Costs and benefits of Danish active labour market programmes ☆

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Abstract

Since 1994, unemployed workers in the Danish labour market have participated in active labour market programmes on a large scale. This paper contributes with an assessment of costs and benefits of these programmes. Long-term treatment effects are estimated on a very detailed administrative dataset by propensity score matching. For the years 1995 – 2005 it is found that private job training programmes have substantial positive employment and earnings effects, but also public job training ends up with positive earnings effects. Classroom training does not significantly improve employment or earnings prospects in the long run. When the cost side is taken into account, private and public job training still come out with surpluses, while classroom training leads to a deficit.
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1. Introduction

Since 1994 unemployment benefit collection throughout longer spells of unemployment has been conditional on participation in active labour market programmes (ALMPs) in Denmark. As a result, large-scale enrollment of unemployed into programmes has occurred, so that the Danish system of ALMPs is one of the most extensive in the OECD. Today, Denmark and Sweden are the countries in Europe that spend most money on active labour market policy as a share of GDP, and the policies have - at least for the Danish case - been implemented without much prior knowledge about potential beneficial effects, let alone whether such benefits exceed the costs of the programmes.

Active labour market policies constitute an important element of the functioning of labour markets not just in Denmark but in most European countries, while in the US they have limited scale. As pointed out by Kluve and Schmidt (2002) and Kluve (2006), in this light it is somewhat paradoxical that the practice of evaluating programmes is much less developed in Europe than in the US. In a recent meta-analysis of evaluations of European ALMPs Kluve (2006) finds that it is almost exclusively the programme type that matters for programme effectiveness, but that the studies very rarely are accompanied with rigorous cost-benefit analyses as the cost side mostly is neglected. The purpose of this paper is to help fill this gap with an assessment of costs and benefits of the large-scale system of ALMPs in Denmark.

We measure the net social benefit from the ALMPs by subtracting the programmes' costs from its discounted stream of benefits. As noted by Heckman et al. (1999), the primary social benefit reported in most cost-benefit analyses is the discounted earnings gain, which is usually of far larger magnitude than other measured benefits. Therefore it is important to obtain credible and precise estimates of the earnings gain. We calculate treatment effects on employment and earnings for a sample of unemployed who are followed over the years 1995 – 2005, and we show that in a labour market such as the Danish it is also very important to be in a position to estimate long-term effects, since the benefits may not appear until years after the first entrance into programmes. On the cost side we take into account direct costs of operating the programmes (administration costs, cost of education and training expenditures), corrected for marginal costs of public funds. Among relevant effects unaccounted for are the value of lost leisure, general equilibrium effects such as displacement of non-participants and potential ex ante effects on the transition rate out of unemployment.

We have access to a rich register-based non-experimental data set, and non-experimental evaluations have to address the issue of possible bias in the programme effects due to selection of participants into programmes. The method of matching (see e.g. Heckman et al., 1997) assumes that all relevant variables that affect both the selection process and labour market outcomes are known, such that conditional on these variables the programme effects are identified and unbiased. This is the conditional independence assumption (CIA). We argue that our data set contains so much information that most heterogeneity is observed, thus making the CIA plausible.

Surveys of the literature are given by Heckman et al. (1999), Martin and Grubb (2001) and Kluve and Schmidt (2002), but a few European evaluations of particular relevance for this paper should be mentioned.[3] Raaum et al. (2002) undertake a cost-benefit analysis of the specific programme 'labour

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[3] Among Danish evaluations there exists one post 1994-reform study of the entire system of Danish ALMPs (see Munch and Skipper, in press), but this study is mainly concerned with short-term effects, since they estimate treatment effects in a timing-of-events unemployment duration model. After accounting for selection into programmes based on observed and unobserved characteristics, they find that most programmes have negative net effects on the transition rate from unemployment to employment, which is often attributed to negative locking-in effects, but sometimes also negative post-programme effects. One exception is private employment programmes which tend to have a small positive net effect on the transition out of unemployment.

market training' in Norway. They have access to data for the period 1992–1997, and find that the effect of the programme on annual earnings mostly is positive and rising over time, and that for women with labour market experience the gains exceed the costs, while for men costs are close to benefits. For labour market entrants, however, the gains are lower than the costs.

The extent of the Swedish system of ALMPs comes closest to the Danish, and treatment effects of the Swedish ALMPs are estimated by Sianesi (2001). A sample of first-time unemployed individuals (in 1994) is followed over a 6–year period, and, except for job subsidies, adverse employment effects are found. For example for labour market training and work experience placement (almost 70% of the participants are enrolled in these programmes), there is initially negative (locking-in) effects, and it is not until after 4–5 years that they become (insignificantly) positive. The disappointing effects are partly attributed to the massive use of large-scale programmes which is claimed to have resulted in inefficient programme admin-istration and partly to the fact that participation is a way to renew eligibility for unemployment benefits. Larsson (2003) evaluates the effects of two Swedish youth programmes on earnings, employment probabilities and the transition to regular education for a two-year period in the first half of the 1990's. She finds negative short-term effects (one year after programme start) on earnings and employment, but these negative effects tend to become insignificant after two years.

Gerfin and Lechner (2002) evaluate the effects of the Swiss ALMPs over a 15 month period, and they find that employment and training programmes have adverse effects on employment outcomes, while temporary wage subsidies have positive employment effects. The importance of studying long-run effects is also suggested by Lechner et al. (2005) who estimate employment effects of West German training programmes over a 7–8 year period. Their conclusion is that the programmes have negative effects in the short run and positive effects over a horizon of about four years. Common to Raaum et al. (2002), Sianesi (2004), Larsson (2003), Gerfin and Lechner (2002), Lechner et al. (2005), and our study is that there is access to rich data sets, and the econometric approach used to estimate treatment effects is propensity score matching.

