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Business cycles and the impact of labour market programmes

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Business cycles and the impact of labour market programmes

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Abstract
By comparing mean outcomes for a large number of matched samples of participants and non-participants we estimate individual earnings effect of the Norwegian labour market training programme (LMT) targeted at unemployed adults in the years 1991-1996. The average training effect on the trained is positive, even after three years. The training effect is positively correlated with post-training job opportunities in the (local) labour market, when job opportunities are measured by time-varying human capital adjusted national and region-specific exit rates from unemployment.

Keywords: Causal effects, matching estimators, training unemployed, impact on annual earnings.

JEL classification: C14, J64, and J68.

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1. Introduction

To combat high and persistent unemployment and promote labour force participation, *active labour market programmes* (ALMPs) may provide a better alternative than income support for the unemployed workers. Through skill upgrading of the unemployed, ALMPs may improve the match between vacancies and unemployed and thus reduce wage-inflation, increase employment and decrease unemployment. During the 1990's annual public expenditures for ALMPs targeted at unemployed exceeded 1 per cent of GDP in many European countries, and the average annual participant inflow in these programmes was more than 5 per cent of the labour force. The Nordic countries are top ranked on this list, although the decline in unemployment has reduced Norwegian expenditures significantly in recent years.[1] The focus on ALMP in the Norway is also illustrated by a high share of active expenditures relative to total unemploymenent expenditures, see Calmfors, Forslund and Hemström (2001).

While the relative volume of such programmes is significantly lower, the public focus on *programme efficiency* seems to be stronger in the United States than in Europe. The international literature on evaluation of labour market programmes is extensive and growing, both in the United States and Europe, including the Nordic countries. A large part of this literature focuses on post-programme outcomes at the individual level, measured by employment probabilities, employment duration, or annual earnings. The major challenge for such evaluations is to report unbiased estimates of the causal impact of the treatment. In line with most microeconomic evaluation studies, this paper deals with individual effects only and more precisely, the average effect for the participants. A positive impact on the labour market success for participating individuals, is a necessary, but probably not sufficient to achieve the overall macroeconomic goals: reduced wage inflation, increased employment and reduced unemployment. The net impact at the macro level also depends on any dead-weight loss, substitution and displacements effects, see e.g. Calmfors (1994) and Heckman, LaLonde and Smith (1999).

Very few, if any, of the programme evaluations question whether the individual programme effect depends on the state of the (local) labour market during the post-programme period. When no employers open new jobs, or if they do not open vacancies in response to voluntary quits, any improvement of skills through labour market programs will not help unemployed back to work. This extreme case is unrealistic, even during a slump, but it

[1] This observation is based on figures published by OECD in Employment Outlook 1990-2001 and is also shown in Calmfors, Forslund and Hemström (2001).

illustrates the possibility that job opportunities available in the post-programme labour market may affect the individual effects. On the other hand, when competition for vacant jobs is intensive, unemployed persons who upgrade their skills through ALMP may improve their job prospects significantly, compared to a situation where firms face labour shortages and hires "whoever" comes along. Consequently, programme effects may vary systematically over the *business cycles* at the national or local level, but the direction needs to be studied empirically. If business cycles matter, it may explain why short and long run effects differ simply because the macroeconomic, or even the local labour market conditions, change over time. Insight into the influence of business cycles on individual programme effects is highly relevant to policy-makers who aims at choosing an optimal timing and volume of ALMP. This knowledge can also turn out to be useful when assessing and comparing the estimated impact of various programmes across time and regions or even between countries.

In this paper we use non-experimental Norwegian data covering the years 1991-1997 to study the importance of business cycles on short term and medium term individual effects of a labour market training programme, the *LMT programme*. This is the largest labour market programme in Norway targeted at unemployed adults, offering classroom training in a large number of subjects, mainly vocational, but also some general subjects. The courses last typically 5 - 20 weeks. Similar programmes are found in many other countries.

In order to identify any impact of post-programme labour market conditions on individual programme effects, data containing labour market variation are needed. The typical evaluation considers one cohort, or a limited number of cohorts, of participants. Such studies have to rely on spatial (or regional) variation in labour market conditions since a given post-programme period is only observed under one set of macroeconomic conditions.[2] Thus, if the programme operates at a limited number of geographical locations, the impact of labour market conditions on the programme effects will not be identified. Unlike most evaluation studies, we are able to disentangle the impact of post-training business cycles from the importance of the time span between the training and the post-training period. Using several cohorts of participants, we can estimate first, second and third year effects under different labour market conditions, even controlling for fixed regional effects.

It is well known that non-experimental evaluation methods may provide *biased estimates* of the impact of programmes. The conventional evaluation bias comprises the bias due to selection on unobservables as well as bias due to non-overlapping supports of the

[2] "Time since programme" will be (perfectly) correlated with "calender time effects".

explanatory variables in the treatment sample and the comparison sample (mismatching) and different distributions of these variables within the two samples (misweighting). Evaluation methods based on *matching techniques* may reduce the conventional measure of bias - as far as selection on observables is concerned. Such estimators are increasingly being used in evaluation studies; in this paper we apply such estimators as well.

A commonly used conditioning set is the probability of being in the participant group versus in the comparison group. Provided that the outcome is independent of participation conditional on this probability the matching estimator is unbiased. This is the *conditional independence assumption*, CIA. Participation in a specific programme is, however, not the outcome of a simple binary choice, or of a selection process with only two mutually exclusive outcomes. First, the target group is often offered alternative programmes. Second, those who participate in programmes do also have more than one option – not only unemployment, but possibly also options as employment, education, retirement etc. Thus, matching the samples to be compared on *all* these propensities would make the CIA more plausible. In this paper we use probability scores matching estimators to assess the impact of a single programme by comparing participants (the treatment group) with unemployed non-participants (the comparison or no-treatment group). The two samples are matched by probabilities of (a) taking part in the programme to be evaluated, (b) taking part in other alternative programmes, and (c) leaving the unemployment register, all as alternatives to staying unemployed. To make each group of participants homogenous we conduct separate analyses for those starting LMT at about the same time, i.e. in winter or in autumn each year. This gives us 12 cohorts of participants (2 x 6 years). The data are cut by gender and unemployment benefit entitlement giving a total of 48 subsamples for which we estimate separate training effects.

Our analyses show that the impact of LMT on annual earnings is *positive.* With few exceptions, the effects are statistically and economically significant. The positive training effects persist. Even after three years, earnings of participants with recent work experience, i.e. those who receive unemployment benefits before the start of the training spell, are significantly higher than among the non-participants. Among participants without recent work experience, i.e those without UB entitlement, not all effects are statistically significant.

A meta-analysis of the large number of group- and cohort-specific training effects confirms that the average training effect on the trained do vary over the business cycle. Participants gain more when job opportunities in the post-training period are favourable. The effects are significantly lower when the national, or the local, labour market is characterised by a high unemployment rate and few transitions from unemployment to jobs.

The paper is organised as follows. Section 2 reviews some previous studies relevant for our study. Section 3 discusses briefly the evaluation problem, and section 4 presents the matching procedure. Section 5 presents the programme to be evaluated and the Norwegian labour market during the period covered by the study.

Section 6 presents the design of the study and the data. Data contain participants in LMT and non-participants during the period 1991-1996. The matching procedure and the outcomes of the matching for each of the 48 subsamples are presented in section 7. Section 8 presents the results of the effect evaluation, both first year effects for all the 48 subsamples, and later years effects as far as data on the outcome are available. In this section we also present a test for unobserved heterogeneity based on pre-training annual earnings.

Section 9 contains meta analyses of the estimated training effects for each the subsamples, focusing on how the impact of LMT on annual earnings correlates with job opportunities (the business cycle) at the national as well as the regional level. Section 10 concludes.

2. Previous studies

The international literature on evaluation of labour market programmes is extensive and growing, both in the United States and in Europe. A large part of this literature focuses on post-programme outcomes at the individual level. Barnow (1987), LaLonde (1995), Fay (1996) and Heckman, LaLonde and Smith (1999) review much of the empirical results and the methodological discussions. No consensus about the impact of the active labour market programmes on individual success has emerged from the large number of evaluations in recent years. The content and the organisation of the programmes, the target groups and recruitment procedures as well as the economic environment at the time of the evaluation differ across the studies. There is also a large variety in evaluation design and estimating techniques. Thus there is no surprise that the results diverge. The mixed results may also reflect a lack of suitable data as well as robust estimation methods.

The general impression is that some, but not all programmes do have the intended impact, at least for some of the participants and in the short run. Large scale, low cost programmes perform not as good as more costly programmes, targeted at smaller groups of unemployed (OECD 1993, Martin 1998). Activation strategies especially targeted at people receiving unemployment benefits, encouraging them to intensify job search with later obligation to participate in programmes, have also shown evidence of increased job motivation and increased transitions to employment (Martin and Grubb 2001).

Nordic studies

Recently quite a number of studies in the Nordic countries have been published. Sweden has a long tradition with ALMPs, and the labour market training programme has been evaluated several times. This programme, AMU in Swedish, is quite similar to the Norwegian programme evaluated in the present study, the LMT programme, or AMO in Norwegian.

The early Swedish evaluations report positive impacts on employment probabilities and earnings. Axelsson (1992) evaluates the labour market training programme by comparing annual earnings (before taxes) for a sample of participants and a sample of unemployed non-participants in 1981. The analyses are based on non-experimental data within the framework of a fixed effect log-linear earnings model as well as by difference in differences. The overall impact is estimated to be positive and significant, and the second year effect turns out to be larger than the first year effect: about 9,000 and 7,000 Swedish kroner respectively.

Evaluations using data from the late 1980'ies onwards show, however, insignificant and even negative impacts of the labour market training programme. Many of the Swedish studies focus on impact for young participants, either in special programmes for youth or in ordinary training and employment programmes. The results for this group are also mixed, and in general not very positive: Ackum (1991) finds mostly insignificant effects for young participants, Korpi (1994) presents both significantly positive and insignificant effects, while Regner (1997) reports negative impacts of training programmes. Larsson (2000) evaluates two programmes for youth (20-24 years) and concludes that (in the short run, after one year) both seem to have negative impacts on employment and annual earnings (after two years the impacts are insignificant) – whereas the impacts on transition to ordinary education are mostly insignificant.

In a recent study, Sianesi (2002) applies a multiple-treatment matching framework to evaluate the differential performance of six main types of Swedish labour market programmes. This study covers 30,600 adults 25-54 who became unemployed for the first time during 1994 and who were eligible for unemployment benefits. The sample is followed until the end of November 1999, i.e. a post-training period of maximum 5 years. The differential performance of the six programmes – and the non-treatment state (waiting longer in open unemployment and searching for a job) - is assessed in relation to employment rates over time and the probability to be in a compensated unemployment spell. On average people who is in a programme (any programme) at a given moment subsequently enjoy higher employment rates than if they had postponed participation. Secondly, the best programme is clearly *employment subsidies*, not surprisingly as this is an arrangement based on a job promise by the programme employer – after completion of the programme. The employment probability is 40 percentage points higher about 7 months after entering the programme - compared with waiting. The impact decreases over time and is about 20 percentage points 60 months after entering the programme.

One of the six programmes evaluated is *labour market training*. Compared with waiting, participation in LMT is found to have a positive, significant effect on employment, increasing from about 5 percentage points 12 months after entering the programme to almost 20 percentage points 60 months after entering. Compared with the other five programmes LMT is the least effective when it comes to employment rates over time. Further details on the Swedish experience can be found in surveys by Björklund (1990), Zetterberg (1996), Ackum Agell & Lundin (2001) and Calmfors, Forslund and Hemström (2001).

Also in Denmark and Finland there are programmes similar to the Norwegian LMT programme. Jensen et al. (1993) evaluate the Danish LMT, which offers somewhat shorter courses (2-5 weeks), mainly targeted at employed, but open for unemployed as well. Effects on subsequent wage level and unemployment are analysed by fixed effects models. Wage effects are found to be small and insignificant in most cases. When it comes to effects on unemployment, the estimated models predict that participants with substantial pre-training unemployment will experience a decrease in post-training unemployment.

Westergaard-Nielsen (1993) evaluates the same programme for a different period and within a different framework. This study shows that training gives an overall positive impact on the wage level, significant for men – also for those with some unemployment experience - and insignificant for women. When it comes to subsequent unemployment, participation in LMT gives a small overall reduction for men, not for women. As Jensen et al. (1993), Westergaard-Nielsen (1993) finds that this is the case also for those with pre-training unemployment experience. However, for those with substantial unemployment experience, Westergaard-Nielsen (1993) finds that post-training unemployment increases. The Danish Ministry of Labour, AM (2000) and Westergaard-Nielsen (2001) recently evaluate effects of the Danish employability enhancement programmes. While AM (2000) is rather optimistic with respect to the individual effects, Westergaard-Nielsen (2001) is more sceptical when it comes to the efficiency of the active labour market policy in Denmark

Evaluation studies of ALMPs in Norway typically report more positive results than the evaluations in the other Nordic countries. For labour market training, evaluations indicate positive impacts on employment probabilities, see Torp (1994) and Aakvik (1998), while Raaum and Torp (2002) find positive annual earnings effects.

A business cycle perspective on programme effects

There are numerous reasons for why training can affect future earnings of the LMT participants. First, successful training helps trainees to accumulate human capital that is relevant to potential employers. Increased human capital may have a positive effect on wages as well as the probability of employment. However, if training increases the reservation wage, this may have the opposite effect on the employment probability. Secondly, as training represents a meaningful activity to most participants, it may help to prevent social isolation and mental problems during a period of non-employment. This may in turn enhance job search efficiency and reduce the probability that unemployed workers drop out of the labour force. Thirdly, LMT may represent a signal about unobserved characteristics like motivation and effort, which correlates with productivity. Potential employers may consider a personal unemployment record, which include LMT to be better than a record with only open unemployment. This "signalling effect" of LMT is crucially dependent on the reputation of the programme. Programmes associated with long-term or low-qualified unemployed may give a negative signal to employers. Finally, training has an alternative cost as time available to ordinary job search activities is reduced. Various empirical studies show that labour programme participants have very low transition rates to ordinary employment during the programme period; see e.g. Røed and Zhang (1999).

Turnng to the impact of labour market conditions, the location in the business cycle may influence active labour market programmes in different ways. First, the composition of the eligible population, typically unemployed adults, may change with respect to observed and unobserved characteristics as both demand and supply of labour change. Secondly, the recruitment process may change. This applies to both self-selection (who wants to participate?) and the administrative selection, reflecting changing priorities in the implementation of labour market policy. Finally, the state of the local or national labour market and the demand for labour in the post-training period may affect the impact of training on individual outcomes, e.g. earnings.

