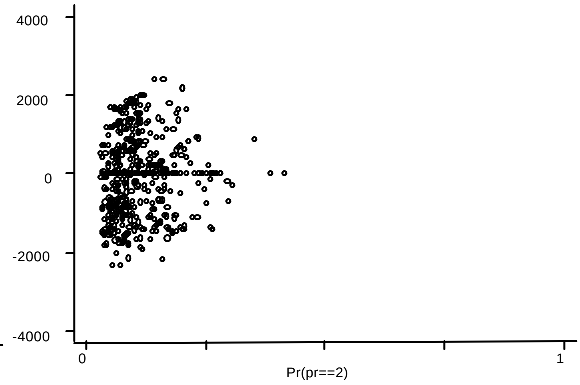
(ii) on non-participants (1 year after)



(iii) on participants in lmt (2 years after)

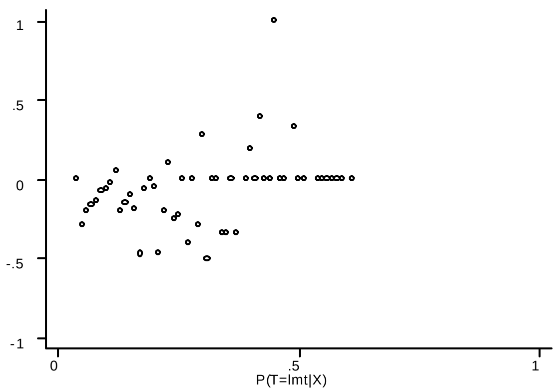


(iv) on non-participants (2 years after)

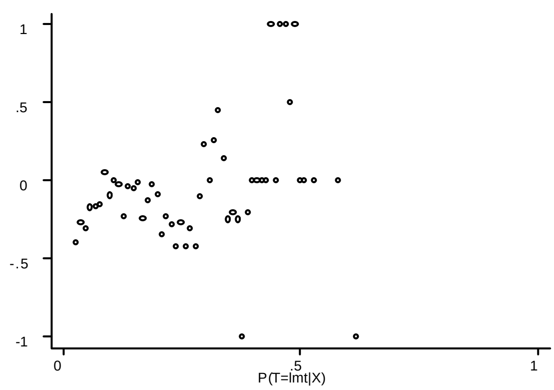


C2.2 Employment effects of labour market training compared to non-participation

(i) on participants in lmt (within 12 months)

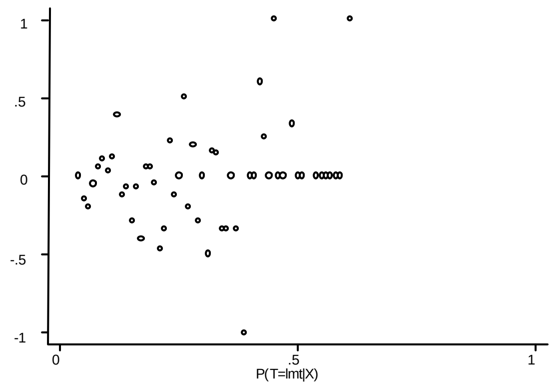


(ii) on non-participants (within 12 months)

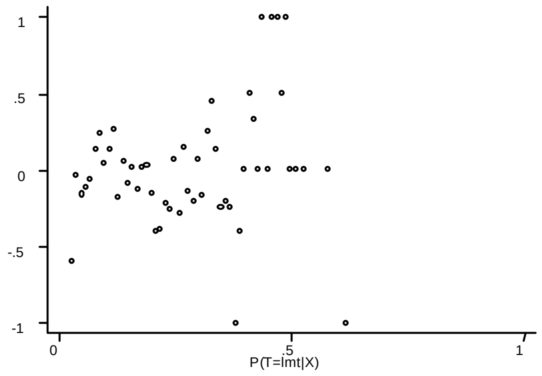


Heterogeneity, cont.

(iii) on participants in lmt (within 24 months)

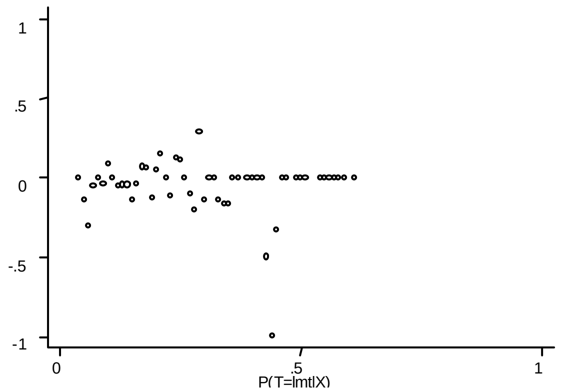


(iv) on non-participants (within 24 months)

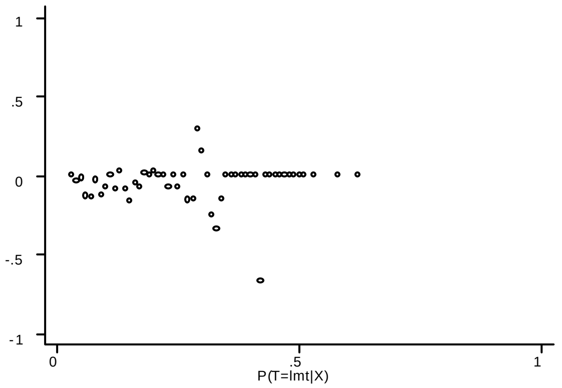


C2.3 Study effects of labour market training compared to non-participation

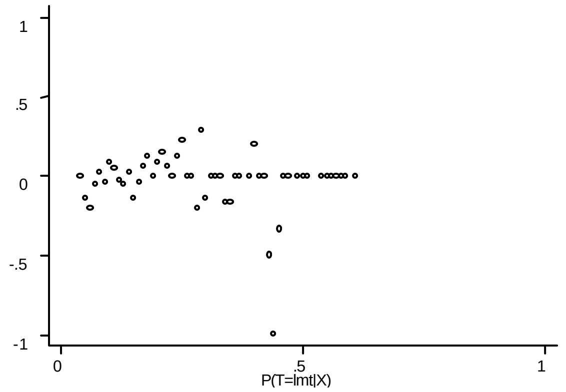
(i) on participants in lmt (within 12 months)