There is also a North American literature that contributes with (relatively) long-term experimental impact estimates for a small number of programmes. Couch (1992) presents long-term impacts for the National Supported Work Demonstration program, while General Accounting Office (1996) presents long-term impacts from the National JTPA Study, and common to both studies is that the impacts remain more or less constant over time. In addition, Hotz et al. (2006) estimate long-term effects of the California Greater Avenues to Independence (GAIN) programme and find that classroom training catches up and ultimately outperforms strategies that emphasize "work first". Finally, the long-term follow-up report on the National Job Corps Study by Schochet et al. (2003), and the long-term results presented by Social Research and Demonstration Corporation (2002) for the Canadian Self-Sufficiency project show that the impacts also may fade out over time. The existence of these quite divergent patterns of long-term impacts from the experimental evaluations highlights the dangers associated with simply extra-polating from short-term or medium-term impacts and, thereby, illustrates the value of estimating long-term effects.

The rest of the paper is organised as follows. In the next section the institutional framework of the Danish labour market is described. Section 3 outlines the evaluation problem and the method of matching. Section 4 describes the data set and the selection process into programmes, while Section 5 reports the estimated programme effects. Section 6 discusses the costs and benefits accounted for, and Section 7 compares costs and benefits of the programmes. Finally, Section 8 concludes.

2. The Danish labour market

2.1. Institutional framework

In Denmark labour market institutions play an important role in implementing labour market policies, and many labour market reforms are the outcome of tripartite agreements between unions, employer confederations and the government. This is also true for the labour market reforms of the 1990s, that introduced active labour market measures to the unemployed on a larger scale.

Those who are unemployed in Denmark receive relatively generous financial support, either in the form of unemployment insurance benefits or social assistance benefits. The receipt of unemployment insurance payments - unemployment benefits - is conditional on (voluntary) membership of an unemployment insurance (UI) fund. Today once a member has been unemployed for more than four years, the right to receive unemployment benefits is suspended until the member has been in employment for a period. Similarly, individuals who join a UI fund have to be employed for a certain time period before they earn the right to receive unemployment benefits. For low income workers the unemployment benefits replace up to 90% of the previous wage. If the individual chooses not to join a UI fund and becomes unemployed, he or she is eligible for social assistance, which basically consists of cash benefits. Social assistance benefits are available to any adult person who is unable to provide for him- or herself either through work, support from the spouse or through other social services.

The voluntary nature of Danish UI system implies that individuals may self-select into or out of the UI system. Several factors may influence the decision to join a UI fund - unemployed workers may for example be attracted to possible participation in ALMPs, but also access to favourable early retirement schemes and the individual's expected unemployment risk are found to play important roles, see Parsons et al. (2003).

In the early 1990s the Danish economy was in a recession, but conditions improved significantly since 1993, and the unemployment rate dropped from a high of 12.4% in 1993 to 4.5% in 2006. A considerable part of this reduction is due to the strong economic expansion throughout the last part of the 1990s. In the same period, a large number of people switched to voluntary schemes of withdrawal from the labour market comprising early retirement, transitional early withdrawal benefits and paid leave schemes, which also reduced unemployment. Any remaining part of the reduction in unemployment can presumably be ascribed to changes in the framework for the labour market, cf. Danish Economic Council (2002). More decentralised wage negotiations are likely to have been a factor behind the fall in unemployment, but changes in labour market policy presumably also have contributed to this improvement.

2.2. The 1994 labour market reform

In the 1990s a shift in labour market policies was introduced starting with the 1994 labour market reform. An important element of the reform was the introduction of active labour market measures to the unemployed on a larger scale. The main objective of these programmes was to improve the employment prospects of the unemployed. Another element in the reform was the abolition of the rule allowing the unemployed to renew their eligibility for benefit periods by participating in active labour market programmes. The maximum period for receiving benefits was reduced from nine to seven years for a particular spell of benefit receipt.

Subsequent changes have aimed at strengthening active labour market measures, on the principle that benefit entitlements should be conditional on participation in active labour market

programmes (the "right and duty" principle). The benefit period has gradually been shortened to four years, and the time until participation in ALMPs has been advanced correspondingly so that by January 2001 the unemployed were in principle obliged to participate after one year of unemployment, while initially the unemployed had four years of unconditional benefit collection. Once this period of unconditional benefits has expired the unemployed must participate in ALMPs during 75% of further time spent in unemployment.[4] Furthermore, availability and eligibility criteria have been tightened. A special youth programme was introduced in 1996, resulting in earlier ALMP participation and cuts in benefits.[5]

The proportion of the unemployed participating in programmes has increased substantially since the first reform in 1994. This is partly due to the strengthening of active measures, and partly due to the fact that the reforms also entailed a forward shift in the active period such that more people are affected by the requirements of the ALMPs. In 1995 around 38,000 yearly full-time UI fund members participated in some ALMP and this number declined to 32,000 in 2005. In the same period the number of yearly full-time social assistance recipients participating in active measures rose from around 26,000 in 1995 to 31,000 in 2005. When comparing these numbers to the corresponding numbers of unemployed (288,000 in 1995 and 157,000 in 2005) it becomes clear that the scale of the Danish system of ALMPs today is massive, and this has led Kluve and Schmidt (2002) to highlight Denmark as the prime example among European countries performing the transition from a benefit system of passive measures to one of active measures.