The purpose of the present study is to make identical evaluations of a programme at various points in time over a business cycle. The LMT programme is well suited for studying how the state of the labour market, i.e. business cycles, affects the impact of ALMP. First, it has a fairly long record and it has been operated at a significant volume every year in the period of interest - even when unemployment was as low as 3 per cent of the labour force. Second, the eligibility criteria are quite simple and have mainly been the same in the whole period. Participants have to be unemployed and to register at the local PES (public employment service), they have to be 19 years or older (our study includes only persons 25-50 years) and employable, i.e. not vocationally disabled and ready to take a job. Thus, even if the mix of courses has changed over the business cycle, e.g. more general training during the slump and training more targeted at specific need in the market during the boom, the evaluated programme is essentially the same throughout the first half of the 1990s.

Assume an unbiased estimator for the average treatment effect of the treated is identified. Any variation in the estimated effect over the business cycle is then a mix of changes in the composition of the treatment group (assuming effect heterogeneity) and variation in demand for labour across post-programme periods.

In the present study, to overcome some of this possible composition effects, we specify four different treatment groups: men and women, entitled and not entitled for unemployment benefits. It turns out that the observable composition of each treatment group is rather stable over the period. Thus, interpreting variations in the estimated effect over the evaluation period we focus on the last point, i.e. changes in the demand for labour.

Our hypothesis is that the effects of ALMPs are more positive (less negative) during a boom than during a slump. When employment is increasing employers have to recruit from outside the market: young entrants, re-entrants and unemployed. Among unemployed we believe employers will prefer job applicants with some programme experience compared with other unemployed with the same characteristics. Decreasing employment and increasing unemployment means a low turnover rate and very few job openings. Thus even for the best qualified among the unemployed the employment probability is low during a slump.

This kind of business cycle impact is probably stronger for effects of programmes emphasising *quick-job-entry*, as intensified employment service and job search training. It is probably less strong for *human capital development* programmes focusing on basic skills and vocational training. As we only evaluate one programme we are not able to test this hypothesis.

3. The evaluation problem

There are various concepts of causal effects – even for a specific and well-defined treatment and for a given outcome. First, the treatment in question needs to be contrasted with *an alternative* treatment or to non-treatment. Second, we have to specify for *whom* we evaluate the impact, whether it is the average effect for a specific group or the whole distribution of effects.

Denote Y_1 as the given outcome at the relevant point in time conditional on the specific treatment of interest, and denote Y_0 as the outcome conditional on non-treatment, or the alternative treatment. Defining the impact as the difference between these two, we get (Y_1 - Y_0.) Thus the causal impact of the treatment does not only rely on the specification of the treatment to be evaluated. The definition of the non-treatment status is just as important.

For each person only one outcome is observed. Thus whether we want to estimate the expected impact for *any* potential participant, for those *not* participating, or for those who *do* participate, we need to estimate or simulate the counterfactual outcome.

Assume we have cross-sectional data. Let D = 1 for those in the treatment group and let D=0 for those in the non-treatment group. Let X be a vector of observed characteristics. Assume the outcome Y depends on X and D, as well as an unobserved error term U:

(1a) D= 1: $Y_1 = a_1X + U_1$

(1b) D= 0: $Y_0 = a_0X + U_0$

The most common evaluation parameter of interest is the mean impact for participants or *average (expected) treatment effect for the treated* (ATET).[3] The ATET is the expected difference between $Y_1$and Y_0, conditional on D=1, given by

$$(2)\ \Delta(X) = E(Y_1 - Y_0 \mid X, D=1) = E(Y_1 \mid X, D=1) - E(Y_0 \mid X, D=1)$$

To identify this parameter we have to predict Y_0, because this is not observable for D=1. Given model (1) the effect $\Delta(X)$ defined by (2) is a mix of structural effects $\{ a_1 X - a_0 X \}$ and error terms $E(U_1 - U_0 \mid X, D=1)$.

There are many methods of constructing the unobserved counterfactual $E(Y_0 \mid X, D=1)$. One common method is to use the outcomes of non-participants (or participants in the alternative treatment) as a proxy, i.e. $E(Y_0 \mid X, D=0)$. However, comparing participants and non-participants for instance in a standard regression analyses, i.e. comparing the expectations $E(Y_1|X, D=1)$ and $E(Y_0|X, D=0)$, we may get a biased estimate of $\Delta(X)$. This *selection bias* is given by

[3] Other parameters of interest are for instance the average (expected) treatment effect for a person drawn randomly from the eligible population or the expected effect for a person drawn randomly from the combined sample of participants and non-participants. In addition it is of interest to assess the whole distribution of effects: What fraction of the participants benefits from the treatment, and what is the effect for those in the left-hand-side tail of the outcome distribution?

(3) $B(X) = E(Y_0 \mid X, D=1) – E(Y_0 \mid X, D=0)$

B(X) is rigorously defined only for values of X common to D=1 and D=0. Conditional on this X the bias rigorously defined is due to genuine differences in the distributions of the error terms (unobserved differences).

The *conventional evaluation bias* (LaLonde 1986) defined by $B = E(Y_0|D=1) - E(Y_0|D=0)$ is analogous to selection bias B(X) given by (3) but does not condition on X. Heckman, Ichimura, Smith and Todd (1998) show that the conventional evaluation bias comprises the selection bias rigorously defined as well as bias due to non-overlapping supports of X in the two samples (mismatching) and different distributions of X within the two samples (misweighting). Heckman, Ichimura and Todd (1997) demonstrate that, in the Job Training Partnership Act (JTPA) study, bias due to selection on unobservables is empirically less important than selection due to lack of matching on X for the samples of participants and non-participants.

In the jungle of complicated econometric evaluation models, it is important to keep in mind one of the fundamentals in empirical research; "Good data help a lot".[4] From assesments of evaluation strategies on US data, there seems to be a consensus that some features are of particular importance. Heckman, Ichimura and Todd (1997) summarise these as follows:

(I) Participants and controls have the same distributions of unobserved attributes. (II) Participants and controls have the same distributions of observed attributes. (III) The same questionnaire is administrated to both groups, so outcomes and characteristics are measured in the same way. (IV) Participants and controls are placed in a common economic environment.

In the present study of LMT, we estimate the ATET where the treatment and the non-treatment groups are sampled from the same populations. All persons are fulltime unemployed, registered at the local branch of PES, at the same time, i.e. taking care of (IV). Information on all groups is collected in the same way and from the same sources without sample attrition (administrative registers), i.e. fulfilling (III). The matching procedure described in the next section takes care of feature (II).

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[4] This has indeed been stressed by e.g. Heckman and his colleagues in numerous contributions over the last ten years.

4. Matching

The logic of matching is to re-establish some of the features characterising experimental data when we actually use non-experimental data. By matching we construct samples of participants and non-participants to ensure that they meet certain conditions related to independence between the outcome (or the effect to be evaluated) and treatment status. The brief presentation to follow leans heavily on Heckman, Ichimura and Todd (1997, 1998).

Assume that the outcomes (Y_0, Y_1) and the treatment status D are statistical independent conditional on X. (This X-vector may be the same or another than the X-vector in the outcome model.) Thus

(4) $(Y_0, Y_1) \perp\!\!\!\perp D \mid X$

This is equivalent to $\text{Prob}(D=1 \mid Y_0, Y_1, X) = \text{Prob}(D=1 \mid X)$, which rules out the Roy model of self-selection. In addition, assume that

(5) $0 < P(X) = \text{Prob}(D=1 \mid X) < 1$

By (5) we exclude cases of $P(X)=1$ and $P(X)=0$, i.e. persons with X-values that ensure they will always or never receive treatment. Such persons are not possible to match with persons from the other group. According to Rosenbaum and Rubin (1983) condition (4) is the *ignorability condition* for D, while together with (5) it constitutes the *strong* ignorability condition.

Conditions (4) and (5) are, however, stronger than what is necessary to estimate ATET. To identify $E(Y_0 \mid X, D=1)$ it is sufficient to assume

(4’) $Y_0 \perp\!\!\!\perp D \mid X$

(5’) $P(X) < 1$

(4’) is called the conditional independence assumption (CIA). This does not rule out the dependence of D and Y_1. To get an unbiased estimate of ATET it is sufficient with the even weaker assumption: $E(Y_0 \mid X, D=1) = E(Y_0 \mid X, D=0)$

Assume the X-variables that meet the conditions (4’) and (5’) are identified. Thus by *matching* the two subsamples on these variables we eliminate the bias in the $\Delta(X)$ estimator given by (2), but only the bias due to observables.[5] Provided that the CIA holds, we have B(X) = 0 for the matched samples. If CIA does not hold, other estimation methods may eliminate selection on unobservables. Difference-in-differences will for instance eliminate selection on person specific, time-invariant unobservables.[6] When the number of matching variables (observed variables that may affect the relation between participation status and outcome) is very large, multivariate matching on explanatory variables is hard to handle.

Rosenbaum and Rubin (1983) show that if CIA holds, matching the two samples on the *propensity score* P(X) is sufficient to secure unbiased estimates. They show that (for random variables D, Y and X) when Y_0 is independent of D conditional on X, Y_0 is also independent D, conditional on P(X)= Prob(D=1|X).

If the propensity score is smaller than one, then $E(Y_0| D=1, P(X)) = E(Y_0| D=0, P(X))$. Thus, if P(X) is known or if it can be parametrically (or semi-parametrically) estimated, we may match the two samples on the univariate propensity score.

The propensity score matching methods are further developed by Heckman, Ichimura and Todd (1997, 1998), see also Heckman, Ichimura, Smith and Todd (1998), Imbens (2000) and Lechner (2001a). Empirical implementations of the various estimators are found in some of the same papers as well as in Deheija and Wahba (1998, 1999), Brodaty, Crepon and Fougere (2001), Smith and Todd (2002), Larsson (2000) and Lechner (2001b).

Although increasingly popular, the propensity score matching technique is not necessarily an easy way to obtain non-biased estimates using non-experimental data. For instance, Smith and Todd (2002) find little support for claims by e.g. Deheija and Wahba (1998, 1999), about the effectiveness of these estimators as a method for controlling for selectivity bias. They find that various cross-sectional matching estimators are highly sensitive to the choice of sub-sample and to the variables used to estimate the propensity scores. Smith and Todd (2002) also find that difference-in-differences matching estimators may perform better. As an explanation they point at possible problems with the data, for instance that the features (III) and (IV) mentioned above, are not achieved.

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[5] Matching here means pairing each programme participant with one (or several) non-participants, selected from the population of non-participants (without or with replacement). The pairs are constructed on the bases of identity or similarity in the X variables. The mean impact of the treatment on treated is then estimated by the mean differences in the outcomes of the matched samples.

[6] We do not estimate difference-in-differences for two reasons. First, it turns out that pre-training earnings differentials between participants and non-participants are very small and statistically insignificant. Second , pre-training are not observed for all cohorts in the data.

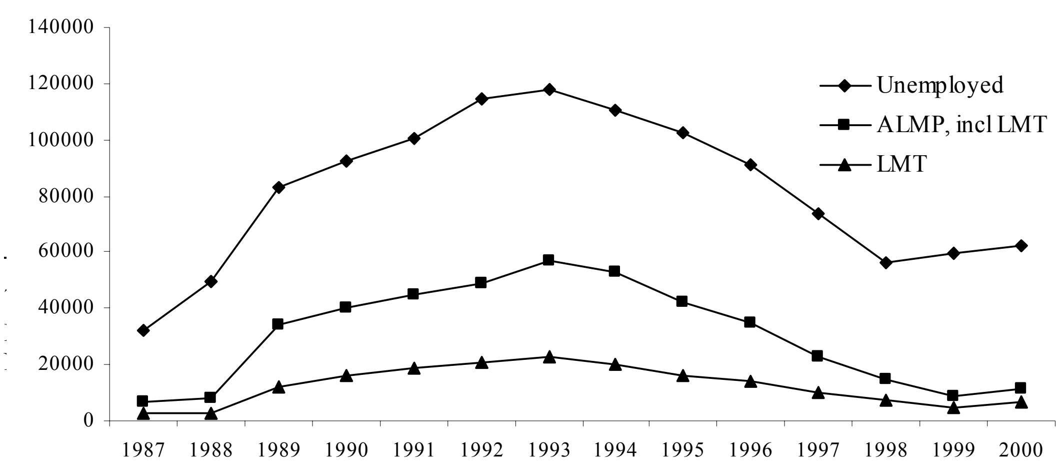
Of special interest for our study is the extension of the method from a conventional two-state framework to allow for the case with *multiple mutually exclusive states*, developed by Imbens (2000) and Lechner (2001a). Lechner (2001a) presents a *matching protocol*, suggesting a specific algorithm – with some variants - in four steps for estimating the treatment effects. As pointed by Lechner (2000a) this algorithm does not give asymptotically efficient estimators, because the trade-off between bias and variance is not addressed (the algorithm minimises the bias). See Hirano, Imbens and Ridder (2000) for a discussion of efficiency of estimators based on propensity score matching. More sophisticated and computer intensive matching estimators - that also control for *un*observables - are discussed by Heckman, Ichimura and Todd (1998).

5. Labour market policies and the business cycle in Norway during the 1990s

During the period covered by our data, 1990-97, unemployment has fluctuated as illustrated in Figure 1. In 1990 unemployment was relatively high by Norwegian standards and increasing with a peak in 1993. The unemployment rate peaked in 1993 at 5.5 per cent, increasing from 1.5 in 1987 and sliding back to 3.3 per cent in 1997 and 2.4 in 1998.[7]

The average number of persons involved in ALMPs increased from 7,000 in 1987 to 57,000 in 1993, showing how ALMPs are used to dampen the labour market effects of business cycles. During the bottom of the slow-down, 2.5 per cent of the labour force participated in these programmes. As most ALMPs last for less than half a year, the total number of persons participating in programmes *during* one year is about twice the participation rate at a point in time. From 1993 to 1997, the average participation in ALMPs decreased from 57,000 to 23,000. In 1999 the number of participants was as low as 8,000.

Figure 1. Unemployment, participants in active labour market programmes (ALMP) and training (LMT). Persons, annual average. 1987-2000.



The *Labour Market Training* programme is by far the largest programme, covering about 40 per cent of all ALMP-participants. The aim of LMT is to maintain and improve the skills of the unemployed and thereby to enhance their employability. The programme is organised as off-the job courses, mainly targeted at unemployed adults. Moreover, a substantial number of people (re-)enter the labour market via the training programme.