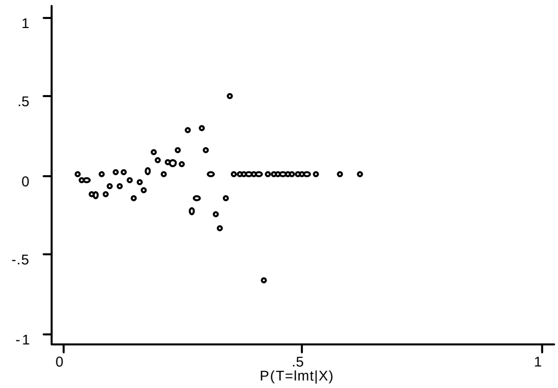
(ii) on non-participants (within 12 months)



(iii) on participants in lmt (within 24 months)



(iv) on non-participants (within 24 months)

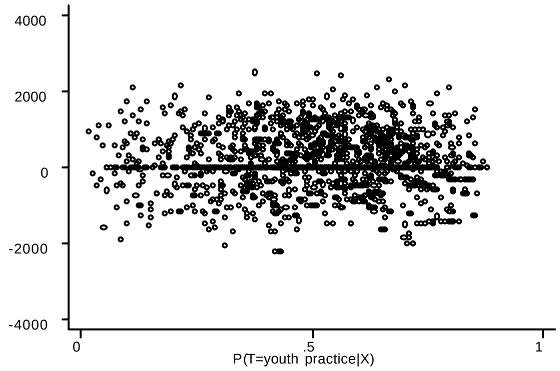


Heterogeneity, cont.

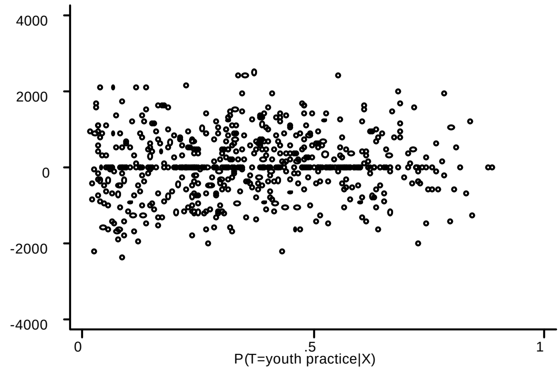
C3 Youth practice compared to labour market training

C3.1 Earnings effects of youth practice compared to labour market training (SEK 100)

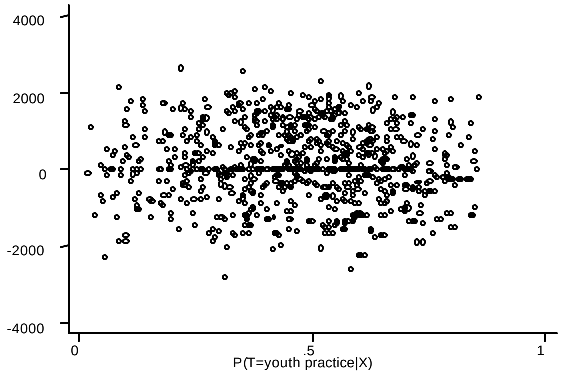
(i) on participants in youth practice (1 year after)



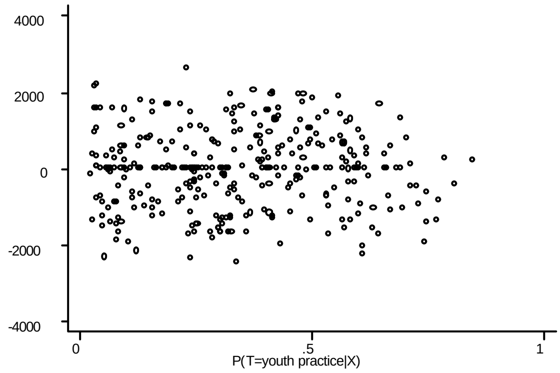
(ii) on participants in lmt (1 year after)



(iii) on participants in youth practice (2 years after)

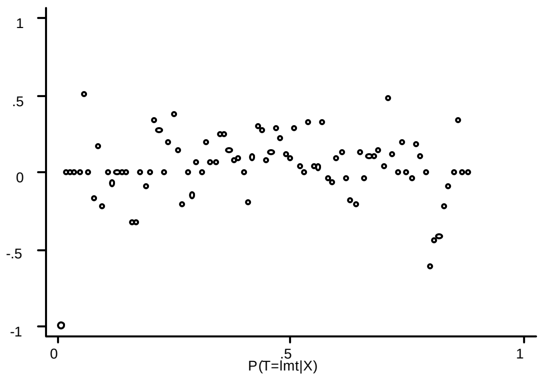


(iv) on participants in lmt (2 years)