2.3. The four programme types considered

In this study we focus exclusively on members of UI funds, since social assistance recipients are often also disabled or have other social problems besides being unemployed. Bolvig et al. (2003) provide a description of the programmes offered in the social assistance system, and they estimate short-term employment effects of participation.

There are several different types of programmes offered to unemployed UI fund members, and in this study they are aggregated into four main types: private job training, public job training, classroom training and residual programmes. The definition of these programme types is largely dictated by the data. Private and public job training programmes cannot be disaggregated further, but in any case these programmes are fairly homogenous. Classroom training encompasses a more diverse mix of different programmes, but again data does not allow a more detailed classification. Residual programmes have been aggregated because most of the sub-programmes in this category have too few participants to allow for estimation of programme effects.

Private employers taking in an unemployed in a job training programme receive a wage subsidy, and the wage rate of participants in private job training equals the negotiated salary among the regularly employed. In contrast, the participants in public job training are employed in a public institution where a maximum hourly wage rate applies, and the monthly earnings equal the unemployment insurance payments. Working hours are adjusted to ensure that both the requirements with respect to hourly wage and monthly earnings are met. Participation in private and public job training is meant to result in an upgrade of the professional and technical skill base and facilitate a general rehabilitation to the labor market. The duration of private job training spells are on average shorter than those in the public sector, with average durations of 22 and

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[4] After the latest reform in 2002 the unemployed are instead required to participate in a programme every time they have had six consecutive months of unemployment.

[5] For more details and effects of this particular programme, see Jensen et al. (2003).

Table 1
Distribution of programmes, 1995–2005 [a]

	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005
Percent											
Private job training	13	9	9	9	5	7	7	6	8	11	9
Public job training	32	19	18	14	9	14	11	11	12	14	19
Classroom training	34	49	48	57	69	66	71	53	52	49	54
Employment progr.	6	10	13	12	8	7	5	5	4	0	0
Entrepreneurship progr.	8	7	7	2	1	0	0	0	0	0	0
Remedial educ. progr.	6	4	3	4	4	2	1	2	3	2	1
Job search assistance	0	0	0	0	0	1	1	19	17	20	14
Other programmes	1	0	0	2	2	3	3	4	5	4	3

[a] The group considered are unemployed members of UI funds in the age group between 18 and 50. Only the first programme for each person for each year is included.

39 weeks respectively. This is probably because the participants in private job training tend to have better employment prospects, based on their education, age and labour market experience, than participants in public job training.

Participants in classroom training receive a compensation equivalent to that of their UI benefits.[6] The average duration of classroom training is 28 weeks, and usually there is only access to programmes with a maximum duration of two years. Classroom training is a rather hetero-genous programme type, as a substantial number of different courses are available. Since there is a relatively long average duration of these three main programme types, it is important to consider long-run effects.

Residual programmes consist of i) Employment programmes, ii) Entrepreneurship subsidies, iii) Remedial education programmes, and iv) Job search assistance programmes. Employment programmes can either take place at a private or public employer, and they are typically targeted towards a weaker group of unemployed who are having difficulties in finding jobs under regular circumstances. The unemployed receives a compensation equal to the unemployment insurance benefit. Employment programmes in the public sector have a relatively long duration (up to three years) and entail, among other things, that the work being done has to be of a kind that would not otherwise be undertaken by the public sector. Entrepreneurship subsidies constitute a funding equivalent to 50% of regular UI benefits when recipients start up smaller business enterprises. This programme type was abandoned in 1998. Remedial education programmes are directed at weak unemployed who are not ready to enter into classroom training or employment programmes, but need some basic skills and some preparation for the labour market. Finally job search assistance programmes were introduced at a larger scale from 2002 and they provide career counseling, information on job vacancies and assist in matching workers to jobs. To sum up, the residual programme type is very heterogenous, but is primarily targeted towards the weaker unemployed.

There has been a shift in the composition of the types of programmes; see Table 1. The most frequently used programmes are classroom training, private job training and public job training. In 1995 34 percent of all participants were enrolled in classroom training, while this percentage had risen to 71 percent in 2001 after which it declined somewhat. At the same time, the proportion of those participating in private job training first declined from 13 percent after which it regained importance. The share of participants in public job training also fell considerably from an initial share of 32 percent.

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[6] Those below the age of 25 receive half of the maximum UI benefits.

3. The evaluation problem and matching

In this section, we will briefly discuss the non-experimental estimator applied and the identifying assumptions underlying this estimator. We begin with a brief outline of some notation, assumptions, and a formulation of the traditional impact estimator enriched to encompass our setting with more than two alternatives to choose from.[7] The objective of the evaluation is to measure the effect or impact of a treatment from the different and mutually exclusive set, $d \in \{0, 1, ..., D\}$, on outcome variables, $\{Y^0, Y^1, ..., Y^D\}$. Let $Y_i^{d'}$ be the person-specific outcome in the presence of treatment $d=d'$, and Y_i^0 the outcome in the absence of any treatment, $d=0$. Hence, the person-specific impact of the programme d' is defined as $\Delta_i = Y_i^{d'} - Y_i^0$. The fundamental evaluation problem is that we do not observe the same person with both outcomes at the same point in time. Therefore it becomes impossible to construct the person-specific impact for anyone by simply looking at the data. Instead, attention usually shifts to constructing means. The parameter we are interested in, is the average effect of treatment on the treated (ATET) defined as

$$\Delta = E[Y^{d'} - Y^0 | d = d'] = E[Y^{d'} | d = d'] - E[Y^0 | d = d'].$$ (1)

Hence, the problem is to find the counterfactual $E[Y^0 | d=d']$ in (1), which is unobserved but must be constructed in order for the defined impact measure to be identified, i.e. some assumptions are needed to obtain identification.