In the first training year of this study, 1991, the average number of participants was 19,000. Then it increased to 23,000 in 1993, before it started to decrease: in 1996 the average number of participants was 14,000, in 1999 only 4,500.

The programme is funded by the central government and organised by the local employment service under the supervision of the Directorate of Labour and the Ministry of Labour. The courses are provided by the employment service, often in co-operation with other public and private institutions. Vocational training is dominant and a wide range of subjects and crafts are covered. Most of the courses are short, from 5 to 20 weeks. In some cases there are basic courses and follow-up courses within the same subject, with a total duration of one year (or even more). LMT is available for all job seekers and participation is voluntary.[8] Unemployed persons who refuse to accept offers of training may lose their unemployment insurance benefit. This sanction is, however, rarely carried out.

The courses are free of charge. All participants are entitled to a training allowance, but recipients of unemployment benefits (UB) may opt to collect their benefits. Participants eligible to unemployment benefits typically keep these as the training allowance is lower. UB compensates about 62.4 per cent of previous earning, while the allowance is flat rated. Economic incentives to participate in LMT are driven by the training allowance, but also related to the eligibility and exhaustion of unemployment benefits. Time spent in LMT and the allowances collected *do not* qualify the participants for unemployment insurance.[9] But as unemployed not eligible for UB receive training allowances, they sure have economic incentives to take part in LMT.

The capacity of most courses is limited. The rate of rationing at each course depends on the number of qualified applicants related to the capacity of the course. Thus the recruitment to LMT is partly a self-selection process and partly an administrative selection process. Røed, Torp, Tuveng and Zhang (2000) have studied this recruitment process. Based on register data

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[8] For some courses applicants have to qualify through education, previous vocational training or work experience. to be eligible.

[9] According to the Norwegian system it is necessary to have earnings from an ordinary job to qualify for unemployment insurance benefits. Until 1997 earnings received during a temporary employment programme (but not in a training programme as LMT), counted as qualification for future unemployment benefits.

as well as interviews with the administrative staff at local branches of PES they point at a possible trend towards *positive selection to LMT*, i.e. participants have observed – and may be also unobserved - characteristics assumed to correlate positively with employability. They find it difficult to draw any conclusions on how this selection changes over the business cycle. It seems, however, that the positive selection is weaker when unemployment is low (as in 1997-1999) than when unemployment is high (as in 1992-1994). On the other hand the staff at PES reports that during the slump the capacity of LMT was sufficient to offer training to "everyone". During the boom the administrative staff had to be more selective. Courses directed at expressed needs of labour among employers were given priority, as were unemployed expected to be able to fill manifest vacancies.

Selection to LMT and the variation in selection over the business cycle may be captured by *observable* characteristics of the participants and the non-participants. Matching estimators of ATET will then be unbiased and any variation in the estimates over the business cycle will reflect that training effects do depende on labour market conditions in the post-programme period. However, if the difference is tied to *un*observables it will cause biased estimates of the effects, and this bias may change over the business cycle. Assume the positive selection of participants to LMT is weaker during the boom than during the slump, as indicated by Røed et al. (2000). Then we would expect the estimated effects to be less upward biased in the boom than in the slump. Thus the influence of a change in the selection bias will be to partly disguise any positive association between training effects and post-training job opportunities for the unemployed.

6. Data and design of study

The data are drawn from a large Frisch Centre database containing individual level information from numerous administrative registers, delivered by Statistics Norway. We select individuals from all entrants (and re-entrants) in the public unemployment register during December 1990 – July 1996. Our data from this register contain monthly observations of unemployment, labour market programme participation by type and unemployment benefit entitlement. From this population we select 12 cohorts of LMT participants, two cohorts every year from 1991 to 1996.

We use annual labour earnings 1992-1997 measured in Norwegian 1997 kroner to estimate the impact of the programme. A large number of individual pre-training characteristics are available. The comprehensive and detailed data sources constitute a solid basis for evaluating the effects of the LMT program throughout the first half of the 1990s. The data enable us to study the extent to which training effects vary with the state of the labour market, i.e. job opportunities, and how training effects evolve as post-training time prolongs. In this section we describe the data which then is used to model the selection into training and the creation of comparison groups of non-participants (section 7) as well as estimating the training effects (section 8).

Participants and non-participants

LMT courses typically start in August or September and then there is another wave of courses starting in January and February. The composition of training courses does not differ substantially between the autumn and winter seasons. As the majority of courses last for 5-20 weeks, most courses in the autumn are completed by the end of the year, but in some cases continuation courses start early next year. Most winter courses end before the summer, while some continue after the summer holiday. Since the post-programme success of the training is measured by annual earnings, and the time passed after having completed the training may affect the impact on earnings, it is preferable to analyse the impact of autumn and winter courses separately. We then have 12 (training year*season) cohorts, where each cohort is split into four groups by gender and unemployment benefit entitlement. We restrict ourselves to participants aged 25-50, since selection into other programmes and education, as well as labour market behaviour in general, are different for teen-agers and young adults. The upper age limit set is to avoid transitions out of the labour force due to early retirement or disability pension which become increasingly important as we include unemployed in their fifties and sixties.

The participants and non-participants are defined by the same procedure across cohort groups. The population of potential LMT participants consists of all fulltime unemployed persons registered at the end of December and July, for the winter and autumn cohorts respectively.[10] Then we consider the register status two months later, i.e. at the end of February and September, respectively. LMT participants constitute the treatment group. In order to define a suitable comparison group we divide non-participants into three groups according to their status in the register; still unemployed (U), participating in another labour market programme (PROG), or having left the unemployment register (OUT). Those who leave enter jobs or exit from the labour force, but we cannot distinguish between the two transitions.

[10] By this sample restriction we exclude LMT participants who enter training directly from outside the register.

The comparison group is selected among those still unemployed.[11] From these individuals we select non-participants who are “observationally equivalent” to the participants, as far as pre-training characteristics are concerned. The logic behind this matching, how it is implemented and the results of procedure are described in following section.

In Table 1 we report the sample sizes of the different cohorts, by group. The sum columns two to four constitutes the populations at risk, defined as the members of unemployment stock two months before and each column shows how they are distributed according to LMT, PROG and OUT transitions. The U-group is those still unemployed. Cohort W91 consists of all fulltime unemployment in December 1990 and their status at the end of February 1991, cohort A92 consists of all fulltime unemployment in July 1991 and their status at the end of September 1991, and so on.

The samples of participants vary between 600 and 2,500 individuals. Unemployed without unemployment benefits (No UB) are more likely to enter training. Among those with UB, men and women are equally likely to participate in training. For those without UB, more women than men enter the training programme.

[11] Lechner (2001b) estimates the impact of four different programmes on employment (relative to non-participation), measured by number of days employed during limited a post-programme period (per cent), by data from the Swiss canton of Zurich. The paper presents and compares different estimators of the causal impact. It is shown that effect based on a comparison of a treatment group to an aggregated comparison group of individuals has no meaningful causal interpretation, while pair-wise effects give clear-cut causal effects.

Table 1. Sample sizes and transitions from fulltime unemployment, 1991-1996.

Males, UB

Cohort	No trans. U	Transition to OUT	PROG	LMT	LMT rate
W91	12427	4794	704	1238	0.065
A91	16079	7856	1295	2185	0.080
W92	19699	5806	989	1558	0.056
A92	19144	8643	1612	2491	0.078
W93	21442	5374	1362	1900	0.063
A93	18157	8842	2379	2206	0.070
W94	18882	5322	1559	1566	0.057
A94	14250	8269	1981	1777	0.068
W95	14594	4664	966	862	0.041
A95	12034	6887	1402	1484	0.068
W96	11445	4161	889	836	0.048
A96	10325	5781	941	1086	0.060

Females, UB

Cohort	No trans. U	Transition to OUT	PROG	LMT	LMT rate
W91	8054	4328	545	907	0.066
A91	10636	7303	766	1964	0.095
W92	11298	3850	509	988	0.059
A92	13368	8538	1151	2110	0.084
W93	12831	4075	906	963	0.051
A93	13014	8872	1739	1752	0.069
W94	12141	3717	1022	853	0.048
A94	11714	8502	1816	1893	0.079
W95	11055	3815	744	695	0.043
A95	11365	8312	1404	1618	0.071
W96	9631	3951	725	711	0.047
A96	10495	7704	1192	1351	0.065

Males, No UB

Cohort	No trans. U	Transition to OUT	PROG	LMT	LMT rate
W91	3818	2307	317	638	0.090
A91	4688	3068	512	1055	0.113
W92	5422	2310	463	733	0.082
A92	5427	3384	574	1132	0.108
W93	6065	2461	385	807	0.083
A93	6828	3562	740	1127	0.092
W94	7441	2954	543	844	0.072
A94	7183	3990	884	1237	0.093
W95	7933	2940	492	634	0.053
A95	7227	4071	763	1186	0.090
W96	6679	3355	467	685	0.061
A96	6902	3713	654	982	0.080

Females, No UB

Cohort	No trans. U	Transition to OUT	PROG	LMT	LMT rate
W91	2315	1684	237	595	0.123
A91	3396	2671	495	1540	0.190
W92	3429	1792	345	760	0.120
A92	3901	2860	518	1705	0.190
W93	3928	1923	339	776	0.111
A93	4756	3171	782	1561	0.152
W94	4628	2349	451	735	0.090
A94	5112	3428	949	1914	0.168
W95	5988	2325	393	734	0.078
A95	6060	3755	874	1942	0.154
W96	5050	2892	465	857	0.093
A96	6192	4019	835	1762	0.138

W9j = Winter 199j , A9j = Autumn 199j, j = 1,..6.

The transition probabilities are estimated for each of the 48 sub-samples as functions of a large number of individual characteristics. From the unemployment register we collect information on pre-training labour market program participation, unemployment record, previous occupation and unemployment benefit entitlement (UB). In addition we have register information on age, gender, material status, number of children, educational attainment, work experience measured by yearly pension points (proportional to earnings), immigrant status and school enrolment during the previous six months. Fixed county of residence effects, measured at the time when training starts, is used to control for variations in local labour market conditions and supply of labour market programmes. All these variables are used to model the transitions from unemployment, including enrolment into LMT, see section 7. More information on the individual characteristics is given in the Appendix.

Earnings profiles of participants

According to the Norwegian labour market authorities, the main objective of LMT is to increase the ability of unemployed to get permanent jobs. Even if employment is the overall goal of the programme, several arguments favour the use of *post-training earnings* to measure programme effects. The first argument is *relevance*. Post-training earnings in year t (Y_t) can be decomposed into days of employment (e_t), average hours per day employed (h_t) and average wages per hour (w_t), which gives: $Y_t = e_t h_t w_t$. Here e_t measures how quickly the person enters employment as well as the stability of the job. h_t depends on opportunities, qualifications and preferences of the individual. Part-time unemployment is common among LMT applicants, indicating that many are rationed with respect to working hours. If the training effect on earnings is due to longer daily working hours, this should be considered as a success in line with (re-)employment. The hourly wage reflects productivity and the quality of the employment match. If LMT contributes to more productive employees and a better matching, these effects are obviously socially beneficial. As the Norwegian wage structure is fairly compressed; see e.g. Barth and Zweimüller (1994), earnings mainly reflect the duration of employment. If there is a positive effect of training on earnings we do not expect wage increments to be an important explanation. Finally, cost-benefit comparisons also favour earnings as a measure to evaluate the programme effect.

In line with the objective of LMT, earnings should include wages as well as income from self-employment. Transfers, unemployment benefits, social support and training allowances ought to be excluded. Our data on earnings are collected from public tax- and wage-registers. Unemployment benefits are subtracted, implying that our earnings are very close to income from work, including earnings from self-employment.[12]

Since Ashenfelter (1978), studies of programme effects have been concerned about the earnings dynamics. Participants typically experience that earnings drop prior to the training period and gradually increase during the post-programme period. Figures 2 and 3 illustrate the mean earnings profiles of the 1995 cohorts, by gender and unemployment benefit status.

The "Ashenfelter-dip" is clearly experienced by those with unemployment benefits. The earnings profiles of the groups without benefits are clearly very different, illustrating that LMT is a potential stepping stone in the process of (re-)entering the labour market. One might

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[12] The available measure of income, "Income Qualifying for Pension" (PI), includes unemployment benefits and wage earnings from various labour market programmes. Training allowances are not included. We are adjust the PI for unemployment benefits, but earnings from participation in other labour market programme than training are difficult to sort out and are therefore included.

also suspect that financial incentives (i.e. training allowances) make it economically wise to spend time on LMT during this process, even if the effects on future labour market prospects from this investment are minor. Anyhow, Figures 2 and 3 clearly motivate our split by gender and unemployment benefit eligibility when we estimate earnings effects of LMT.

Short and medium run effects

The earnings data presently available cover the years 1992-1997. Consequently, we estimate first year (short run) effects for all cohorts, while second and third year effects can only be estimated for the first ten and eight cohorts, respectively. The Norwegian business cycle turned some time during 1993, which means that the variation in job opportunities is somewhat limited when we consider the effects beyond three years. In a companion paper, Raaum, Torp and Zhang (2002b) we compare individual long run effects and direct programme costs of LMT, focusing on participants in 1992 and 1993.

7. Selection on observables and matching

This section describes how the comparison groups of non-participants are established and used to simulate the counterfactual outcome of LMT participants, i.e. $E(Y_0 \mid X, D=1)$. There are various matching techniques and estimators used in the evaluation literature. In this study we apply a variant of traditional pair-wise nearest-neighbour-matching in the case of a multinomial choice model, inspired by the *matching protocol* suggested by Lechner (2001a).

We start out with the population of all fulltime unemployed, registered at time *t,* who are eligible for the LMT programme. Each member faces several options, here specified as four mutually exclusive states at time $t+dt$: to remain unemployed (U), to take part in the programme to be evaluated (LMT), to take part in another programme (PROG), or to leave the unemployment register (OUT). We use a multinomial logit model to estimate the probabilities of each state at time $t+dt$, as functions of a large number of individual characteristics at time *t*. dt is approximately two months. As we have 12 cohorts split by gender and unemployment benefit status, we estimate and predict probabilities for 48 different samples.

The estimated parameters from the multinomial logit model are used to predict the probabilities of LMT, PROG and OUT for each individual in the subsample of participants (LMT=1) as well as for all those still potential participants when the programme starts, i.e. unemployed non-participants (U=1).

Figure 2. Earnings profiles. Participants Winter 1995. By gender and unemployment benefit.