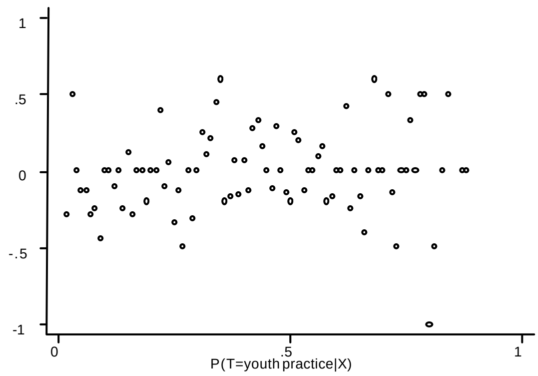


C3.2 Employment effects of youth practice compared to labour market training

(i) on participants in youth practice (within 12 months)

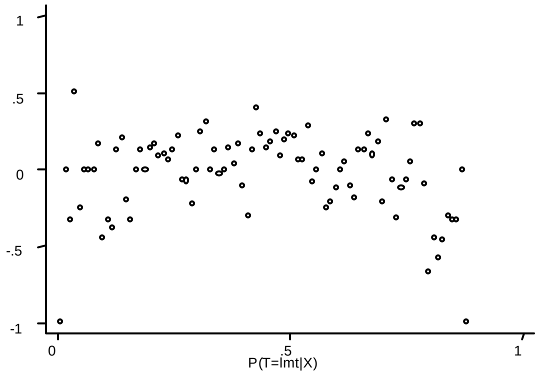


(ii) on participants in lmt (12 months)

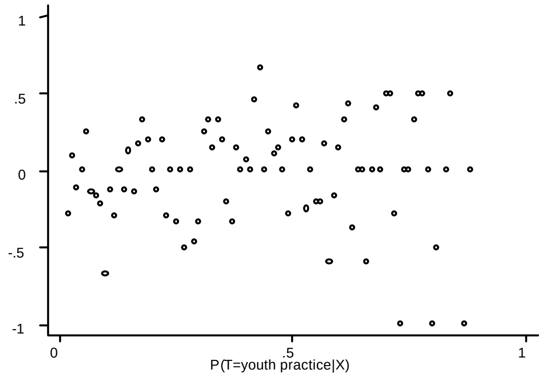


Heterogeneity, cont.

(iii) on participants in youth practice (within 24 months)

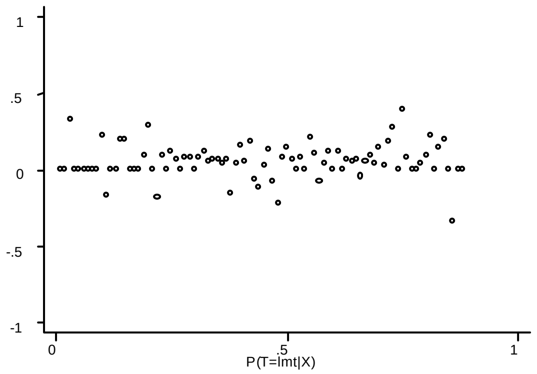


(iv) on participants in lmt (24 months)

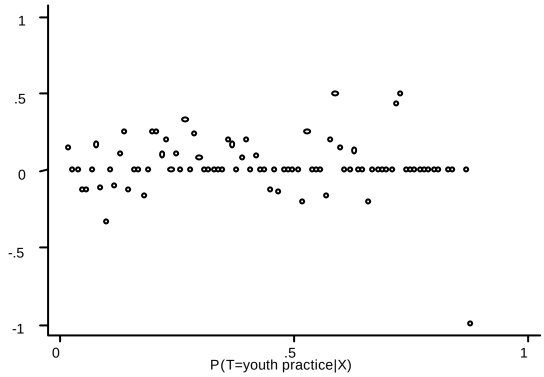


<u>C3.3 Study effects of youth practice compared to labour market training</u>

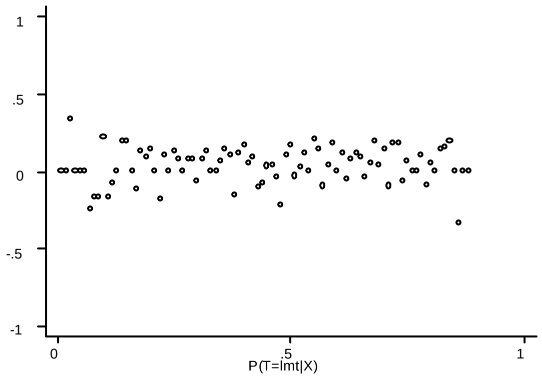
(i) on participants in youth practice (within 12 months)



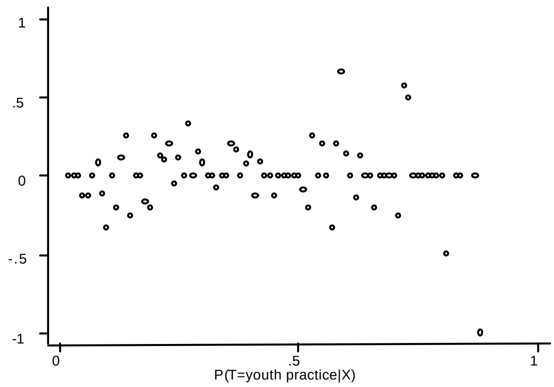
(ii) on participants in lmt (within 12 months)



(iii) on participants in youth practice (within 24 months)



(iv) on participants in lmt (24 months)