Matching is based on the assumption that all outcome-relevant differences between programme participants and non-participants are captured in their observed characteristics such that any difference in outcomes can be attributed to the programmes. The idea is to construct comparison groups among all the non-treated which are as similar as possible to the groups of participants in terms of their observed attributes. Alternatively one can think of matching as a method to reweight the untreated observations so that they have the same distribution of observable characteristics as the treated observations.[8] That is, conditioning on observables, **X** should eliminate the selective differences between programme participants and non-participants. Thus in focusing on (1) we make the assumption (following Imbens (2000) and Lechner (2001))

$$E[Y^0 | X, d = d'] = E[Y^0 | X, d = 0] = E[Y^0 | X].$$ (2)

In order to be able to utilise (2) it is necessary to make sure that there is a non-participant analogue to each participant, i.e.,

$$P^{d'|0,d'}(X) < 1.$$ (3)

where $P^{d'|0,d'}(\cdot)$ is the conditional choice probability of treatment d' given either treatment d' or no treatment, 0.

When a large number of covariates, **X**, is in use, matching can be difficult to implement due to the dimensionality of the problem. A way to circumvent the curse of dimensionality without imposing arbitrary assumptions is based on the results in Rosenbaum and Rubin (1983) and

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[7] The analysis of the multiple case presented below is formalised in Imbens (2000) and Lechner (2001). The notation is taken from Lechner (2001).

[8] A third way to think about matching is that it represents using predicted values from a non-parametric regression of the untreated outcome on observable characteristics, **X**, or on $P(X)$ as the estimated expected counterfactual outcome for each treated unit.

extended to the case with multiple treatments in Imbens (2000) and Lechner (2001). Here the focus is shifted from the set of covariates to the probability of programme participation, $P^{d'|0,d'}(\mathbf{X})$. As long as (2) and (3) hold it is shown that,

$$E[Y_0|P(X), d = d'] = E[Y_0|P(X), d = 0] \tag{4}$$

over the common support, $S_P = Supp\ (P^{d'|0,d'}(\mathbf{X})\ |d=d') \cap Supp\ (P^{d'|0,d'}(\mathbf{X})\ |d=0)$. This new conditioning variable, $P^{d'|0,d'}(\mathbf{X})$, changes the conditional independence assumption (CIA) into (4), which together with $P^{d'|0,d'}(\mathbf{X})<1$ are sufficient conditions required to justify propensity score matching to estimate the mean impact on the treated. Clearly, the functional form of $P^{d'|0,d'}(\cdot)$ is rarely known and has to be estimated, shifting the high-dimensional estimation problem from that of estimating $E\ [Y \mid \mathbf{X}]$ to that of estimating $E\ [D=d'|\mathbf{X}]$. In practice it is often estimated by a logit or, as in this paper, a probit, see the discussion in Black and Smith (2004) on this issue. Moreover, the adoption of a one-dimensional specification of selection clearly illuminates both the common support considerations as well as the differences in distributions of covariates that would not be addressed by standard OLS. The matching estimator implemented is described in Appendix A.

By focusing exclusively on the ATET we evaluate the costs and benefits below of the programmes compared to a situation *without* participation for those who historically ended up in the respective programmes. That is, the ATET is informative about the gross gain accruing to the economy from the existence of a programme compared to the alternative of shutting it down, but as argued by Heckman et al. (1999) this parameter is relevant in a cost benefit analysis only if there are no general equilibrium effects. It will be discussed at length later (Section 6) that some general equilibrium effects are not taken into account in the present paper while others are. Alternatively, one could do a pair wise comparison of the programmes, i.e., what would have been the effect of sending the subpopulation who self selected into programme d' compared relative to programme d''. This is done in Lechner et al. (2005), and their strategy is followed in an evaluation of Danish classroom training programmes in Jacobsen et al. (2006). Yet a third strategy would be to calculate and report average treatment effects (ATE). This may also be relevant in a cost benefit analysis, because it would allow us to compare the net benefits of the different types of programmes for the same hypothetical composition in all programmes. The latter strategy would require further assumptions than those currently invoked. Specifically, in assuming either (2) or (4) only, we allow for participants to select into the different training programmes based on idiosyncratic outcomes in the treated states. I.e., we only need to assume that participants and matched non-participants are equally productive in the *non-participation* state. To calculate ATE we would need to assume a counterpart to (2) for the missing participation outcome among those who did not participate.

4. Data and the selection into programmes

Our data set is a register-based 10% random sample of the Danish population for the years 1988–2005 consisting of two parts. The first part is annual observations on a long list of socioeconomic variables which are extracted from the integrated database for labour market research (IDA) and the income registers in Statistics Denmark. The second part is detailed event history information about the labour market state of the individuals.[9] That is, we know whether

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[9] This part of the data is based on information from four different administrative registers CRAM (unemployment), CON (employment) AMFORA (programme participation) and SHS (social income transfers, i.e., sickness benefits, maternity leave etc.).

the individuals are employed, unemployed, participating in ALMPs or out of the labour force in any week.

Since the main objective of the ALMPs is to improve the employment prospects of the participants, we evaluate the employment outcome of the unemployed. For that reason we construct the quarterly employment rate throughout the period 1995–2005 based on labour market spells - the quarterly employment rate is easily derived from information on the weekly employment status from the administrative registers. However, the earnings outcome is what is relevant for cost-benefit analyses since this measure also includes effects on hours worked and the hourly wage rate, thus capturing impacts on productivity and match quality. Therefore we also evaluate the earnings outcome, and we use annual labour earnings in the period 1995–2005 as our measure. The annual labour earnings are directly measured in the registers, as employers are bound by law to inform the authorities about the earnings of their employees, since it forms the basis for income taxation. Both the wage and employment measures are thus directly measured in public registers and must be considered highly reliable for this kind of analysis.