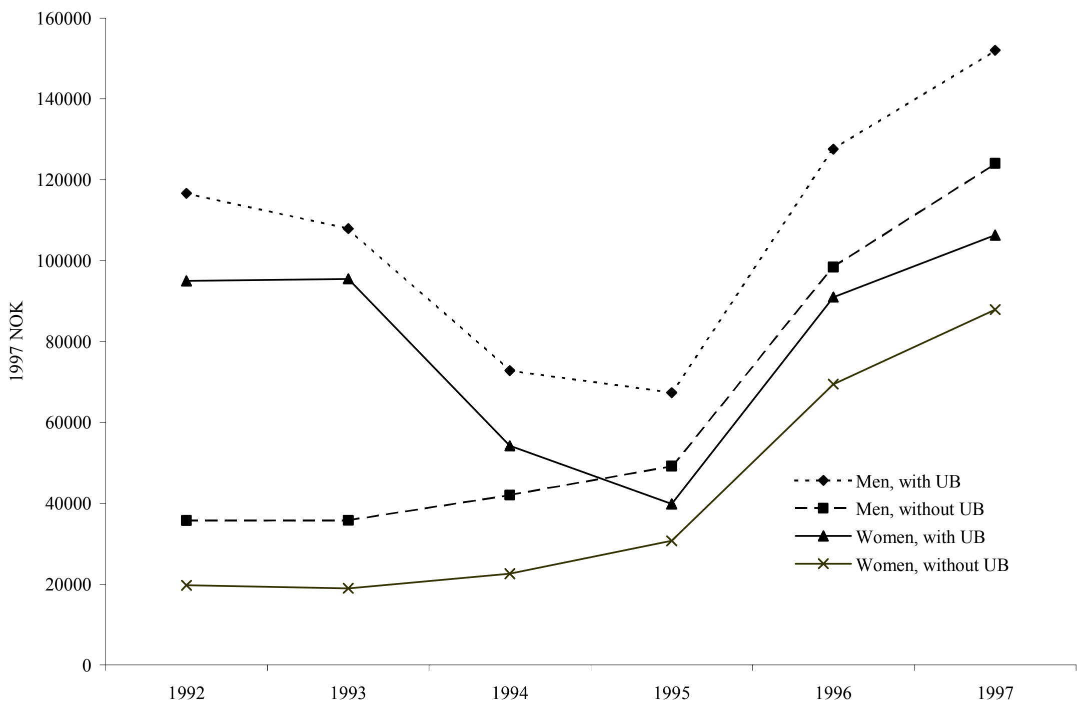
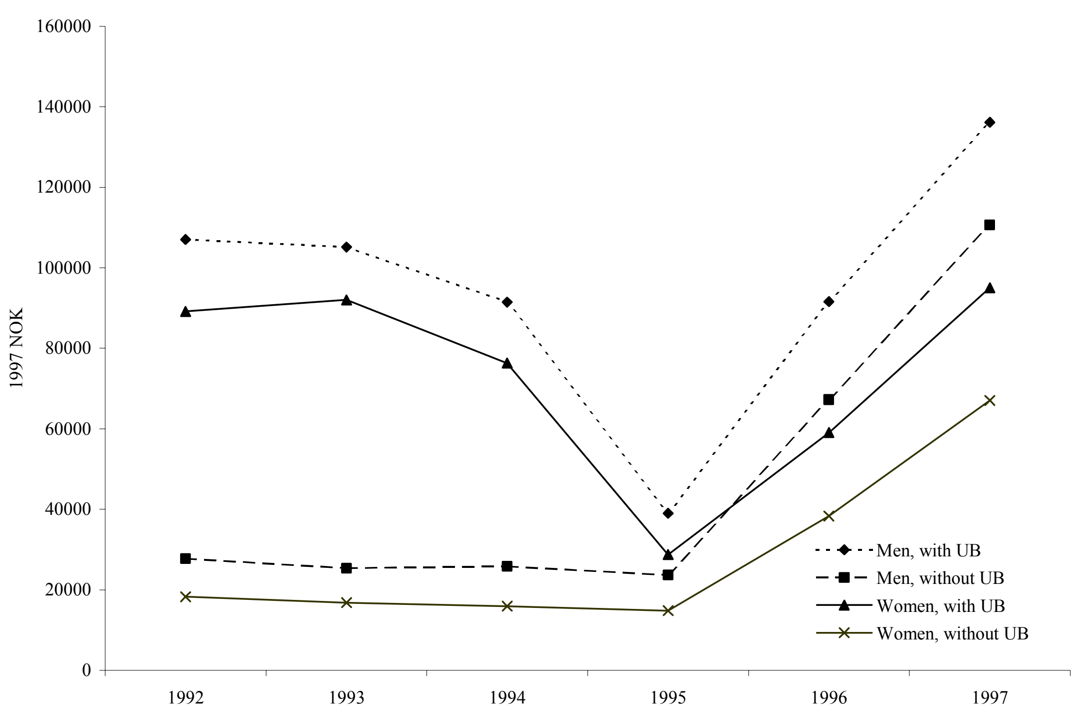


Figure 3. Earnings profiles. Participants Autumn 1995. By gender and unemployment benefit.



To eliminate as much as possible of the potential selection bias, we select unemployed non-participants with the same predicted structure of transition probabilities as those in the treatment group. The first step in the matching procedure is to exclude observations outside the common support, i.e. we exclude observations from the sample of participants with estimated probabilities that are larger than the maximum value of the same probabilities in the comparison group. Similar we exclude observations from the LMT-sample with estimated probabilities that are smaller than the minimum value of the same probabilities in the comparison group. Then we use the same procedure to exclude observations in the comparison group with estimated probabilities outside the range of the probabilities in the LMT-sample.[13] This defines the common support samples.

Next we take each observation from the sample of participants and search through the comparison group to find the closest match based on the three estimated probabilities. In this process we use the *Mahalanobis metric* as a measure of distance with the inverse covariance matrix from the original gross sample as weights; see Rubin (1979). Since we keep the matched comparison group member (i.e. matching with replacement), a single observation may be used several times. In our case, however**,** a limited number of non-participants are used more than once. Accross cohorts and groups, 5 to 12 per cent are used twice, up to 3 per cent are useed three times as control while up to 2 per cent are used three times or more.

What explains participation?

In a separate working paper, Raaum, Torp and Zhang (2002a), we report the estimates of the multinomial logit model for selected subsamples, women and men, with and without unemployment benefits for some cohorts. The observables used to estimate the propensity scores are defined in the Appendix of this paper. The estimations show that various explanatory variables have some influence on the transitions from unemployment to the three other states. The partial impact of most variables differs, however, across subsamples.

When it comes to the relative probability of LMT, there are some robust patterns. First of all, the probability is higher for those who participated in LMT the previous quarter as well, ceteris paribus. This parameter is significantly positive for most subsamples. Next, those with a fairly long unemployment record, i.e. 11 months or more, are less probable to participate in

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[13] We compare one probability at the time: First we accept all observations from the comparison group with estimated values Prob(LMT=1| X) within the range of estimated values of Prob(LMT=1|X) for the participant group. Next we accept all observations from the treatment group with estimated values Prob(LMT=1| X) within the range of estimated values of Prob(LMT=1|X) for the comparison group. Then we proceed with similar comparisons of estimated values Prob(OUT=1|X) and Prob(PROG=1|X) for both samples.

LMT (relative to stay in U). We also find that for a majority of the subsamples the relative probability of LMT is larger for immigrants than others. This may mirror the fact that LMT includes special courses target at this group. The partial impact of age seems to be negligible (ceteris paribus), even if those aged 46-50 years are less apt to participate in LMT for some subsamples. Education is somewhat more important, as low education (10 years or less) and unknown education is negatively correlated with the relative probability of LMT. We also find substantial regional differences. For many of the subsamples the relative probability of LMT is larger in the northern counties of Norway (Finmark, Troms, and Nordland) than in the southern and central parts. This illustrates the importance of comparing participants and non-participants from the same location if we are to eliminate the misweighting on observables.

Matching results

We assess the success of our matching procedure in two ways. First, we compare the distributions of the predicted probabilities among (i) the participants, (ii) all potential unemployed non-participants and (iii) the unemployed non-participants picked by the matching procedure. Next we compare mean predicted probabilities and average pre-training characteristics among participants and the matched non-participants.

As illustrated in Table 1 the number of observations in the group of unemployed non-participants is much larger than the number of observations in the group of participants. This holds for all 48 subsamples. This simplifies the matching. The common support criteria (based on the predicted probabilities) leaves out rather few observations. Accross cohorts and groups, less than 5 per cent of the unemployed non-participants (on average about 1.5 per cent) and less than 2 per cent of the participants (on average about 0.5 per cent) are excluded because they do not meet this common support criterion, see Raaum, Torp and Zhang (2002a) for details.

Figures 4 and 5 present plots of the predicted probabilities of Prob(LMT=1), Prob(OUT=1) and Prob(PROG=1) for the two 1991 cohorts, respectively.[14] In each panel there are three lines. The line marked with *squares* is for all unemployed non-participants. For the predicted values of Prob (LMT=1) this line is (in general) to the left of the two other lines.

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[14] Plots are estimates of *Epanevhnikov Kernel* densities on predicted probabilities P_i(I=LMT,OUT). Bandwidth is estimated by $h=0.9m/(n^{1/5})$, where m=min(sqrt(variance(p_i), interquartilerange(p_i)). The densities are estimated with *STATA,* see "Reference Manual, [R] kdensity" (2001), *Stata Statistical Software, Release 7.0*, StataCorp.

Figure 4. Predicted probability distributions Prob(LMT=1), Prob(OUT=1) and Prob(PROG=1). Cohort winter 1991.

—o— LMT participants ----Δ--- Matched non-participants ......□....... All unemployed

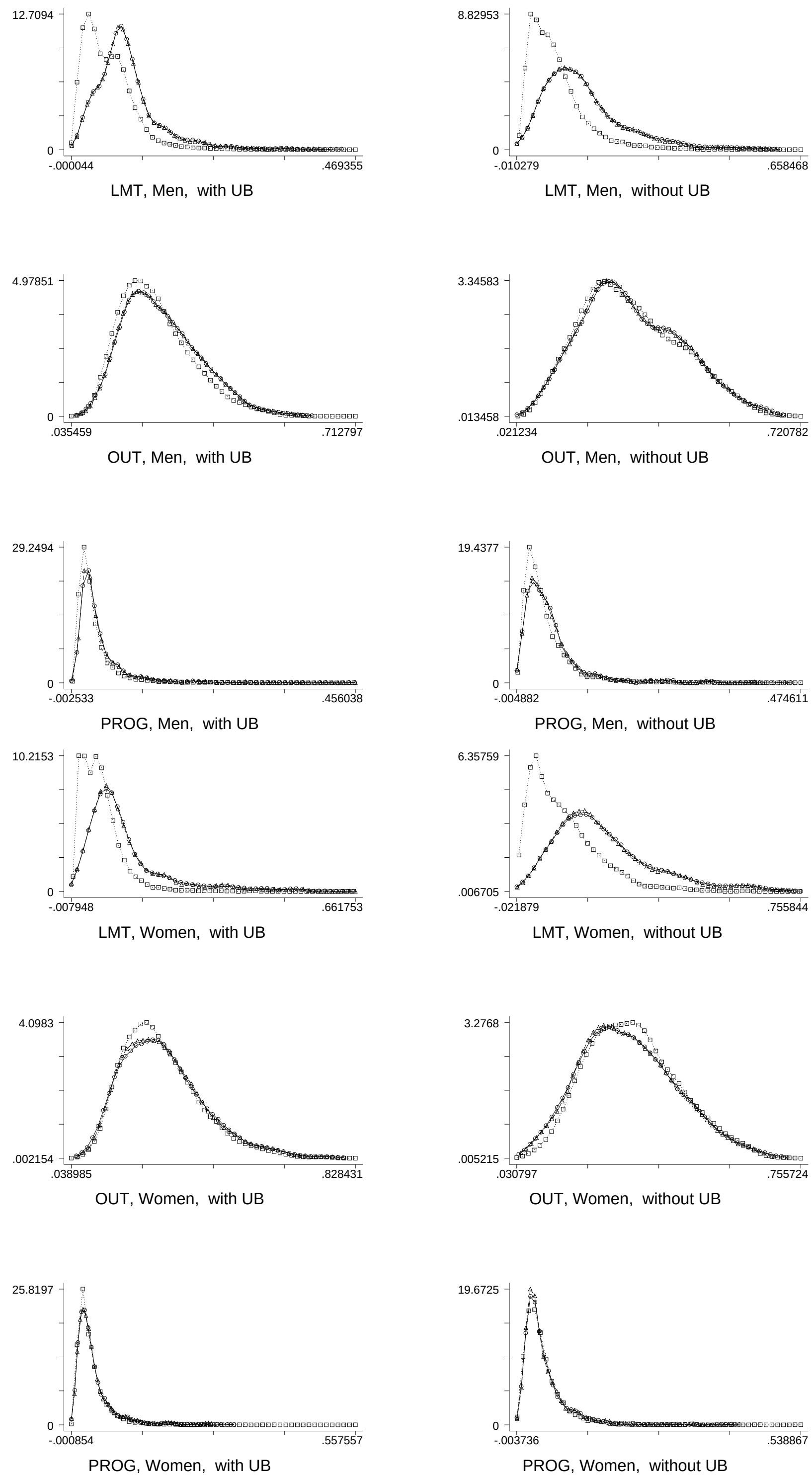
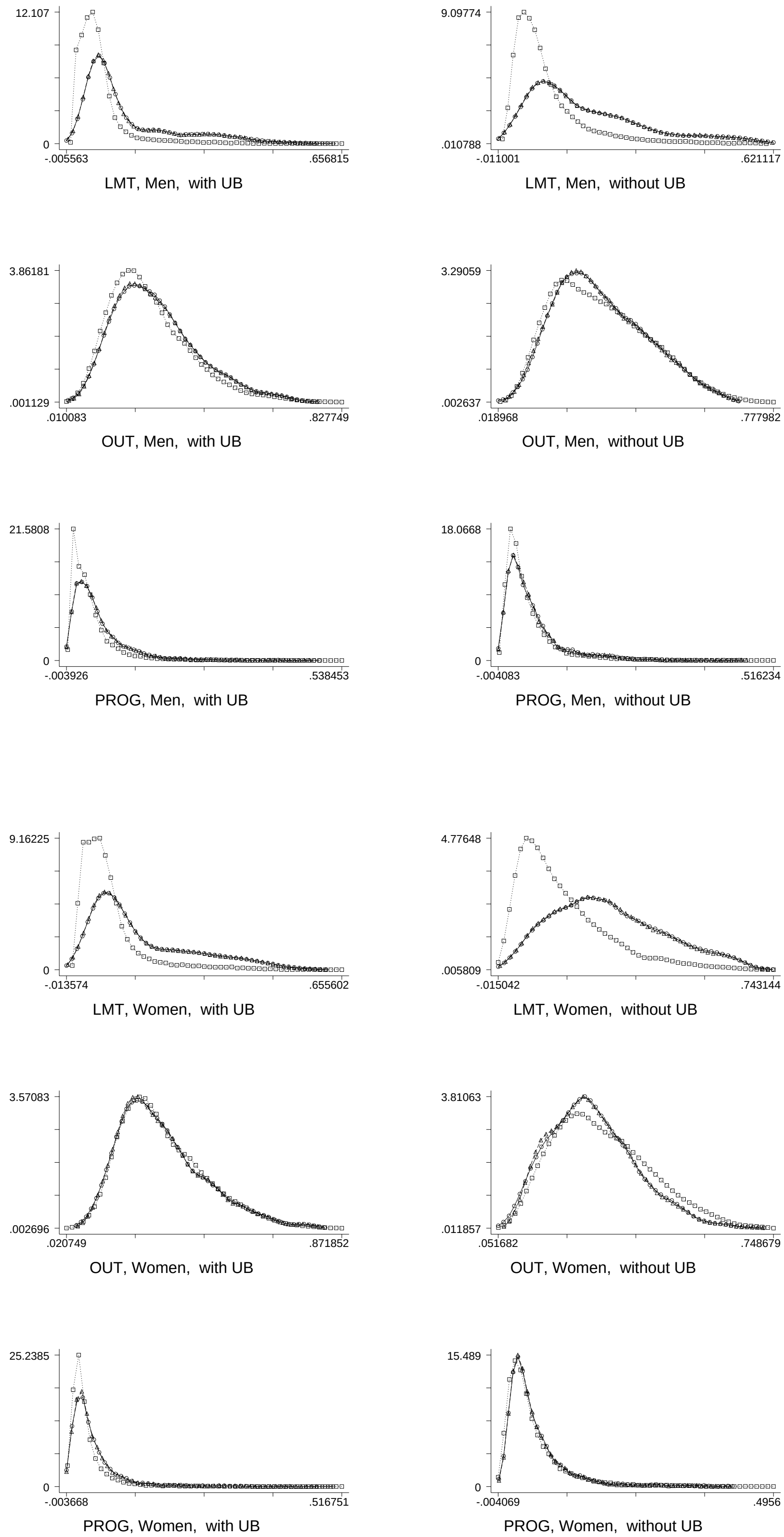