Appendix D: Sensitivity Analysis

D1 Binomial propensity as a matching criterion

TABLE D1.1

Results from the binomial logit estimations

	YP – Non			LMT – Non			YP – LMT		
	Coeff. (1)	Std.e. (2)	Odds r. (3)	Coeff. (4)	Std.e. (5)	Odds r. (6)	Coeff. (7)	Std.e. (8)	Odds r. (9)
Constant	-55.3	8.7	–	-4.31	10.0	–	**-73.4**	**11.4**	**–**
Personal char.:									
Female	0.15	0.08	1.16	-0.17	0.11	0.84	**0.31**	**0.11**	**1.36**
Age	**5.36**	**0.79**	**213**	0.37	0.89	1.44	**7.07**	**1.03**	**1175**
Age^2	**-0.13**	**0.02**	**0.88**	-0.01	0.02	0.99	**-0.17**	**0.02**	**0.85**
Non-Nordic	**0.38**	**1.99**	**1.46**	**1.08**	**0.14**	**2.93**	**-1.02**	**0.19**	**0.36**
Regional char.:									
Forest county	-0.14	0.11	0.87	**0.38**	**0.14**	**1.46**	**-0.51**	**0.14**	**0.60**
City county	**-0.64**	**0.10**	**0.53**	-0.08	0.12	0.92	**-0.48**	**0.13**	**0.62**
Education[1]:									
High School 1-2 years	**0.34**	**0.14**	**1.40**	-0.12	0.16	0.89	**0.38**	**0.17**	**1.46**
High School 3-4 years	**0.28**	**0.14**	**1.33**	-0.08	0.16	0.93	0.22	0.17	1.25
University	0.25	0.19	1.28	**-0.54**	**0.16**	**0.58**	**0.63**	**0.25**	**1.88**
Specific education[2]:									
Yes	**-0.30**	**0.10**	**0.74**	-0.17	0.12	0.85	-0.11	0.12	0.90
Missing	-0.18	0.14	0.83	0.02	0.18	1.02	-0.30	0.18	0.74
Experience[2]:									
Some	-0.09	0.11	0.91	-0.01	0.14	0.99	-0.20	0.14	0.82
Good	**-0.35**	**0.13**	**0.71**	**-0.39**	**0.16**	**0.68**	-0.08	0.17	0.92
Pre-program labour market status:									
Pre-program unemployment (days)	**0.01**	**0.00**	**1.01**	**0.01**	**0.00**	**1.01**	**0.00**	**0.00**	**1.00**
Earnings 1 year before registration with ESR (10,000 SEK)	-0.04	0.01	0.96	0.00	0.00	1.00	**-0.05**	**0.01**	**0.95**
Local employment office variables[3]:									
Share of program part. of all unemployed	**2.04**	**0.49**	**7.66**	**2.21**	**0.59**	**9.10**	-0.56	0.65	0.57
YP of all program participants	**0.77**	**0.38**	**2.16**	0.49	0.47	1.64	0.34	0.54	1.41
LMT of all program participants	-0.08	0.50	0.92	**1.16**	**0.58**	**3.18**	-1.09	0.65	0.34
Missing	**0.88**	**0.22**	**2.40**	**0.76**	**0.25**	**2.14**	-0.12	0.22	0.88
No. of observations	3,681			2,630			2,263		
Log likelihood	-1,902			-1266			-1,153		
LR chi2 (38)	1262.74			308,11			324.36		
Pseudo R2	0.2492			0,1085			0.1233		

TABLE D1.2

Binomial logit: Results for the average treatment effect on the treated q_0^{ml}.

t-values in parentheses, *relative effects in italics*.

	YP – Non (1)	Non – YP (2)	LMT–Non (3)	Non–LMT (4)	YP–LMT (5)	LMT–YP (6)
Earnings one year after program start (SEK)	**-10,710** **(- 2.68)** ***-17 %***	**13,630** **(3.04)** ***23 %***	**-16,060** **(-3.51)** ***-26 %***	**24,390** **(5.71)** ***50 %***	**16,100** **(3.81)** ***45 %***	**-14,340** **(-3.26)** ***-25 %***
Earnings two years after program start (SEK)	-138 (-0.02) *-0 %*	-2,680 (-0.38) *-3 %*	-6,580 (-1.05) *-9 %*	**15,950** **(2.22)** ***22 %***	12,110 (1.45) *19%*	-10,070 (-1.60) *-13 %*
Employment within 12 months after program start (percentage points)	-0.02 (-0.71) *-6 %*	**0.10** **(2.86)** ***35 %***	**-0.08** **(-2.53)** ***-24 %***	**0.09** **(2.91)** ***34 %***	**0.08** **(2.28)** ***35 %***	-0.05 (-1.59) *-18 %*
Employment within 24 months after program start (percentage points)	0.05 (1.78) *14 %*	0.02 (0.68) *6 %*	0.02 (0.57) *5 %*	-0.04 (-1.14) *-9 %*	0.00 (0.00) *0 %*	-0.03 (-0.81) *-7 %*
Studies within 12 months after program start (percentage points)	-0.02 (-0.89) *-15 %*	0.00 (0.18) *3 %*	**-0.06** **(-2.83)** ***-49 %***	**0.05** **(3.02)** ***81 %***	0.03 (1.49) *42 %*	-0.04 (-1.81) *-43 %*
Studies within 24 months after program start (percentage points)	-0.00 (-0.15) *-2 %*	0.00 (0.08) *2 %*	-0.03 (-1.55) *-28 %*	0.03 (1.60) *38 %*	0.01 (0.44) *8 %*	-0.02 (0.86) *-20 %*
No. of observations $m - l$*	1,596–684	1,960–674	580 – 438	2,010–462	1,648–411	602 – 396

Note: **Bold type** indicates statistical significance at the 5 % level. For description of the dependent variables, see Figure 5.1. See also notes in Table 6.4. * The number of observed earnings two years after is somewhat lower than for the other outcome variables.

TABLE D1.3

Binomial logit: Results for the average treatment effect on the population g_0^{ml}.

t-values in parentheses, *relative effects in italics*.