4.1. Sample selection choices

One restriction of the sample is to consider only UI fund members between 18 and 50 years of age. We exclude individuals above the age of 50 since this group is eligible for early retirement and other schemes for transition out of the labour force. We select those who were unemployed in the first week of 1995, and the four treatment groups then consist of those unemployed who end this "defining" unemployment spell by entering one of the four types of ALMPs. Thus our sample is a stock sample - everyone unemployed in the first week of 1995 - rather than a flow sample. As a result, relative to the population of all UI spells, our sample over-samples long spells. In terms of programme enrollment this will lead to an over-representation of participants in the residual programme category whereas participation in private job training and classroom training will be under-represented relative to the general population. To the extent that programme effects vary with unemployment dynamics up until enrollment, this will have consequences for the generalizability of our results to the full population of participants, but we will not address this question further in the present paper.

In the group of non-participants we include all unemployed as of the first week of 1995 who did not terminate their defining unemployment spell with ALMP participation. That is, we allow for cross-overs in the sense that they possibly participate in programmes following later spells of unemployment. After these sample restrictions there are 12,327 persons in the group of non-participants, while there are 501 participants in private job training, 1206 participants in public job training, 1241 participants in classroom training and 743 participants in residual programmes.

The length of the unemployment period before programme start must be expected to be an important factor behind whether the unemployed will participate in a programme, so to make meaningful comparisons a variable such as unemployment duration prior to participation must be constructed for the group of non-participants in some way, even if such a variable is not well defined for non-participants. We follow an approach suggested by Lechner (1999) and applied in e.g. Gerfin and Lechner (2002) and Larsson (2003). For each non-participant a hypothetical programme starting date from the empirical distribution of starting dates is drawn. Persons with a simulated starting date later than their actual exit date are excluded from the data set. Lechner (1999) compares this procedure to two alternative methods and finds that the one applied here

fares best with respect to two different summary statistics of the match.[10] In addition Lechner (2002) gives a sensitivity analysis of the procedure by using predicted starting dates (i.e. using variables that both influence the outcomes and the selection in predicting a start date for non-participants) instead of just simulated start dates from the raw distribution, as well as limiting the sample of participants to those only who start within the first three months after the sampling selection, and the results appear to be robust. After application of the procedure for the four programme types separately between 4044 (classroom training) and 5661 (private job training) individuals remain in the groups of non-participants. This means that a substantial fraction of the original group of 12,327 non-participants are relatively short-term unemployed in the sense that they are assigned a hypothetical programme starting week that comes after their actual exit from unemployment. It should be emphasized that this loss of data is not causing bias. Instead, it is part of the matching process, which keeps only those individuals who validly match against the true participants in the simulation.

One particular issue demands special attention when evaluating employment and earnings effects in a large-scale system such as the Danish, where programme starts are ongoing and differing across individuals, but where participation in principle is mandatory after a certain period of time in unemployment. Such problems are also encountered for the evaluations of the Swedish system of ALMPs, and Sianesi (2004) argues that to pick a comparison group among those who do not enroll in a programme amounts to conditioning on the future outcome of interest, since these unemployed do not enroll exactly because they have left the UI system or found employment by waiting long enough to receive an acceptable job offer.[11] Sianesi (2004) proposes a solution to this problem by pairing a member of the treatment group with a non-participant, who has remained unemployed for at least as long as the treated. In this case treatment effects should be interpreted as the effect of ALMP participation compared to waiting longer in unemployment. However, this approach is not appropriate for a cost-benefit analysis, because the desired counter-factual in principle is no participation at all (or as close to no participation as possible). In addition we think that the problems described above are less pronounced in our analysis, since by January 1995 the unemployed were allowed a very long period of four years of unconditional UI benefits before ALMP participation becomes mandatory, so "no participation" is by no means equivalent to "employment".[12] However, we cannot completely rule out that the estimated treatment effects are plagued somewhat by problems related to conditioning on the future. If that is the case it should be kept in mind, that the employment rate among non-participants is too high, and so any bias is towards finding that the programmes do not work, cf. Fredriksson and Johansson (2004).

Fig. 1 shows the number of quarters until participation in the first programme measured from the start of the defining unemployment spell. Private job training are used relatively early in the unemployment spell, but overall there is a high degree of variation in the timing of programme starts. I.e. during the first two years the quarterly enrollment rate lies fairly constant between 6 and 10 percent (with the exception of private job training).

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[10] The two summary statistics are the median absolute standardized bias (see Rosenbaum and Rubin (1985)) and a joint Wald test for paired mean differences and as such the tests measure the ability of the three procedures to equate the values of the conditioning variables for the treated with those of the matched controls.

[11] This argument is formalized by Fredriksson and Johansson (2004).