Figure 5. Predicted probability distributions Prob(LMT=1), Prob(OUT=1) and Prob(PROG=1). Cohort autumn 1991.



The thicker left-side tail indicates more people with a low probability of LMT=1. When it comes to the predicted values of Prob(OUT=1) the difference between the three lines is not as large (and the line marked with squares is often to the right, indicating more people with a high probability of OUT=1).

The two other lines are for the matched samples, non-participants marked with *triangles* and participants marked with *circles.* As can be seen these two lines are very close both for predicted values of Prob(LMT=1) and the predicted values of Prob(OUT=1). The closer the lines, the more successful is the matching with respect to the propensity scores.

Figure 5 presents similar panels for the subsamples of Autumn 1991 cohort and reveals that patterns are quite stable across cohorts.[15] For all subsamples the matching seems pretty successful, see Raaum, Torp and Zhang (2002a) where similar plots for all cohorts are included.

The success of the matching procedure can also be assessed by studying the differences in mean propensity scores and X-variables included in the multinomial choice model. In Appendix we present mean predicted propensity scores as well as mean values of X-variables *after matching* for selected cohorts, in Tables A1 and A2, respectively. Generally the predicted probabilities are very close. Comparing the mean values of the predicted probability of taking part in LMT (LMT=1) for participants and non-participants we typically find differences less than 0.5 per cent, Table A1. Similar and small differences are found for predicted probabilities of PROG=1 and OUT=1, see Raaum, Torp and Zhang (2002a) for the other cohorts. As expected from the similarity in predicted probablities, mean values of pre-training observables are very similar for participants and matched non-participants, see Table A2 in Appendix.

Cross over and substitution

Participation is defined according to training status by the end of February (Winter) and September (Autumn). The majority of courses start in the beginning of the term, i.e. January/February and August/September. Non-participants are not excluded by administrative procedures, nor by our matching procedure, to start training later on, either in the same term (January –June, July-December) or in the next.

[15] Similar illustrations for the other cohorts as well as for the predicted values of Prob(PROG=1) and Prob(U=1) can be found in Raaum, Torp and Zhang (2002a).

Table 2. Participation in LMT. Cross over and substitution. Fraction by gender and unemployment benefit entitlement. Average across all cohorts.

	Cross over	Substitution ("delay")	
Period:	Same term	Next term	Two terms later
Male, UB			
Participants	1	0.4842	0.2164
Non-participants	0.0717	0.1207	0.1024
Female, UB			
Participants	1	0.5207	0.2214
Non-participants	0.0588	0.1089	0.1002
Male, No UB			
Participants	1	0.5186	0.2607
Non-participants	0.0781	0.1466	0.1313
Female, No UB			
Participants	1	0.6067	0.2965
Non-participants	0.0928	0.1576	0.1422

If members of the comparison group start in LMT the same term (6 months), they are characterised as cross-overs. If they enrol during the two following terms, we label it substitution. In Table 2, we first report the average fraction of cross-overs and find that about 6 to 9 per cent of the non-participants do enroll in training during (i.e. later in) the training term. Second, between 10 and 15 percent of the non-participants turn up as participants during the following terms. Participants, however, are much more likely to be enrolled in the following two terms. This is partly due to courses with long durations that stretch into the next term. There in no strong indication of inter-temporal substitution in the sense that participation is delayed for a substantial fraction of the non-participants.

8. Training effects

The individual effects are estimated by group (i.e. gender, cohort and unemployment benefit entitlement) for each of the three post-training years.[16] All effects are *average training effect on the trained*, simply defined as the mean earnings of the participants minus the mean of the matched unemployed non-participants, as explained in section 3. The main results are presented in Table 3, where we have aggregated the cohort-specific training effects.

Table 3. Average effects of training on annual earnings (NOK 1997). Average over cohorts.

	First year effect		Second year effect		Third year effect	
Season	Winter	Autumn	Winter	Autumn	Winter	Autumn
Male, UB						
Average effect	11,120	-3,755	14,052	8,127	13,517	9,142
Std.error	3,826	2,618	3,825	2,992	3,767	3,085
# positive effects	5	0	5	3	4	4
# negative effects	0	4	0	0	0	0
# insignificant effects	1	2	0	2	0	0
Female, UB						
Average effect	11,966	-6,316	17,113	8,762	20,215	14,107
Std.error	3,379	1,931	3,631	2,300	3,742	2,425
# positive effects	5	0	5	4	4	4
# negative effects	0	1	0	0	0	0
# insignificant effects	1	5	0	1	0	0
Male, no UB						
Average effect	8,851	-34	11,940	10,134	10,935	11,587
Std.error	4,780	3,252	5,199	3,893	5,472	4,242
# positive effects	2	0	4	3	2	2
# negative effects	0	1	0	0	0	0
# insignificant effects	4	5	1	2	2	2
Female, no UB						
Average effect	7,931	-9,645	10,102	-1,533	10,574	2,675
Std.error	3,224	1,713	3,594	2,077	3,611	2,228
# positive effects	2	0	3	0	2	1
# negative effects	0	5	0	1	0	0
# insignificant effects	4	1	2	4	2	3
Training years	*1991-1996*	*1991-1996*	*1991-1995*	*1991-1995*	*1991-1994*	*1991-1994*

Note: Average effects are weighted by the number of participants in each cohort.

[16] In total, 120 (=48+40+32) effect estimates. All are presented in the Appendix, Tables A3-A5.

The weighted average training effect, with the corresponding standard error, is reported together with the number of statistically significant positive and negative effects.

The effects differ by UB status and season. Consider first the group who collected unemployment benefits at the time of enrollment (two first panels). The training effects are positive and significant, in economic as well as in statistical terms. The only exception is the first year effects for Autumn courses. These negative effects are likely to reflect the short time span between the training and the outcome periods, amplified by the continuation of a training period into the next calendar year, among autumn course participants. In practice, what we call the post-training period actually include periods on training for more than one third of the participants.[17]

For winter courses, annual effects vary between 11, 000 and 22,000 NOK. The effect is positive for most groups and outcome years. The effect of Autumn courses varies between – 6,300 and 17,100 NOK. Ignoring the (negative) first year effect for Autumn courses, 44 of 48 effects are significantly positive. The effects of Winter and Autumn courses tend to converge as we expand the distance between the training and the post-training period. We find relatively small differences between men and women. If any, women seem to gain more than men.

The results are more mixed for the participants without unemployment benefits. These indiviuals are typically in the process of entering the labour market and the training effects are less favourable. The effects differ by season and gender. Winter courses have positive effects, both for men and women, but they are not always significant in statistical terms. Half of the estimated effects (15 out of 30) are significantly positively, while the rest are not different from zero. There is no obvious gender difference for the Winter courses. The first year effects of the Autumn courses are again negative, but males without UB entitlement have similar training effects as males with UB. For women without UB who started training in August-September, however, the training effect is close to zero.

In total, Table 3 shows that the LMT programme raises the earnings of the participants. Assessing the effects three years after training, significantly positive effects are found for 23 of the 32 groups (cohort*gender*UB entitlement) and the average effect on annual earnings is more than 10 000 NOK.

The estimated training effects are significant and some may find them suspiciously large, given that the typical participant spent 4-7 months on training during the training period. We are not extremely successful in modeling the selection into training and critical observers

[17] In Raaum and Torp (2001) we study training starting in August-September 1991, but define 1993 as the first

may argue that many of the unobserved characteristics which determine participation is likely to be correlated with earnings potential, violating the CIA. From pre-training earnings records we can gain more confidence in the consistency of our estimates. By looking at whether individual earnings are correlated with *future* LMT participation, we test the joint null hypothesis that CIA holds for post-training earnings *and* the over-identifying restriction that pre-training earnings are uncorrelated with unobserved characteristics determining participation, see Heckman and Hotz (1989).[18] Of course, this test has no power with respect to an alternative null where only post-training earnings are correlated with training status via unobserved characteristics.

We cannot perform the pre-training test on all cohorts since earnings are not observed in our data before 1992. Hence, we can only test from cohort five (winter 1993) onwards. In Appendix, Table A6, we report estimates of the earnings differential between participants and (matched) non-participants in Y_{k-s} where k is the training year and s varies between one and four. The results are clear. For none of the cohorts, groups or pre-training years we are able to reject the null. Pre-training earnings are not (significantly) different for participants and non-participants. Even the point estimates are generally low and we find positive as well as negative pre-training differentials. There are, however, some indications that post-training earnings of participants are somewhat higher among 1995-96 cohorts of males receiving UB.

Ideally one would like to have an internal comparison group of rejected applicant to measure the counterfactual outcome for participants, see e.g. Raaum and Torp (2002). In our previous study of training effect we argue that; "Our data indicate that training programmes attract applicants with better employment prospects than the average unemployed. This kind of self-selection, e.g. on post-training variables, is hard to identify and correct for". However, the magnitude of the bias is not very large. Moreover, our previous recommodation follows from a study with a stock-sampled comparison group without matching. We believe our previous warnings about external comparison groups do not necessarily undermine the strategy in this paper.

Finally, it is worth noticing the considerable heterogeneity in training effects across groups and outcome years. Evaluation studies typically study a limited number of cohorts and outcome periods. Our results illustrate the potential problem of low external validity, at least in studies of Norwegian data.

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outcome year.

9. Are training effects higher when job opportunities are favourable?

With the average training effects at hand, we are able to test the hypothesis that individual programme effects are higher when labour market conditions are favourable. This section offers a meta-analysis of the large number of estimated training effects, exploiting variations in job opportunities *during the post-training periods* between men and women, across time and regions. First, we consider the training effects reported in the previous section and investigate whether these effects correlate with job opportunity indicators at the national level. Second, we disaggregate by geographical region and test whether county-specific training effects vary systematically with local labour market conditions, across and within counties. Although the main focus is on the association between training effects and the business cycle, we also investigate whether there are systematic differences in training effects between men and women or by unemployment benefit entitlement.

9.1 Macro Variation in Job Opportunities

We use two indicators of how job opportunities vary over time and by gender. First, we consider the average annual unemployment rates from the Labour Force Surveys (LFS) for those aged 25-54, by gender. However, the unemployment stock is likely to be a noisy measure of variation in job opportunities over time and across groups, because it is heavily influenced by the *inflow* into unemployment. The gender-specific outflow rate from unemployment to employment is likely to be a better measure of job opportunities for the unemployed. This motivates our second indicator, which is a human capital adjusted unemployment outflow rate calculated for the 1990's by means of data covering all unemployment spells in Norway.[19] The yearly indicator is equal to the average monhtly exit rates for prime aged unemployed receiving unemployment benefits, evaluated at mean value of observables like age, schooling, marital status and unemployment duration. While the outflow rate follows the LFS unemployment rate over time, the two indicators differ systematically when job opportunities are compared across gender, see Figure A1 in Appendix. The LFS unemployment rate indicates a more favourable labour market for women than for men, up to 1996, while the outflow rates (at any given year) show that job opportunities among unemployed men are considerably better than for unemployed women.[20] Tables 4 and 5 describe how the cohort-

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[18] The matching means that we do not have to worry about observables.

[19] The cohorts used to estimate the LMT effecst are extracts from this complete data base. Further details on the data and the duration model used to estimate the outflow rates are given in the Appendix.

[20] This is consistent with a lower inflow to unemployment among women, see Brinch (2000) for Labour Force

and group-specific training effects reported in section 8 correlate with the two job opportunity indiators (JOI's). We also include season, gender and UB entitlement dummies.

Table 4. The Impact of Job Opportunities on First year Training Effects. Estimated OLS parameters (standard errors).