	YP – Non (1)	LMT – Non (2)	YP – LMT (3)
Earnings one year after program start (SEK)	**- 10,260** **(-2.61)** ***-16 %***	**-23,950** **(-6.38)** ***-35 %***	**13,690** **(4.20)** ***36 %***
Earnings two years after program start (SEK)	2,100 (0.43) *3 %*	4,410 (0.96) *7 %*	-2,310 (-0.59) *-3 %*
Employment within 12 months after program start (percentage points)	-0.05 (-1.84) *-15 %*	**-0.09** **(-3.42)** ***-27 %***	0.04 (1.57) *16 %*
Employment within 24 months after program start (percentage points)	0.02 (0.46) *5 %*	0.03 (1.14) *8 %*	-0.02 (-0.73) *-5 %*
Studies within 12 months after program start (percentage points)	-0.01 (-0.65) *-9 %*	**-0.05** **(-3.27)** ***-50 %***	**0.04** **(2.43)** ***67 %***
Studies within 24 months after program start (percentage points)	0.00 (-0.09) *0 %*	-0.02 (-1.37) *-20 %*	0.02 (1.31) *18 %*

See the notes in Table D1.2.

D2 Separate analysis of women and men

TABLE D2.1

Women and men: Results for the average treatment effect on the treated q_0^{ml}.

t-values in parentheses, *relative effects in italics*.

	YP – Non	Non–YP	LMT–Non	Non–LMT	YP–LMT	LMT–YP
Earnings one year after program start:						
Women	**-10,530**	7,650	**-16,780**	**21,490**	**12,810**	-9,390
	(-2.16)	(1.54)	**(-2.50)**	**(3.79)**	**(2.12)**	(-1.50)
	-19%	*15 %*	***-32 %***	***58 %***	***40 %***	*-21 %*
Men	**-14,224**	**21,940**	**-22,570**	**34,940**	**19,150**	**-12,590**
	(-2.57)	**(4.10)**	**(-3.68)**	**(6.56)**	**(3.70)**	**(-2.00)**
	-19 %	***36 %***	***-31 %***	***73 %***	***47 %***	***-20 %***
Earnings two years after program start:						
Women	-570	-11,640	-5,220	3,610	13,600	**-20,930**
	(-0.56)	(-1.39)	(-0.62)	(0.37)	(1.19)	**(-2.49)**
	-1%	*-15 %*	*-9 %*	*6 %*	*28 %*	***-29 %***
Men	1,560	10,440	-9,450	**22,170**	6,640	**-18,650**
	(0.15)	(1.21)	(-1.09)	**(2.51)**	(0.64)	**(-2.04)**
	2 %	*11 %*	*-11 %*	***28 %***	*8 %*	***-19 %***
Employment within 12 months after program start:						
Women	-0.03	0.04	0.01	-0.01	0.05	0.03
	(-0.82)	(1.09)	(0.20)	(-0.24)	(0.93)	(0.72)
	-11 %	*17 %*	*4 %*	*-3 %*	*24 %*	*17 %*
Men	-0.06	**0.10**	**-0.10**	**0.17**	**0.11**	**-0.09**
	(-1.72)	**(2.50)**	**(-2.41)**	**(4.51)**	**(2.79)**	**(-2.05)**
	-16 %	***30 %***	***-27 %***	***60 %***	***49 %***	***-26 %***
Employment within 24 months after program start:						
Women	0.03	0.00	0.06	-0.09	-0.00	0.03
	(0.80)	(0.07)	(1.11)	(-1.89)	(-0.03)	(0.55)
	10%	*1 %*	*22 %*	*-22 %*	*-0 %*	*10%*
Men	0.06	-0.02	-0.00	0.03	0.06	-0.09
	(1.49)	(-0.45)	(-0.00)	(0.68)	(1.33)	(-1.88)
	13 %	*-4 %*	*-0 %*	*6 %*	*14 %*	*-18 %*
Studies within 12 months after program start:						
Women	0.02	0.01	**-0.09**	0.05	**0.07**	**-0.08**
	(0.67)	(0.30)	**(-2.44)**	(1.78)	**(2.26)**	**(-2.08)**
	15 %	*7 %*	***-64 %***	*53 %*	***94 %***	***-61 %***
Men	**-0.05**	0.02	-0.03	0.03	-0.00	0.00
	(-2.23)	(0.66)	(-1.16)	(1.35)	(-0.20)	(0.12)
	-43 %	*45 %*	*-32 %*	*35 %*	*-6 %*	*5 %*
Studies within 24 months after program start:						
Women	0.04	0.01	**-0.09**	0.05	0.05	-0.07
	(1.26)	(0.38)	**(-2.26)**	(1.76)	(1.44)	(-1.69)
	27 %	*9 %*	***-52 %***	*52 %*	*40 %*	*-46 %*
Men	-0.03	0.05	0.01	0	-0.02	0.03
	(-1.31)	(2.28)	(0.32)	(0)	(-0.75)	(1.07)
	-26 %	*19 %*	*11 %*	*0 %*	*19 %*	*47 %*
No. of observations *m – l**						
Women	662–325	743–333	205–160	743–170	662–158	205–152
Men	851–378	1,049–372	350–266	1,049–275	851–243	350–235

Note: **Bold type** indicates statistical significance at the 5 % level. For description of the dependent variables, see Figure 5.1. See also notes in Table 6.4. * The number of observed earnings two years after is somewhat lower than for the other outcome variables.

TABLE D2.2

Women and men: Results for the average treatment effect on the population g_0^{ml}.

t-values in parentheses, *relative effects in italics*.