[12] This is backed up by the fact that only 1.5 percent of the individuals in the sample are in the midst of an unemployment spell that ends up lasting at least four years with no ALMP participation when the data start in January 1995.
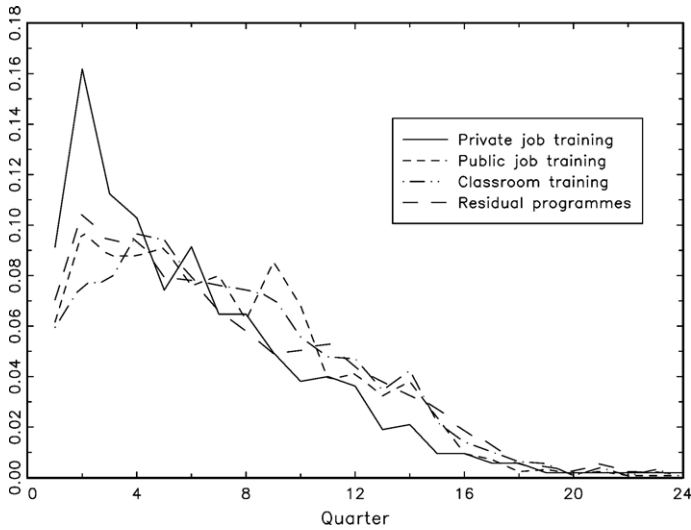
Fig. 1. Number of quarters until participation.

Table 2 shows the proportion of time the different sub-groups of the data spend in programmes for 1995–2005, where no distinction is made between programme types (i.e. subsequent ALMP participation could be of a different type). Participants in public job training and residual programmes appear to have the highest average participation rates and it is declining towards 5% in 2005. The numbers in parantheses show that even if participants are allowed to switch to other programme types later on, the initial programme type remains the dominant type during the first years where participation rates are high.

It is seen that for the group of non-participants the proportion of time spent in programmes rises to 11% in 1999 after which it declines. The participation spells for non-participants are primarily due to enrollment in classroom training and residual programmes following intermediate spells of employment and unemployment. Due to these positive participation rates non-participation in the present analysis does not represent a world entirely without ALMP participation. However, the participation rates of participants and non-participants tend to converge after 4–6 years,[13] such that the estimated effects of ALMPs primarily should be ascribed to participation rate differentials during the first 3–4 years. Thus to sum up, the estimated treatment effects will be close to the pure effect of the programme used to define the treatment status.

4.2. The selection process into programmes

As outlined in Section 2.2 an important determinant of the selection of the unemployed into programmes is the individual's history in the UI system, i.e. in 1995 the unemployed were entitled to four years of unconditional benefits before they were obliged to participate in ALMPs. To be more precise, it is the individual's seniority in the UI system that matters for participation, and in 1995 the UI seniority was reset whenever the individual had been employed for 26 weeks.[14] Thus

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[13] It is important here to note that if one considers only matched non-participants (as opposed to all potential controls in Table 2) the participation rates almost coincides already after around two years.

[14] This requirement was strengthened to 52 weeks by January 1997.

Table 2
Proportion of Time Spent in Programmes [a]

Year	Private job training	Public job training	Classroom training	Residual programmes	Non-participants
1995	0.39 (0.97)	0.39 (0.98)	0.18 (0.90)	0.30 (0.95)	0.02
1996	0.23 (0.71)	0.38 (0.80)	0.31 (0.80)	0.49 (0.92)	0.07
1997	0.13 (0.29)	0.32 (0.54)	0.28 (0.64)	0.51 (0.88)	0.09
1998	0.13 (0.14)	0.25 (0.42)	0.24 (0.51)	0.28 (0.79)	0.11
1999	0.09 (0.08)	0.19 (0.29)	0.18 (0.51)	0.16 (0.57)	0.11
2000	0.07 (0.12)	0.14 (0.26)	0.11 (0.43)	0.10 (0.50)	0.08
2001	0.07 (0.14)	0.12 (0.26)	0.09 (0.42)	0.10 (0.51)	0.06
2002	0.07 (0.10)	0.12 (0.22)	0.09 (0.32)	0.10 (0.57)	0.06
2003	0.06 (0.05)	0.10 (0.24)	0.07 (0.33)	0.07 (0.51)	0.05
2004	0.05 (0.17)	0.09 (0.38)	0.07 (0.31)	0.06 (0.32)	0.05
2005	0.05 (0.06)	0.07 (0.31)	0.06 (0.34)	0.05 (0.30)	0.04
# individuals	501	1206	1241	743	12,327

[a] The time spent in programmes is calculated as weeks spent in any programme type in a given year divided by 46 (annual number of working weeks). The columns are defined from the individual's initial programme type. Numbers in parentheses indicate the initial programme type's proportion of total time spent in programmes.

these legislative facts should be captured by our modelling of the selection process, and to that end we use information on the exact duration of the present unemployment spell (in weeks) and a precise measure of the UI seniority; the number of weeks the unemployed previously were unemployed and received UI benefits at the beginning of the present unemployment spell (taking into account the 26 weeks employment requirement).

We believe that all the factors that affect both participation and outcomes can be captured by a very long list of additional regional and individual socioeconomic variables and variables measuring labour market history. Of demographic variables we include four age group dummies, gender, marital status, dummies for age of children, citizenship, and housing type. Attained education is captured by dummies for basic schooling, high school and further education with vocational education as reference category. We also include the rate with which UI benefits replace the latest observed wage rate. This rate has a rather high ceiling of 90%. Individual wealth is also observed, and so is union membership. In Denmark the ALMPs are administered by local councils at the county level, and administrative practices have been observed to deviate somewhat, so to control for such differences and other local labour market differences we also include dummies for each county, 13 different regions in all. Local labour market behavior may also be influenced by the size or thickness of the labour market, so we also distinguish between Copenhagen, the five largest cities beside Copenhagen and other parts of the country.