	Without Job Opportunity indicator		**Job Opportunity indicators**			
			Unemployment rate (%)		Unemployment outflow rate	
Unemployment rate (%)			-5,872 (1,021)	-5,728 (991)		
Unemployment outflow					361,612 (61,350)	357,181 (58,657)
Winter	16,102 (1,614)	16,071 (1,573)	15,980 (1,227)	15,953 (1,188)	15,967 (1,215)	15,940 (1,161)
No UB	-1,950 (1,427)	1,731 (2,460)	-2,501 (1,089)	536 (1,868)	-2,551 (1,078)	833 (1,820)
Female	-3,745 (1,518)	-1,449 (1,946)	-9,500 (1,527)	-7,473 (1,802)	1,635 (1,462)	3,674 (1,664)
Female*no UB		-5,414 (2,982)		-4,447 (2,258)		-4,965 (2,200)
Constant	-3,061 (1,441)	-4,470 (1,608)	863 (1,293)	-391 (1,405)	-4,777 (1,126)	-6,048 (1,214)
R^2	0.7228	0.7426	0.8434	0.8566	0.8467	0.8633
# observations	48	48	48	48	48	48

Note: Dependent variable is the average training effect = mean earnings differential between participants and non-participants; by gender, UB entitlement, season and training year. OLS weighted by the inverse of the standard error of the estimated training effect. Reference group is men, with UB on Autumn courses. The unemployment outflow is measured as deviations from the mean.

Survey (LFS) evidence. Actually, a comparison of LFS-based outflows from unemployment to employment, see Brinch (2000), reveals that rates are higher for men (Figure V.3.3) than for females (Figure V.9.3) during the first half of the 1990's.

In the preferred specification, the gender differential differs by UB entitlement. As noted in section 8, the first year effect is negative for the Autumn courses but significantly positive for Winter courses, see second column Table 4. The average second year effect is positive for both seasons, see Table 5. No significant difference is found between males with and without UB entitlement. While men and women with unemployment benefits have about the same effect of training, there is a significant gender difference in favour of men for participants without UB entitlement.

Table 5. The Impact of Job Opportunities on Second Year Training Effects. Estimated OLS parameters (standard errors)

	Without Job Opportunity indicator		**Job Opportunity indicators**			
			Unemployment rate (%)		Unemployment outflow rate	
Unemployment rate (%)			-4,340 (1,416)	-4,175 (1,259)		
Unemployment outflow					245,941 (99,380)	242,913 (88,473)
Winter	7,707 (1,851)	7,669 (1,671)	7,614 (1,668)	7,581 (1,482)	7,707 (1,732)	7,670 (1,542)
No UB	-6,292 (1,726)	187 (2,695)	-6,559 (1,557)	-331 (2,382)	-6,559 (1,618)	-136 (2,477)
Female	-2,306 (1,797)	1,620 (2,104)	-5,565 (1,937)	-1,674 (2,105)	1,943 (2,403)	5,781 (2,458)
Female*no UB		-9,780 (3,311)		-9,387 (2,922)		-9,691 (3,039)
Constant	9,623 (1,706)	7,309 (1,735)	1,1687 (1,678)	9,388 (1,653)	7,233 (1,866)	4,969 (1,807)
R^2	0.5013	0.6009	0.6068	0.6984	0.6009	0.6733
# observations	40	40	40	40	40	40

Note: See Table 4.

Including the LFS unemployment rate, we find that a one percentage point increase in unemployment is associated with a reduction in the first year effect of about 6,000 NOK and somewhat less for the second year, see column four in Tables 4 and 5. The gender differential is amplified because women, according to the LFS unemployment rate, met more favourable labour market conditions.

While the estimate for the stock indicator can be interpreted directly, the impact of the outflow measure needs further explanations. The average monthly exit rate is about 0.06. If the outflow rate increases by 0.01, the estimated first year effect increases by around 3,500 NOK. Comparing the 10th and the 90th percentile in the observed outflow distribution, the predicted first year training effect difference is around 17,500 NOK. The impact of job prospects on training effects is somewhat weaker for the second year, but it remains significant, see Table 5, column six.

We find a statistically as well as economically significant assosciation between post-training job opportunities and average training effects. The gender difference in training effects is, however, sensitive to the type of JOI used. While the outflow specification attributes a significant part of the lower female training effect to less favourable job opportunities for women, the gender difference is amplified when the unemployment rate is used to proxy job prospects. Actually, among participants with UB, women gain more from training than men when we control for differences in job opportunities by means of the outflow indicator, see column six in Tables 4 and 5.

9.2 Job Opportunities Between and Within Counties

The JOI based on the adjusted outflow from unemployment is also calculated at the county level, see Appendix, Table A9 for details. Correspondingly, the estimated training effects are split into 19 county-specific effects, by gender, unemployment benefit entitlement and training cohort, using participants and non-participants from the same county.[21] We regress the group specific training effect in each county on the corresponding county level JOI, separately by outcome years. Table 6 reports estimates with and without county dummies. The inclusion of county fixed effects can be motivated by differences in training programmes and labour market characteristics across regions as well by time-invariant measurement error in the county-specific JOI.

[21] Note, however, that the matching is not performed at the county level.

The differences between men and women, winter and autumn courses as well as between those entitled to unemployment benefit and those without are very similar to those reported in Table 4 and 5. The significantly positive effect of the JOI shows that the training effects are higher in local labour markets with more favourable employment opportunities. Consequently, using variation in job opportunities across and within regions confirm our findings based on macro-level indicators.

The impact of job opportunities on training effects is economically important. The difference between the 10^{th} and the 90^{th} percentile in the county-specific JOI distribution is about 0.05. Thus, the corresponding predicted training effect differential is found by dividing the coefficient in the first row of Table 6 by twenty.

Table 6. The Impact of Job Opportunities on First, Second and Third Year Training Effects. County-level effects. Estimated parameters (standard errors).

	First year effect		Second year effect		Third year effect	
Unemployment outflow JOI	213,440 (47,566)	303,270 (44,841)	190,722 (63,709)	216,203 (65,489)	239,013 (81,189)	269,572 (93,147)
Winter	16,613 (1,215)	15,254 (1,070)	7,070 (1,432)	6,260 (1,354)	4,796 (1577)	4,368 (1,544)
No UB	2,267 (1,867)	210 (1,642)	-48 (2,278)	-1,377 (2,148)	-48 (2,518)	-947 (2,459)
Female	2,045 (1,634)	3,183 (1,456)	4,998 (2,104)	5,462 (2,045)	10,028 (2,590)	10,516 (2,748)
Female*no UB	-6,051 (2,262)	-5,018 (1,982)	-10,943 (2,793)	-9,983 (2,627)	-11,548 (3,125)	-10,706 (3,044)
Constant	-6,423 (1,216)	5,050 (1,780)	5,432 (1,546)	15,844 (2,402)	5,594 (1,967)	12,721 (3,036)
Fixed effects	None	County Dummies	None	County Dummies	None	County Dummies
R^2	0.2222	0.4176	0.1147	0.2399	0.0934	0.1714
# observations	874	874	722	722	589	589

Note: Dependent variable is the county level training effect = average earnings differential between participants and non-participants; by gender, UB entitlement and season. OLS weighted by the inverse of the standard error of the estimated training effect. Reference group is men, with UB on Autumn courses in the Oslo region.

Across outcome years, we find that the effect differential is about 10,000 to 15,000 NOK when comparing the 10th and the 90th per centile in the county-specific JOI distribution.

Whether we use macro or regional variation in job opportunities, we find a similar association between the state of the labour market and the impact of labour market training. We consider this as strong evidence for the case that training effects do depend on how outcome years are located in the business cycle.[22]

10. Conclusions

By comparing mean outcomes for matched samples of participants and non-participants we evaluate the Norwegian labour market training programme (LMT) targeted at unemployed adults. We estimate the average earnings effects of training on the trained, using individuals participating in LMT drawn from all entrants (and re-entrants) in the Norwegian public unemployment register during December 1990 – July 1996. As we evaluate average effects only, we construct fairly homogenous groups of participants with separate analyses for those starting training at about the same time, i.e. in winter or in autumn each year. This gives us 12 cohorts of participants over the six year period. The samples are also separated by gender and unemployment benefit entitlement. This gives a total of 48 subsamples of participants and their partners of unemployed non-participants.

Matched samples of unemployed non-participants are used to simulate the counterfactual outcomes of the participants. The matching procedure selects unemployed non-participants with the closest set of predicted probabilities from a multinomial choice model where training, participation in other programmes, exit from the unemployment register or remaining unemployed are alternative outcomes.

Unlike most evaluation studies which typically study one, or a limited number of cohorts of participants, we are able to disentangle the impact of business cycles on training effects from the importance of the time span between the training and the post-training period. Since a single cohort study will have perfect correlation between "time since programme" and "calendar period", any impact of post-programme labour market conditions can only be identified by means of spatial (or regional) variation. Using several cohorts of participants, we are able to estimate first, second and third year effects under different labour market conditions, even controlling for fixed regional effects.

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[22] We have also included the average outflow JOI during the last 12 months before training among the controls in Table 4,5 and 6, and the estimates of the other variables, like the JOI during outcome years, are basically

Two main conclusions can be drawn from this study. First, the training programme has, on average, a positive impact on post-training annual earnings for those who participate. Second, the training effect is larger when job opportunities in the post-programme period are favourable. When job opportunities are bleak, participants gain less in terms of post-training earnings. This insight follows from the *meta analysis* of the large number of group- and cohort-specific training effects where two indicators of post-training job opportunities are used. The first indicator is the national average of annual unemployment rates by gender from the Labour Force Surveys. The second indicator is based on all Norwegian unemployment spells in the 1990's and is a measure of annual average of outflow from unemployment, based on human capital adjusted monthly exit rates. We argue that the outflow indicator is a more precise measure of post-training labour market opportunities. Training effects are positively correlated with job opportunities measured by both indicators.

As in most non-experimental studies, the estimated training effects can be driven by selction on unobservables rather than a causal impact on post-training outcomes. In our case, the institutional setting does not provide any clearcut indication. As for most programmes targeted at unemployed, the recruitment to LMT is a mixture of self-selection and administrative decisions. Previous studies of selections process suggest that there is, if any, a positive selection to LMT, i.e. participants have observed – and possibly also unobserved - characteristics assumed to correlate positively with employability. In this study pre-training earnings records are available. When looking at whether individual earnings are correlated with *future* LMT participation, the null hypothesis of no correlation is not rejected. Since pre-training earnings are not significantly different for participants and non- participants, we gain more confidence in the consistency of our estimates.

Information on how labour market conditions affect estimated programme effects are useful when assessing and explaining differences in effects across time, regions and even countries. For example, in the case of Sweden, programme effects hvae changed systematically over the business cycle. The programe effects are more negative (or less positive) when evaluations are based on post-programme outcomes during the area of high unemployment in the early 1990s, compared to studies using data for the 1980s or towards the end of the 1990s. Our results are also useful for policy making as the optimal timing and volume of ALMP must take into account that individual effects are likely to vary over the business cycle.

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unchanged. No systematic pattern is found for the impact of the pre-training JOI.

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Appendix

1. Definition of variables

Dependent variable

(a) Annual earnings defined by tax registers, including wages, self-employment income and sickleave benefits measured in 1997 NOK. (Unemployment benefits and other transfers are not included).

Explanatory variables

(a) Married (dummy)

(b) Level of education: Educ1 - Educ6

6 dummies: le 9 years, 10 ys, 11-12 ys (reference), 13-16 ys, ge 17 ys, and unknown

(c) County of residence: 19 dummies

one for each county in Norway, county of Oslo as reference

(d) Age: Age1 – Age5

5 dummies: 25-30 ys, 31-35 ys, 36-40 ys (reference), 41-45 ys, 46-50 ys

(e) Immigrant from outside OECD: Immigrant (dummy)

(f) Unemployment history: Month-1 – Months-1923,

i.e. number of months of unemployment before *t*: 16 dummies: 0 (reference), 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12,13-15, 16-18, 19-23 months

(g) Earnings history: Earnings1-Earnings2123,

i.e. number of years of annual earnings above B.a. before the year *T* (B.a. = Basic amount in the Norwegian Social Insurance Scheme, annually regulated, about Euro 5-6,000 in the period of interest). Earnings history serves as an indicator of aggregated experience

13 dummies: 0 (reference), 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11-15, 16-20, and 21-23 years of income above B.a. (23 is maximum since for the first sample (T= 1991) as the scheme was established in 1967, when the Norwegian Social Insurance Scheme was established)

(h) LMT history: LMT1 – LMT8,

i.e. number of quarters participated in LMT during last 23 months before *t:* 8 dummies, one for each of the latest 8 quarters LMT8 means last quarter, LMT7 means the second last quarter .... LMT1 means two years ago

(i) Programme history: PROG1 – PROG8,

i.e. number of quarters participated in other programmes than LMT during last 23 months before *t:* 8 dummies, on for each of the latest 8 quarters PROG8 means last quarter, PROG7 means the second last quarter .... PROG1 means two years ago

(j) Occupational background: Occup1-Occup6

categories (based on ISCO): (1) technical, physical science, humanistic and artistic work (teachers, nurses, doctors, technicians etc), (2) administrative executive work, clerical work and sales work,

(3) agriculture, forestry, fishing and related work, (4) manufacturing work, mining, quarrying, building and construction work (reference), (5) service work, transport and communication, (6) unknown

(k) Children in household: Kid1 – Kid3

4 dummies: 0 (reference), 1, 2 or 3 children and more, below the age of 18

(l) Previous annual earnings: AnnEarn1 – AnnEarn3

3 continuously distributed variables for last year before *t,* second last year, and next to second last year: annual earnings measured by B.a.

(m) Left ordinary education just before *t;* LeftEduc (dummy),
only available for cohorts x-12

(n) Left ordinary high level education just before *t;* LeftEducHigh
(dummy, two highest levels of education) , only available for cohorts x-12.

2. Matching results

Table A1. Means of predicted probabilitis for matched samples. Selected cohorts.

	Winter 1991		Autumn 1991		Winter 1995		Autumn 1995	
Subsample	Partic.	Non-part	Partic.	Non-part	Partic.	Non-part	Partic.	Non-part
Women without UB								
Pred prob of LMT	0.2177	0.2158	0.2905	0.2879	0.1366	0.1357	0.2643	0.2632
Pred prob of PROG	0.0480	0.0472	0.0627	0.0623	0.0472	0.0472	0.0684	0.0682
Pred prob of OUT	0.3172	0.3164	0.2914	0.2917	0.2266	0.2263	0.2351	0.2352
Women with UB								
Pred prob of LMT	0.1195	0.1182	0.1687	0.1677	0.0724	0.0720	0.1191	0.1186
Pred prob of PROG	0.0431	0.0427	0.0468	0.0465	0.0485	0.0484	0.0662	0.0661
Pred prob of OUT	0.2959	0.2960	0.3165	0.3160	0.2377	0.2373	0.3213	0.3211
Men without UB								
Pred prob of LMT	0.1493	0.1480	0.1911	0.1902	0.0865	0.0856	0.1735	0.1724
Pred prob of PROG	0.0525	0.0520	0.0594	0.0592	0.0469	0.0468	0.0649	0.0647
Pred prob of OUT	0.3067	0.3056	0.3047	0.3033	0.2331	0.2330	0.2562	0.2560
Men with UB								
Pred prob of LMT	0.0931	0.0928	0.1398	0.1391	0.0628	0.0621	0.1057	0.1053
Pred prob of PROG	0.0447	0.0444	0.0582	0.0578	0.0509	0.0509	0.0686	0.0685
Pred prob of OUT	0.2518	0.2515	0.2774	0.2769	0.2233	0.2223	0.2801	0.2804

Table A2. Descriptive statistics. Participants and matched non-participants. Selected cohorts.