	YP – Non	LMT – Non	YP – LMT
Earnings one year after program start:			
Women	-8,800	**-21,650**	**12,850**
	(-1.58)	**(-3.87)**	**(3.11)**
	-16 %	***-38 %***	***40 %***
Men	**-17,160**	**-32,420**	**15,260**
	(-3.31)	**(-6.37)**	**(3.37)**
	-22 %	***-40 %***	***34 %***
Earnings two years after program start:			
Women	6,990	8,400	-1,410
	(1.03)	(1.26)	(-0.29)
	13%	*20 %*	*-2 %*
Men	-2,990	3,030	-6,020
	(-0.47)	(0.49)	(-1.13)
	-3 %	*-4 %*	*-6 %*
Employment within 12 months after program start:			
Women	-0.03	-0.03	-0.01
	(-0.79)	(-0.60)	(-0.28)
	-11 %	*-12 %*	*-4 %*
Men	-0.07	**-0.16**	**0.09**
	(-1.86)	**(-4.56)**	**(2.83)**
	-17 %	***-37 %***	***36 %***
Employment within 24 months after program start:			
Women	0.01	0.06	-0.05
	(0.31)	(1.37)	(-1.47)
	3 %	*23 %*	*-13 %*
Men	0.04	-0.02	0.06
	(1.06)	(-0.39)	(1.79)
	9 %	*-4 %*	*11 %*
Studies within 12 months after program start:			
Women	0.00	**-0.06**	**0.06**
	(0.09)	**(-2.15)**	**(2.30)**
	0 %	***-55 %***	***67 %***
Men	**-0.04**	-0.04	0.00
	(-2.02)	(-1.79)	(-0.26)
	-36 %	*-40 %*	*0 %*
Studies within 24 months after program start:			
Women	0.01	-0.04	0.05
	(0.24)	(-1.39)	(1.81)
	6 %	*-33 %*	*38 %*
Men	-0.02	0.00	-0.02
	(-0.95)	(-0.11)	(-1.03)
	-18 %	*-0 %*	*-18 %*

Note: See the notes in Table D2.1.

D3 Time variation in treatment effects

TABLE D3.1

Time variation: Results for the average treatment effect on the treated q_0^{ml}.

Relative effects in italics.

	YP – Non (1)	Non – YP (2)	LMT–Non (3)	Non–LMT (4)	YP–LMT (5)	LMT–YP (6)
Earnings one year after program start:						
Program start 1992	-8,560	**17,340**	**-19,690**	**20,970**	**14,380**	-7,010
Program start 1993	-1,540	**16,810**	-12,090	13,480	2,380	-320
Program start 1994	-4,030	**58,640**	-19,010	17,770	30,620	6,240
Earnings two years after program start:						
Program start 1992	3,370	290	**-12,530**	7,960	**14,930**	-9,370
Program start 1993	2,460	8,740	5,330	8,850	-7,050	6,070
Program start 1994	–	–	–	–	–	–
Empl. within 12 months after program start:						
Program start 1992	0.00	**0.13**	**-0.09**	**0.12**	**0.11**	-0.03
Program start 1993	0.03	0.04	-0.03	0.02	-0.01	0.05
Program start 1994	0.14	0.00	0	-0.08	-0.09	0.09
Empl. within 24 months after program start:						
Program start 1992	**0.11**	0.03	0.01	-0.01	0.07	-0.03
Program start 1993	**0.10**	-0.05	0.05	-0.09	-0.03	-0.02
Program start 1994	0.25	0.00	0	-0.08	0.02	0
Studies within 12 months after program start:						
Program start 1992	0.00	-0.05	**-0.07**	**0.05**	0.04	**-0.08**
Program start 1993	-0.05	**0.06**	**-0.09**	**0.07**	0.04	-0.06
Program start 1994	**0.09**	0	0.09	-0.23	0.07	0.09
Studies within 24 months after program start:						
Program start 1992	0.03	-0.05	-0.04	0.02	0.01	**-0.08**
Program start 1993	-0.04	0.05	-0.08	**0.08**	0.06	-0.07
Program start 1994	**0.11**	0	0.09	-0.23	0.09	0.09
No. of observations 1992*	542 – 277	764 – 281	390 – 271	764 – 278	542 – 222	390 – 217
No. of observations 1993*	897 – 425	967 – 396	132 – 103	967 – 103	897 – 107	132 – 105
No. of observations 1994*	44 – 12	26 – 11	11 – 8	26 – 9	44 – 9	11 – 9

Note: **Bold type** indicates statistical significance at the 5 % level. Standard errors are not reported but are accessible from the author by request. For description of the dependent variables, see Figure 5.1. See also notes in Table 6.4. * The number of observed earnings two years after is somewhat lower than for the other outcome variables.

TABLE D3.2

Time variation: Results for the average treatment effect on the treated q_0^{ml} .

Relative effects in italics.

	YP – Non (1)	LMT – Non (2)	YP – LMT (3)
Earnings one year after program start:			
Program start 1992	**-13,470**	**-21,310**	7,840
	-23 %	**-37 %**	21 %
Program start 1993	-9,620	-9,090	-530
	-14 %	-13 %	-1 %
Program start 1994	-24,440	-27,110	2,670
	-21 %	-27 %	3 %
Earnings two years after program start:			
Program start 1992	220	-10,160	10,380
	0 %	-14 %	18 %
Program start 1993	4,960	4,390	570
	6 %	5 %	1 %
Program start 1994	–	–	–
Employment within 12 months after program start:			
Program start 1992	-0.07	**-0.11**	0.04
	-21 %	**-32 %**	18 %
Program start 1993	-0.01	0.01	-0.01
	-3 %	3 %	-3 %
Program start 1994	0.06	0.15	-0.09
	14 %	50 %	-16 %
Employment within 24 months after program start:			
Program start 1992	0.03	0.02	0.01
	8 %	6 %	3 %
Program start 1993	0.08	0.10	-0.03
	22 %	29 %	-6 %
Program start 1994	0.14	0.15	-0.01
	31 %	50 %	-10 %
Studies within 12 months after program start:			
Program start 1992	0.02	**-0.05**	0.07
	22 %	**-45 %**	175 %
Program start 1993	-0.05	**-0.08**	0.03
	-31 %	**-62 %**	38 %
Program start 1994	0.05	0.10	-0.05
	125 %	100 %	-36 %
Studies within 24 months after program start:			
Program start 1992	0.04	**-0.01**	**0.05**
	44 %	**-9 %**	**63 %**
Program start 1993	-0.04	**-0.08**	**0.05**
	-25 %	**-62 %**	**71 %**
Program start 1994	0.06	0.10	-0.04
	120 %	100 %	-27 %

Note: See the notes in Table D3.1.