Previous studies (see Card and Sullivan (1988), Heckman et al. (1999) and Heckman and Smith (1998)) have shown that a key predictor of participation is recent labour market dynamics/transitions. Individuals recently entering unemployment either from outside the labour force or from previous employment are most likely to seek participation in programmes. As recent labour market dynamics will also be pivotal in explaining future outcomes, it is apparent that such variables are needed in our analyses below, and we have therefore constructed several measures for individual labour market history. As mentioned, UI seniority is included along with a variable indicating whether the unemployed started the unemployment spell with no UI seniority. Also there is a variable measuring labour market experience since 1964. There are variables indicating the number of previous unemployment spells and whether the present unemployment spell is the first. As discussed above we also include unemployment duration prior to participation, and to

Table 3
Selected sample means

Variables [a]	Private job training	Public job training	Classroom training	Residual programmes	Non-participants
Age	31.09	34.32	33.15	33.73	31.94
Woman	0.43	0.58	0.52	0.44	0.51
Education	2.20	2.02	2.33	2.22	2.31
Union member	0.85	0.91	0.83	0.78	0.85
Experience in years	6.64	7.17	7.06	6.98	7.35
UI replacement rate	0.75	0.75	0.73	0.74	0.73
Proportion with no UI seniority	0.47	0.48	0.52	0.56	0.54
UI seniority	26.54	30.85	23.89	24.50	19.50
First U spell	0.14	0.12	0.17	0.15	0.17
No. of U spells 1994	1.06	1.11	0.98	0.94	0.87
Unemployment duration 1994	70.18	85.98	88.65	88.41	74.93
Mean dur. E. 1993–94	78.62	60.38	81.19	76.73	84.74
Mean dur. U. 1993–94	28.88	36.24	30.61	29.96	23.71
Fraction unempl. 1994	0.25	0.31	0.25	0.27	0.21
– quarter up to particip.	0.82	0.86	0.85	0.84	0.77
Fraction employed 1994	0.61	0.52	0.58	0.56	0.63
– quarter up to particip.	0.09	0.04	0.07	0.09	0.19
Fraction sick. comp. 1994	0.002	0.002	0.003	0.003	0.003
Private JT 1994	0.04	0.01	0.01	0.02	0.01
Public JT 1994	0.03	0.09	0.04	0.03	0.03
Classroom Tr 1994	0.02	0.02	0.04	0.00	0.01
ALMP participant 1994	0.11	0.12	0.09	0.10	0.06
# individuals	501	1206	1241	743	12,327

[a] The education variable is calculated as the share of the population with primary schooling [*]1+the share of the population with high school[*]2+…+the share of the population with long further education[*] 6. The UI seniority denotes the number of weeks the unemployed previously were unemployed and received UI benefits at the beginning of the present unemployment spell. 'First U spell' indicates whether the present unemployment spell is the first and 'No. of U spells' denotes the number of previous unemployment spells. Unemployment duration in weeks for non–participants is simulated using the empirical distribution of starting dates. Two variables measure the mean duration of employment and unemployment spells respectively during 1993–94. The three 'Fraction' variables measure the fraction of time spent in unemployment, employment and receiving sickness benefits respectively in 1994. The last four variables are dummies for participation in programmes in 1994.

capture aspects of the distribution of unemployment duration indicator variables for unemployment durations are included as well. Mean duration of previous employment and unemployment spells and the fraction of time spent in employment and unemployment are included. Further, a variable for the fraction of time previously spent receiving sickness benefits is included as a crude measure for health status along with dummies for ALMP participation in 1994.[15] In addition, there are variables for income and earnings in 1993 and 1994.[16] Table 3 contains descriptive statistics for some key variables in our analysis.

Even after controlling for this wealth of information we cannot rule out that there is unobserved heterogeneity left which is correlated with employment outcomes and programme participation. For example we do not have variables capturing motivation, personal appearance or

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[15] Before 1994 participation in programmes was much less common (see Section 2.2). The programmes tended to work as ways of extending the (nine years) UI eligibility requirements, and information about participation in earlier years is not available in the data.

[16] An exhaustive listing of variables included in the analysis is given in the note to Table 4.

Table 4
Average derivative estimates and asy. std. err. from participation probits [a]

Variables	Private job training		Public job training		Classroom training		Residual programmes	
	Coeff.	Std.err.	Coeff.	Std.err.	Coeff.	Std.err.	Coeff.	Std.err.
Age 18–25	−0.39	1.13	−2.37	1.60	1.10	1.97	−0.61	1.58
Age 35–39	−1.46	1.05	**3.97**	1.56	1.40	1.87	−0.50	1.37
Age 40–44	−1.80	1.16	2.42	1.71	0.41	2.02	**−2.86**	1.44
Age 45+	**−4.17**	1.07	**3.97**	1.94	1.81	2.18	−1.17	1.68
Woman	**−3.58**	0.91	1.78	1.19	−1.59	1.45	**−4.24**	1.17
Basic schooling	−0.97	3.71	2.33	5.73	6.55	7.05	−7.87	5.60
High school	−0.15	3.25	0.75	5.38	3.38	6.88	−4.75	4.22
Medium further education	5.72	4.17	3.57	4.89	6.33	4.81	−2.76	3.13
Union member	0.53	1.12	**7.23**	1.38	1.51	1.68	−2.46	1.44
Experience	−0.09	0.26	−0.47	0.33	−0.50	0.39	0.07	0.31
UI replacement rate	3.82	2.43	3.21	3.29	−3.82	3.92	−4.52	3.04
No UI seniority	−1.28	1.15	1.53	1.45	−1.45	1.83	2.42	1.40
UI seniority	−0.03	0.03	−0.03	0.03	−0.03	0.04	**0.10**	0.03
First U spell	0.88	1.70	4.17	2.35	1.83	2.56	−0.98	1.96
No. of U spells 1994	1.33	0.88	1.45	1.05	1.71	1.38	**2.03**	0.99
Unempl. dur. 1994	−0.05	0.01	−0.06	0.01	0.02	0.02	0.03	0.02
Mean dur. E. 1993–1994	0.00	0.01	0.00	0.01	0.01	0.01	0.01	0.01
Mean dur. U 1993–1994	0.01	0.02	0.03	0.02	0.06	0.03	0.00	0.02
Fraction unemployed 1994	0.51	4.27	−5.76	4.82	0.46	6.76	3.61	5.08
–quarter up to participation	0.06	2.71	−1.74	3.64	−8.13	4.60	4.30	3.74
Fraction employed 1994	8.73	6.27	−4.05	8.66	−1.72	10.2	**−27.4**	8.2
–quarter up to participation	1.12	3.03	−2.53	4.45	−4.87	5.33	6.67	4.00
Fraction sickness comp. 1994	−1064	834	−57	223	0	125	−293	404
Private JT 1994	8.15	7.27	0.41	7.29	11.52	8.38	−5.10	3.39
Public JT 1994	−2.38	3.53	7.33	7.05	**33.29**	10.01	**−9.80**	3.32
Classroom Tr 1994	3.78	6.27	3.65	7.40	−4.65	9.35	**14.82**	7.41
ALMP participant 1994	0.71	4.32	−0.04	6.30	0.69	8.54	14.59	7.86