(a) Participants and matched non-participants with unemployment benefits

	Men Winter 1991		Men Winter 1995		Women Winter 1991		Women Winter 1995	
	Particip.	Non-part.	Particip.	Non-part.	Particip.	Non-part.	Particip.	Non-part.
Age (years)	33.848	33.823	34.352	34.055	35.441	35.570	34.703	34.913
Married	0.485	0.498	0.545	0.574	0.215	0.206	0.338	0.341
Number of children	0.936	0.879	0.845	0.833	1.158	1.177	1.185	1.204
Low education (9 years or less)	0.086	0.076	0.146	0.131	0.054	0.072	0.143	0.146
Low medium educ (10 years)	0.400	0.423	0.355	0.335	0.469	0.464	0.391	0.393
Med educ (11 to 12 years)	0.345	0.342	0.345	0.357	0.347	0.341	0.323	0.322
High educ 1 (13 to 16 years)	0.139	0.124	0.102	0.111	0.108	0.111	0.103	0.105
High educ 2 (17 years +)	0.020	0.020	0.016	0.019	0.018	0.011	0.010	0.007
Ostfold	0.058	0.047	0.053	0.071	0.076	0.068	0.079	0.082
Akershus	0.070	0.069	0.091	0.092	0.089	0.076	0.103	0.133
Oslo	0.090	0.076	0.133	0.115	0.076	0.068	0.153	0.146
Hedmark	0.035	0.046	0.029	0.034	0.035	0.024	0.036	0.027
Oppland	0.048	0.032	0.037	0.034	0.047	0.061	0.052	0.043
Buskerud	0.045	0.052	0.080	0.073	0.038	0.032	0.066	0.061
Vestfold	0.049	0.045	0.044	0.043	0.068	0.058	0.045	0.043
Telemark	0.026	0.028	0.024	0.024	0.025	0.017	0.020	0.032
A_Agder	0.027	0.029	0.026	0.027	0.016	0.021	0.022	0.029
V_Agder	0.056	0.050	0.037	0.035	0.042	0.054	0.032	0.025
Rogaland	0.096	0.104	0.078	0.072	0.128	0.139	0.049	0.059
Hordaland	0.139	0.157	0.084	0.092	0.142	0.122	0.097	0.094
Sogn	0.015	0.012	0.008	0.008	0.006	0.012	0.007	0.006
Moere	0.063	0.067	0.049	0.044	0.056	0.055	0.052	0.035
S_Trond	0.038	0.034	0.051	0.051	0.054	0.064	0.040	0.051
N_Trond	0.022	0.022	0.050	0.055	0.021	0.028	0.051	0.056
Nordland	0.071	0.071	0.060	0.072	0.050	0.056	0.055	0.051
Troms	0.025	0.028	0.035	0.033	0.019	0.023	0.025	0.022
Finnmark	0.027	0.032	0.031	0.028	0.013	0.022	0.017	0.007
Work experience (years)	12.058	11.969	11.816	11.697	9.162	8.964	9.463	9.343
Aggregated pension points	2.861	2.843	2.618	2.570	1.802	1.764	1.806	1.844
Prevevios open unempl (months)	7.882	7.934	9.476	9.591	7.586	7.701	8.049	8.156
Indic. Of last year's income	3.946	3.879	3.671	3.573	2.820	2.784	2.896	2.896
Indic. Of second last year's income	4.367	4.367	3.645	3.522	2.995	2.974	2.744	2.682
Indic of third last year's income	4.350	4.346	3.512	3.419	2.775	2.700	2.546	2.546
Occup 1	0.165	0.135	0.114	0.114	0.186	0.165	0.245	0.261
Occup 2	0.113	0.109	0.135	0.146	0.493	0.501	0.421	0.433
Occup 3	0.026	0.024	0.029	0.021	0.017	0.017	0.013	0.013
Occup 5	0.517	0.532	0.484	0.483	0.085	0.093	0.077	0.079
Occup 6	0.169	0.182	0.205	0.204	0.202	0.209	0.228	0.204
LMT1	0.033	0.027	0.094	0.094	0.021	0.017	0.068	0.077
LMT2	0.036	0.031	0.089	0.080	0.030	0.024	0.061	0.068
LMT3	0.047	0.040	0.078	0.073	0.050	0.052	0.066	0.079
LMT4	0.057	0.047	0.077	0.074	0.063	0.073	0.074	0.075
LMT5	0.087	0.079	0.143	0.131	0.094	0.106	0.137	0.120
LMT6	0.078	0.073	0.152	0.133	0.113	0.114	0.144	0.139
LMT7	0.070	0.062	0.145	0.140	0.111	0.099	0.165	0.150
LMT8	0.063	0.058	0.138	0.146	0.117	0.115	0.196	0.193
PROG1	0.051	0.045	0.132	0.130	0.060	0.060	0.104	0.114
PROG2	0.091	0.086	0.147	0.156	0.054	0.059	0.113	0.123
PROG3	0.088	0.092	0.153	0.179	0.054	0.058	0.113	0.156
PROG4	0.088	0.084	0.165	0.180	0.060	0.056	0.118	0.149
PROG5	0.073	0.068	0.109	0.121	0.075	0.080	0.095	0.134
PROG6	0.088	0.091	0.113	0.131	0.069	0.080	0.097	0.115
PROG7	0.062	0.059	0.075	0.062	0.031	0.039	0.075	0.088
PROG8	0.028	0.034	0.034	0.017	0.016	0.016	0.049	0.064
Just left education			0.144	0.144			0.180	0.180

(b) Participants and matched non-participants without unemployment benefits

	Men Winter 1991		Men Winter 1995		Women Winter 1991		Women Winter 1995	
	Particip.	Non-part.	Particip.	Non-part.	Particip.	Non-part.	Particip.	Non-part.
Age (years)	32.748	32.786	32.989	32.765	34.764	33.978	34.022	34.715
Married	0.502	0.521	0.552	0.557	0.165	0.162	0.267	0.253
Number of children	0.907	0.984	0.637	0.618	1.280	1.355	1.459	1.443
Low education (9 years or less)	0.076	0.066	0.110	0.110	0.039	0.035	0.137	0.125
Low medium educ (10 years)	0.400	0.430	0.286	0.248	0.487	0.493	0.307	0.330
Med educ (11 to 12 years)	0.295	0.265	0.263	0.256	0.313	0.313	0.285	0.262
High educ 1 (13 to 16 years)	0.175	0.169	0.139	0.166	0.126	0.125	0.101	0.106
High educ 2 (17 years +)	0.033	0.054	0.014	0.016	0.020	0.022	0.022	0.016
Ostfold	0.071	0.072	0.058	0.060	0.089	0.074	0.069	0.067
Akershus	0.055	0.055	0.104	0.109	0.071	0.083	0.147	0.151
Oslo	0.150	0.126	0.222	0.243	0.072	0.066	0.169	0.212
Hedmark	0.027	0.014	0.028	0.035	0.030	0.025	0.036	0.034
Oppland	0.028	0.032	0.021	0.024	0.027	0.030	0.029	0.033
Buskerud	0.032	0.039	0.076	0.074	0.037	0.025	0.032	0.033
Vestfold	0.060	0.090	0.062	0.047	0.057	0.044	0.066	0.066
Telemark	0.027	0.028	0.032	0.032	0.025	0.022	0.032	0.026
A_Agder	0.025	0.022	0.017	0.024	0.017	0.014	0.011	0.012
V_Agder	0.049	0.058	0.035	0.017	0.046	0.044	0.037	0.041
Rogaland	0.107	0.087	0.057	0.060	0.160	0.194	0.073	0.055
Hordaland	0.150	0.142	0.087	0.088	0.128	0.128	0.107	0.092
Sogn	0.013	0.016	0.003	0.002	0.005	0.007	0.008	0.008
Moere	0.035	0.033	0.038	0.038	0.047	0.054	0.045	0.052
S_Trond	0.060	0.068	0.035	0.027	0.077	0.091	0.044	0.040
N_Trond	0.014	0.019	0.024	0.022	0.030	0.024	0.037	0.029
Nordland	0.046	0.044	0.041	0.049	0.049	0.051	0.038	0.027
Troms	0.028	0.033	0.046	0.035	0.010	0.008	0.014	0.010
Finnmark	0.025	0.022	0.016	0.016	0.022	0.019	0.010	0.012
Work experience (years)	7.836	8.066	6.120	5.533	5.931	5.823	4.481	4.895
Aggregated pension points	1.721	1.832	1.324	1.187	1.143	1.141	0.971	1.007
Prevevios open unempl (months)	7.148	7.594	7.462	7.323	5.596	5.572	5.223	5.295
Indic. Of last year's income	1.791	1.907	1.063	1.038	0.847	0.909	0.507	0.528
Indic. Of second last year's income	2.080	2.188	1.114	1.067	0.982	1.066	0.516	0.565
Indic of third last year's income	2.277	2.357	1.120	1.103	1.112	1.203	0.570	0.601
Occup 1	0.140	0.173	0.117	0.126	0.182	0.162	0.214	0.227
Occup 2	0.087	0.090	0.117	0.112	0.347	0.355	0.292	0.312
Occup 3	0.044	0.041	0.036	0.027	0.012	0.019	0.003	0.006
Occup 5	0.419	0.397	0.314	0.314	0.074	0.067	0.081	0.062
Occup 6	0.113	0.107	0.164	0.144	0.172	0.168	0.164	0.169
LMT1	0.038	0.032	0.115	0.099	0.030	0.030	0.099	0.097
LMT2	0.039	0.035	0.112	0.095	0.046	0.049	0.092	0.095
LMT3	0.061	0.057	0.122	0.104	0.072	0.084	0.101	0.106
LMT4	0.088	0.071	0.120	0.106	0.089	0.106	0.110	0.114
LMT5	0.120	0.106	0.174	0.147	0.126	0.133	0.132	0.115
LMT6	0.096	0.088	0.156	0.137	0.111	0.123	0.126	0.129
LMT7	0.069	0.065	0.131	0.110	0.123	0.108	0.190	0.199
LMT8	0.085	0.080	0.112	0.103	0.148	0.131	0.216	0.222
PROG1	0.052	0.033	0.055	0.062	0.052	0.049	0.032	0.029
PROG2	0.077	0.066	0.062	0.066	0.049	0.052	0.029	0.030
PROG3	0.074	0.079	0.080	0.073	0.054	0.052	0.034	0.032
PROG4	0.101	0.102	0.088	0.077	0.056	0.052	0.055	0.055
PROG5	0.082	0.071	0.080	0.069	0.057	0.057	0.059	0.069
PROG6	0.093	0.104	0.112	0.098	0.069	0.064	0.077	0.077
PROG7	0.074	0.066	0.085	0.077	0.037	0.030	0.051	0.051
PROG8	0.044	0.028	0.047	0.039	0.025	0.030	0.045	0.040
Just left education			0.188	0.181			0.226	0.240

3. Detailed training effects. By group and outcome year.

Table A3. First year effects. By season, gender, unemployment benefit entitlement and training year.

Training year	1991	1992	1993	1994	1995	1996
Winter courses						
Male, UB	11414	8509	7014	13979	8139	22649
	[3784.07]	[3210.01]	[3084.97]	[3332.31]	[5491.92]	[5044.47]
Male, no UB	9110	38	568	7994	13231	24862
	[4825.44]	[4553.98]	[4388.8]	[4282.51]	[5361.35]	[5389.45]
Female, UB	9614	14603	12445	15513	6648	11589
	[3145.15]	[2859.55]	[3100.11]	[3342.19]	[3951.31]	[4068.22]
Female, no UB	3111	5708	10570	6231	7188	12968
	[3620.43]	[3213.62]	[3416.26]	[3381.53]	[3764.92]	[3526.58]
Autumn courses						
Male, UB	-7875	-6185	-4810	-7000	5853	4436
	[2302.32]	[2051.02]	[2346.41]	[2740.42]	[3099.72]	[3742.11]
Male, no UB	-7040	-3128	-2679	3227	4766	4063
	[3192.91]	[3028.56]	[3194.79]	[2935.77]	[3252.95]	[3931.44]
Female, UB	-12735	-6831	-6349	-3801	-9296	3943
	[1748.03]	[1645.94]	[1957.65]	[1917.35]	[2092.67]	[2338.86]
Female, no UB	-14941	-10501	-8320	-11028	-9883	-3593
	[1861.4]	[1796.42]	[1964.72]	[1731.21]	[1781.69]	[2015.6]

Table A4. Second year effects. By season, gender, unempl. benefit entitl. and ***training year.***

Training year	1991	1992	1993	1994	1995	1996
Winter courses						
Male, UB	13606	11761	13968	17113	13457	na
	[4114.45]	[3515.27]	[3226.13]	[3639.87]	[5217.43]	
Male, no UB	14837	-2417	10133	17321	20710	na
	[5316.2]	[4934.32]	[4857.87]	[4925.34]	[6076.64]	
Female, UB	14250	18970	16355	22009	13244	na
	[3412.31]	[3216.56]	[3455.25]	[3767.5]	[4439.52]	
Female, no UB	-420	3130	18332	16038	11249	na
	[3979.53]	[3619.47]	[3749.19]	[4034.93]	[4321.21]	
Autumn courses						
Male, UB	3380	8845	8499	5296	16739	na
	[2765.92]	[2542.49]	[2790.15]	[3277.77]	[3826.45]	
Male, no UB	436	5365	8592	16799	17686	na
	[3886.01]	[3770.06]	[3911.96]	[3800.26]	[4088.12]	
Female, UB	3251	11269	10985	12378	5553	na
	[2132.76]	[2088.17]	[2349.76]	[2363.02]	[2608.43]	
Female, no UB	-5517	-993	2712	-2412	-1402	na
	[2382.13]	[2212.91]	[2399.72]	[2232.31]	[2325.83]	

Table A5. Third year effects. By season, gender, unemployment benefit entitlement and training year.