D4 Various types of labour market training

TABLE D4.1

Various types of training: Results for the mean treatment effect on the treated q_0^{ml}.

t-values in parentheses, *relative effects in italics*.

	YP – Non	Non – YP	LMTv – Non	Non – LMTv	LMTn – Non	Non – LMTn
Earnings one	**- 14,900**	**16,780**	**- 26,930**	**19,730**	**- 19,900**	**30,350**
year after pro-	**(-3.86)**	**(4.46)**	**(-4.59)**	**(3.82)**	**(-2.85)**	**(4.82)**
gram	***- 22 %***	***30 %***	***- 34 %***	***37 %***	***- 41 %***	***70 %***
Earnings two	- 5,570	6,420	- 13,340	6,190	- 4,530	16,150
years after	(-0.83)	(1.03)	(-1.67)	(0.70)	(-0.47)	(1.38)
program	*- 7 %*	*8 %*	*- 15 %*	*8 %*	*- 8 %*	*23 %*
Empl. within	- 0.05	**0.07**	**- 0.09**	**0.11**	- 0.05	**0.15**
12 months after	(- 1.83)	**(2.45)**	**(- 2.10)**	**(2.64)**	(- 0.97)	**(2.93)**
program start	*- 14 %*	***23 %***	***- 23 %***	***39 %***	*- 21 %*	***68 %***
Empl. within	0.04	0.00	0.00	- 0.03	0.02	- 0.04
24 months after	(1.34)	(0.00)	(0.07)	(- 0.64)	(0.37)	(0.60)
program start	*10 %*	*0 %*	*1 %*	*- 6 %*	*7 %*	*- 8 %*
Studies within	- 0.01	0.03	**- 0.05**	**0.06**	0.01	**0.08**
12 months after	(- 0.41)	(1.58)	**(- 2.00)**	**(3.06)**	(0.21)	**(2.41)**
program start	*- 7 %*	*33 %*	***- 50 %***	***92 %***	*10 %*	***186 %***
Studies within	0.01	0.02	- 0.02	**0.05**	0.01	**0.08**
24 months after	(0.64)	(1.10)	(- 0.75)	**(1.96)**	(0.39)	**(2.31)**
program start	*11 %*	*22 %*	*- 20 %*	***57 %***	*17 %*	***173 %***
Obs. *m–l**	1,536 – 707	1,790 – 706	321 – 265	1,790 – 281	137 – 125	1,790 – 129

	YP – LMTv	LMTv – YP	YP – LMTn	LMTn – YP	LMTv-LMTn	LMTn-LMTv
Earnings one	**13,310**	-8,560	**21,270**	**- 21,280**	14,420	**- 19,200**
year after pro-	**(2.25)**	(- 1.54)	**(3.39)**	**(- 3.05)**	(1.88)	**(- 2.37)**
gram	***34 %***	*- 14 %*	***67 %***	***- 43 %***	*39 %*	***- 41 %***
Earnings two	2,780	- 10,410	13,130	- 5,630	13,670	**- 28,450**
years after	(0.24)	(- 1.39)	(0.98)	(- 0.60)	(1.14)	**(- 2.36)**
program	*4 %*	*- 12 %*	*21 %*	*- 10 %*	*21 %*	***- 35 %***
Empl. within	0.05	0.00	0.08	- 0.08	0.00	0.01
12 months after	(1.10)	(0.00)	(1.40)	(- 1.43)	(0.05)	(0.24)
program start	*21 %*	*0 %*	*34 %*	*- 29 %*	*1 %*	*8 %*
Empl. within	0.04	- 0.02	0.05	- 0.10	0.03	- 0.04
24 months after	(0.68)	(- 0.49)	(0.86)	(- 1.60)	(0.43)	(- 0.51)
program start	*9 %*	*- 5 %*	*15 %*	*- 23 %*	*8 %*	*- 10 %*
Studies within	**0.08**	- 0.03	0.05	0.04	0.00	**0.07**
12 months after	**(3.10)**	(- 1.34)	(1.50)	(1.15)	(0.08)	**(2.28)**
program start	***252 %***	*- 40 %*	*95 %*	*83 %*	*7 %*	***450 %***
Studies within	0.03	- 0.02	0.07	0.04	0.01	0.06
24 months after	(1.01)	(- 0.60)	(1.80)	(0.99)	(0.29)	(1.58)
program start	*32 %*	*- 17 %*	*123 %*	*56 %*	*20 %*	*133 %*
Obs. *m – l**	1,536 – 265	321 – 249	1,536 – 128	137 – 120	321 – 95	137 – 96

Note: **Bold type** indicates statistical significance at the 5 % level. For a description of the dependent variables, see Figure 5.1. * The number of observed earnings two years after is somewhat lower than for the other outcome variables.

TABLE D4.2

Various types of training. Results for the aver. treatment effect on the population g_0^{ml}.
t-values in parentheses, *relative effects in italics.*

	YP – Non (1)	LMTv – Non (2)	LMTn – Non (3)
Earnings one year after program start	**- 15,510**	**- 23,090**	**- 33,270**
	(-3.15)	**(-4.69)**	**(-4.94)**
	- 23 %	**- 31 %**	**- 54 %**
Earnings two years after program start	- 2,910	9,650	3,740
	(-0.47)	(1.58)	(0.42)
	- 4 %	14 %	8 %
Employment within 12 months after program start	- 0.06	**- 0.10**	**- 0.13**
	(- 1.58)	**(- 2.70)**	**(-2.43)**
	- 17 %	**- 26 %**	**- 40 %**
Employment within 24 months after program start	0.02	0.02	0.01
	(0.53)	(0.40)	(0.14)
	5 %	4 %	3 %
Studies within 12 months after program start	- 0.02	**- 0.07**	**- 0.07**
	(- 1.01)	**(- 3.58)**	**(-1.97)**
	- 16 %	**- 59 %**	**- 45 %**
Studies within 24 months after program start	- 0.01	- 0.03	- 0.06
	(- 0.28)	(- 1.43)	(-1.66)
	- 5 %	- 30 %	- 38 %

	YP – LMTv (4)	YP – LMTn (5)	LMTv – LMTn (6)
Earnings one year after program start	**7,590**	**17,760**	10,170
	(2.42)	**(3.20)**	(1.83)
	17 %	**50 %**	25 %
Earnings two years after program start	**- 12,560**	- 6,640	5,920
	(-3.38)	(-0.88)	(0,79)
	- 14 %	- 8 %	8 %
Employment within 12 months after program start	0.04	0.07	0.03
	(1.78)	(1.58)	(0.70)
	16 %	32 %	12 %
Employment within 24 months after program start	0.01	0.01	0.01
	(0.24)	(0.24)	(0.13)
	1 %	3 %	2 %
Studies within 12 months after program start	**0.05**	0.05	0.00
	(3.09)	(1.45)	(- 0.07)
	82 %	76 %	- 4 %
Studies within 24 months after program start	0.03	0.06	0.03
	(1.56)	(1.63)	(0.92)
	25 %	80 %	72 %

See the notes under Table D4.1.

D5 Exclusion of the participants' program period

TABLE D5.1

Exclusion of the program period: Results for the treatment effect on the treated q_0^{ml}.

t-values in parentheses, *relative effects in italics*.

	YP–Non (1)	Non–YP (2)	LMT–Non (3)	Non–LMT (4)	YP–LMT (5)	LMT–YP (6)
Earnings one year after program start	240	4,210	**-14,720**	**19,840**	**21,230**	**-10,910**
	(0.06)	(0.91)	**(-3.21)**	**(4.80)**	**(4.84)**	**(-2.14)**
	0 %	7 %	**-24 %**	**41 %**	**53 %**	**-15 %**
Earnings two years after program start	3,590	-5,190	**-8,390**	6,320	11,120	-5,070
	(0.57)	(-0.97)	**(-1.37)**	(1.20)	(1.36)	(-0.78)
	5 %	-6 %	**-11 %**	8 %	16 %	-6 %
Employment within 12 months after program start	-0.00	0.02	-0.05	0.06	**0.09**	-0.03
	(-0.07)	(0.69)	(-1.56)	(1.88)	**(2.58)**	(-0.74)
	-1 %	6 %	-14 %	15 %	**32 %**	-7 %
Employment within 24 months after program start	0.05	0.01	0.03	-0.05	0.02	0.03
	(1.75)	(0.28)	(0.87)	(-1.72)	(0.56)	(0.80)
	12 %	2 %	8 %	-12 %	5 %	7 %
Studies within 12 months after program start	0.00	-0.00	-0.02	**0.05**	**0.04**	-0.03
	(0.03)	(-0.20)	(-0.91)	**(3.06)**	**(2.23)**	(-1.54)
	0 %	-3 %	-20 %	**80 %**	**64 %**	-24 %
Studies within 24 months after program start	0.01	-0.01	0.01	0.04	0.04	-0.01
	(0.78)	(-0.23)	(0.52)	(1.79)	(1.73)	(-0.58)
	13 %	-4 %	11 %	43 %	42 %	-10 %
No. of obs. $m - l$*	1,592–711	1,912–722	580–439	1,912–459	1,592–452	580–388

Note: **Bold type** indicates statistical significance at the 5 % level. For description of the dependent variables, see Figure 8.1. See also notes in Table 6.4. * The number of observed earnings two years after is somewhat lower than for the other outcome variables.

TABLE D5.2

Exclusion of the program period: Results for the average population effect g_0^{ml}.

t-values in parentheses, *relative effects in italics*.

	YP – Non (1)	LMT – Non (2)	YP – LMT (3)
Earnings one year after program start	-2,720	**-19,920**	**17,200**
	(-0.64)	**(-5.04)**	**(4.86)**
	-4 %	**-30 %**	**39 %**
Earnings two years after program start	**-14,380**	**-10,894**	-3,490
	(-2.78)	**(-2.25)**	(-0.87)
	-15 %	**-13 %**	-4 %
Employment within 12 months after program start	-0.01	**-0.07**	**0.05**
	(-0.47)	**(-2.43)**	**(2.20)**
	-3 %	**-19 %**	**16 %**
Employment within 24 months after program start	0.01	0.04	-0.03
	(0.45)	(1.38)	(-1.06)
	2 %	10 %	-6 %
Studies within 12 months after program start	0.00	**-0.05**	**0.05**
	(0.26)	**(-2.67)**	**(3.02)**
	0 %	**-36 %**	**83 %**
Studies within 24 months after program start	0.01	-0.02	**0.04**
	(0.57)	(-1.35)	**(2.16)**
	8 %	-17 %	**44 %**

Note: See the notes in Table D5.2