Training year	1991	1992	1993	1994	1995	1996
Winter courses						
Male, UB	8746	14661	11608	18469	na	na
	[4219.21]	[3568.96]	[3482.13]	[3910.1]		
Male, no UB	15325	-4253	10711	21011	na	na
	[5598.28]	[5236.74]	[5711.5]	[5340.2]		
Female, UB	18099	21003	18466	23525	na	na
	[3569.69]	[3384.2]	[3694.2]	[4323.92]		
Female, no UB	6535	2833	15841	16308	na	na
	[4275.83]	[3778.01]	[3966.32]	[4422.68]		
Autumn courses						
Male, UB	7145	9913	10707	8574	na	na
	[2970.39]	[2703.24]	[3095.34]	[3664.46]		
Male, no UB	5112	6516	14058	19403	na	na
	[4168.54]	[3992.43]	[4404.87]	[4369.46]		
Female, UB	10569	13460	18419	14507	na	na
	[2288.66]	[2234.07]	[2581.22]	[2609.56]		
Female, no UB	-1217	3921	6996	1159	na	na
	[2617.54]	[2402.72]	[2652.1]	[2559.16]		

Table A6. Fourth year effects. By season, gender, unemployment benefit entitlement and training year.

Training year	1991	1992	1993	1994	1995	1996
Winter courses						
Male, UB	15206	16376	7853	na	na	na
	[4258.43]	[3777.54]	[3794.94]			
Male, no UB	17276	7030	12517	na	na	na
	[5735]	[5660.09]	[5773.17]			
Female, UB	17252	18617	18981	na	na	na
	[3625.11]	[3573.27]	[3923.41]			
Female, no UB	9258	-84	18848	na	na	na
	[4350.85]	[4101.17]	[4284.37]			
Autumn courses						
Male, UB	8156	9183	19332	na	na	na
	[3077.35]	[2948.19]	[3337.35]			
Male, no UB	6006	11313	9122	na	na	na
	[4422.26]	[4552.15]	[4852.38]			
Female, UB	8839	13409	21641	na	na	na
	[2421.07]	[2401.03]	[2780.09]			
Female, no UB	1261	8360	7923	na	na	na
	[2628.39]	[2564.03]	[3001.56]			

4. Pre-training tests

Table A7. Earnings differentials between participants and non-participants in the pre-training years. By pre-training year, season, gender, unemployment benefit entitlement and and training years.

Earnings one year before training (s=1)

Season	Winter				Autumn			
Training year	1993	1994	1995	1996	1993	1994	1995	1996
Male, UB	493	1827	5732	5885	-180	694	4675	7358
	[2587.64]	[3456.96]	[4691.82]	[5282.02]	[2771.96]	[3290.66]	[3721.59]	[4315.91]
Male, no UB	-744	-2786	-905	4028	-445	734	-378	-5395
	[3993.16]	[4077.56]	[3836.11]	[4094.48]	[2873.85]	[2869.86]	[2568.46]	[3185.19]
Female, UB	3472	-4463	5854	-167	503	2777	-1144	368
	[2782.59]	[3663.14]	[3860.23]	[4024.09]	[2177.88]	[2298.91]	[2527.83]	[2824.17]
Female, no UB	-3966	986	-1738	1387	-2480	-1878	-1271	1857
	[2450.31]	[2351.49]	[2481.96]	[2272.99]	[1755.63]	[1396.73]	[1359.33]	[1449.55]

Earnings two years before training (s=2)

Season	Winter			Autumn		
Training year	1994	1995	1996	1994	1995	1996
Male, UB	1706	2715	6615	-1913	3201	3734
	[2631.18]	[4364.53]	[5016.06]	[2934.93]	[3671.25]	[4349.99]
Male, no UB	-4083	218	3870	-3289	-1545	-5683
	[3930.46]	[3875.63]	[3851.32]	[2379.97]	[2494.84]	[3283.28]
Female, UB	4450	5183	6482	-85	3190	2337
	[2873.13]	[3790.57]	[3952.12]	[2085]	[2474.36]	[2758.5]
Female, no UB	990	-482	626	-2510	-1575	-697
	[2327.5]	[2353.48]	[2181.42]	[1271.63]	[1332.25]	[1407.47]

Earnings three and four years before training (s=3,4)

	s=3				*s=4*	
Season	Winter		Autumn		Winter	Autumn
Training year	1995	1996	1995	1996	1996	1995
Male, UB	-1727	885	-336	2569	3519	152
	[3761.84]	[4838.95]	[3275.88]	[3972.94]	[3573.9]	[3612.27]
Male, no UB	978	3369	1570	-3798	4155	-5802
	[4242.59]	[4627.81]	[2333.71]	[3203.64]	[4095.59]	[3059.75]
Female, UB	-3041	2167	-2240	4670	524	1904
	[3090.73]	[3737.01]	[2250.82]	[2600.38]	[2834.53]	[2376.02]
Female, no UB	-3516	1122	-383	-1231	-3073	-392
	[2669.96]	[2142.33]	[1203.68]	[1403.5]	[2189.47]	[1312.85]

5. Job Opportunity Indicators

While the LFS unemployment rate is the official unemployment rate produced by Statistics Norway, the alternative indicator is estimated from individual unemployment spells in the Frisch Centre Data base covering *all* registered spells throughout the 1990's. Note that the our alternative JOI is constructed by means of all spells and not only those covered by the study of training effects.

Outflow from unemployment

The computation of our job opportunity indicator is based on a hazard rate model with the exit probability from unemployment as the dependent variable. This approach has been motivated by Røed (2001) and Røed and Zhang (2002). It resorts on the idea of a proportional hazard model in that the hazard rate is proportional in factors depending on calendar time, spell duration and (time varying) explanatory variables. Let d denote actual duration of unemployment spell, t denote calendar time, λ_d be parameters of duration d, and σ_t be parameters associated with calendar time. Note here we make no distributional assumptions on both duration and calendar time. Therefore λ_d and σ_t are estimated non-parametrically. The monthly probabilities of individual exits from unemployment are then parameterised as follows

$$h(t,d,x_t) = 1 - \exp(-\exp(\sigma_t + x_t^{'}\beta + \lambda_d))$$

where $\exp(\sigma_t + x_t^{'}\beta + \lambda_d)$ is interpreted as the integral taken over an underlying continues time hazard rate for the time interval corresponding to spell duration month number d (Prentice and Gloeckler (1978), Meyer (1990)), hence the parameters can be interpreted in terms of the underlying hazard rate. The vector of explanatory variables, x_t , includes a total number of 43 covariates capturing age, gender, educational attainment, immigrant status, and dummy for part time employment. Taking the complete administrative unemployment register from 1990 to 1999, we estimate this model on prime age individuals from age 25 to 59, which are entitled to unemployment benefit. The total number of monthly observations used for the estimation of equation is 10,182,300. Table A8 provides summary statistics of the estimation sample.

Job Opportunity Indicators in Table 4, 5 and 6

To calculate the job opportunity indicator used in section 9, we estimate predicted monthly exit probabilities for representative mean individuals for the period of 1992 to 1997, separately for men and women. The representative individual is constructed by taking mean value of all explanatory variables $\overline{x}_t$ for both men and women. The estimated monthly transition probabilities are then

$$\hat{h}(t,\overline{d},\overline{x}_t) = 1 - \exp(-\exp(\hat{\sigma}_t + \overline{x}_t^{'}\hat{\beta} + \hat{\lambda}_{\overline{d}}))$$

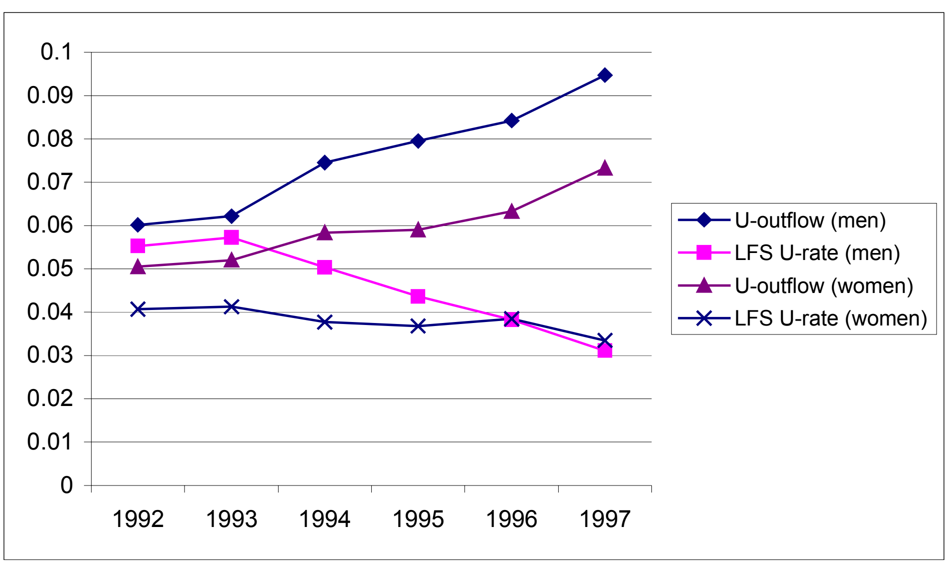
We then take the yearly average of these as proxies for the aggregated job opportunity indicators for each of analysing year. Røed and Zhang (2002) provides detailed discussion on properties of this indicator and extension to the case of mixed proportional hazard model.

Table A8. Summary statistics of estimation sample

	Men	*Women*
Number of monthly observations	4 741 250	5 441 050
Mean transition	0.082	0.066
Mean duration	10.82	11.52
Mean age	36.78	37.35
Education attainment		
Primary school (<= 9 years)	0.17	0.18
1 year Secondary school (10 year)	0.30	0.37
Vocational school (11-12 years)	0.23	0.13
Secondary school (12 years) ref	0.11	0.19
3 years college engineer education (13-15 years)	0.02	0.00
Lower college/university (13-15 years)	0.06	0.06
Higher university (> 15 years)	0.07	0.05
Unknown	0.06	0.04
Part-time employment	0.22	0.42
Married	0.33	0.55
Non-OECD immigrant	0.12	0.07

Figure A1 compares the (estimated) national monthly unemployment outflow rate and the standard unemployment rate from the Labour Force Survey (LFS), by gender for the period 1992 to 1997. Røed (2001) has showed that unemployment rate (as well as the aggregate outflow rate from unemployment) lags behind the estimated job opportunity indicator, which reflects a systematic sorting effect on unemployed over the business cycle. The figure also depicts that the two JOI's tell different stories about the gender differential in job opportunities. Although men have experienced higher unemployment rates than women, the unemployed men are more likely to leave unemployment and enter jobs, than women. We find the job opportunity indicator based on estimated transition probabilities to be a better measure of the how the job opportunities of unemployed job seekers vary over the business cycle.

Figure A1. Unemployment outflow and the LFS unemployment rate.



Job Opportunity Indicator in Table 6

The model where the business cycle effect is identified by within county variations in outflow rates uses the county level variable for men and women as displayed in Table A9.

Table A9. County level (monthly) outflow rates. By gender for 1992-1997.

Men	*1992*	*1993*	*1994*	*1995*	*1996*	*1997*
Østfold	0.052	0.058	0.075	0.084	0.082	0.098
Akershus	0.053	0.055	0.077	0.084	0.089	0.103
Oslo	0.046	0.050	0.064	0.072	0.075	0.083
Hedmark	0.052	0.058	0.079	0.085	0.088	0.094
Oppland	0.058	0.062	0.070	0.082	0.089	0.098
Buskerud	0.053	0.057	0.075	0.087	0.092	0.105
Vestfold	0.060	0.061	0.079	0.089	0.089	0.109
Telemark	0.054	0.061	0.074	0.081	0.084	0.093
Aust-Agder	0.072	0.073	0.086	0.089	0.098	0.114
Vest-Agder	0.073	0.066	0.085	0.086	0.100	0.101
Rogaland	0.082	0.076	0.075	0.076	0.088	0.100
Hordaland	0.052	0.066	0.073	0.076	0.083	0.091
Sogn og Fjordane	0.086	0.088	0.091	0.099	0.107	0.115
Møre og Romsdal	0.080	0.073	0.090	0.100	0.100	0.119
Sør-Trøndlag	0.058	0.056	0.069	0.079	0.084	0.093
Nord-Trøndlag	0.068	0.062	0.070	0.072	0.080	0.083
Nordland	0.067	0.064	0.071	0.076	0.079	0.094
Troms	0.069	0.069	0.078	0.078	0.086	0.099
Finnmark	0.084	0.085	0.077	0.071	0.064	0.073
Mean	*0.064*	*0.065*	*0.077*	*0.082*	*0.087*	*0.098*
St.dev	*0.013*	*0.010*	*0.007*	*0.008*	*0.010*	*0.011*
Women	***1992***	***1993***	***1994***	***1995***	***1996***	***1997***
Østfold	0.040	0.045	0.053	0.056	0.059	0.067
Akershus	0.049	0.052	0.060	0.066	0.070	0.083
Oslo	0.046	0.049	0.057	0.063	0.067	0.074
Hedmark	0.045	0.050	0.053	0.054	0.058	0.068
Oppland	0.050	0.053	0.053	0.057	0.062	0.070
Buskerud	0.049	0.049	0.055	0.062	0.068	0.081
Vestfold	0.047	0.046	0.062	0.058	0.061	0.077
Telemark	0.047	0.048	0.056	0.054	0.059	0.073
Aust-Agder	0.055	0.058	0.058	0.060	0.068	0.083
Vest-Agder	0.055	0.052	0.062	0.060	0.068	0.078
Rogaland	0.058	0.058	0.063	0.059	0.063	0.075
Hordaland	0.044	0.051	0.055	0.058	0.062	0.071
Sogn og Fjordane	0.070	0.066	0.077	0.070	0.080	0.086
Møre og Romsdal	0.051	0.052	0.062	0.062	0.071	0.074
Sør-Trøndlag	0.047	0.047	0.057	0.059	0.060	0.069
Nord-Trøndlag	0.047	0.049	0.053	0.051	0.053	0.059
Nordland	0.054	0.052	0.055	0.053	0.056	0.067
Troms	0.056	0.055	0.062	0.057	0.062	0.073
Finnmark	0.083	0.087	0.076	0.068	0.063	0.068
Mean	*0.052*	*0.054*	*0.059*	*0.059*	*0.064*	*0.073*
Std.dev.	*0.010*	*0.009*	*0.007*	*0.005*	*0.006*	*0.007*