[a] See note to Table 3 for variable explanations. In addition to the variables reported here, we include county and region dummies (13 in all), marital status, citizenship, a number of dummies for the presence of children at different ages in the household, 9 dummies representing previous sector of work (service, trade, construction etc.), size of city of residence, and wealth. Besides, we include in total 6 dummies for educational level and 17 dummies for educational type, membership of UI fund, a dummy for whether the person lives in a city, and housing type. Finally we include a long list of historical employment and unemployment status information: dummies for the length of the unemployment spell leading up to participation, quarterly employment rates from first quarter 1988 until 1995, quarterly unemployment rates from first quarter of 1993 until 1995, quarterly rates of time on cash benefits from first quarter 1993 to 1995, earnings and income transfers in 1993 and 1994, personal wealth in 1994, number of unemployment, employment, sickness and non-labour market participation spells in 1993 and 1994, and a dummy for whether a person's unemployment insurance seniority exceeded 52 weeks at the beginning of 1995. Calculations are based on Jonah Gelbach's margfx.ado, version 4.3. Effects of dummy variables correspond to change of variable from 0 to 1. Bold numbers indicate significance at the 5% level.

caseworker's assessment of the unemployed's chances to find job. However, as is standard in the literature, we have to rely on their indirect effects on observed labour market history. Compared to most other evaluation studies our data set is very detailed, and we think that there is sufficient information to make the CIA plausible.[17]

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[17] We conjecture that left-out variables such as motivation are highly correlated with past labour market experience, and that taking past behaviour into account to a large extent captures the current period's motivation; see Heckman et al. (1998).

To proceed we need for each individual a predicted probability of participation in each of the four programme types. The results of running four binary probit models for participation in each of the four programme types (relative to non-participation) are shown in Table 4. Important determinants of the selection process seem to be gender, union membership, UI seniority and more generally labour market history.

5. Long run employment and earnings effects

5.1. Quality of the match

After estimating the propensity scores the next step is to restrict the sample to the common support. We impose the common support condition separately for each pairwise comparison. That is, for all four pair-wise comparison groups we follow Heckman et al. (1998) and impose a trimming rule that cuts out treated and non-participants in regions where the densities of the counter factual state are 'thin'.[18] This gives rise to a small loss of observations in the treatment groups of between 1.9 percent (residual programmes) and 2.6 percent (private job training). The quality of the match can be further studied by calculating and comparing means of the covariates for the treated and the matched non-participants, and most variables have a very small differential between treated means and matched means.[19]

5.2. Treatment effects

The estimated average quarterly employment effects from the matching analysis are shown in Fig. 2. For all four programme types they start out negative and become positive after some time (except public job training which only recovers to zero). The initial dip in the employment rate differential reflects the locking-in effect, i.e. the participants are not searching while participating in ALMPs. For private job training the effect becomes significantly positive after five quarters and it seems to converge rather quickly to around 5 percentage points. The sudden increase in the employment rate can to a large extent be explained by the fact that many participants continue without subsidies in the same firm after the end of the programme. Public job training spells are on average of longer duration than private job training which is presumably one reason behind the relatively long-lasting negative effect. The employment effects for public job training highlight the need for analysing long-term effects, since it is not until 2000 that the employment rate differential recovers and stabilises around zero. Classroom training is somewhat similar in the sense that the employment rate differential is rising steadily until 2000, but the differential seems to stabilise at a slightly higher level although it is not significantly greater than zero. Residual programmes seem to have severe locking-in effects, but in this case the effects also turn positive after 3–4 years. One of the important programme types in the group of residual programmes is entrepreneurship subsidies; see Table 1. These programmes lasted on average almost two years, so they clearly have contributed to the locking-in effect. The entrepreneurship subsidy programme was abolished in 1998, and this may partly explain the sharp increase in the employment rate around the 12th quarter in Fig. 2. Overall, private job training appears to perform

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[18] The details of the trimming procedure appear in the appendix.

[19] We also obtain reasonably balanced covariates after matching on our estimated propensity score. In no case do the standardized differences in means for covariates (Rosenbaum and Rubin (1985)) exceed 9% and rarely does it exceed 5%.
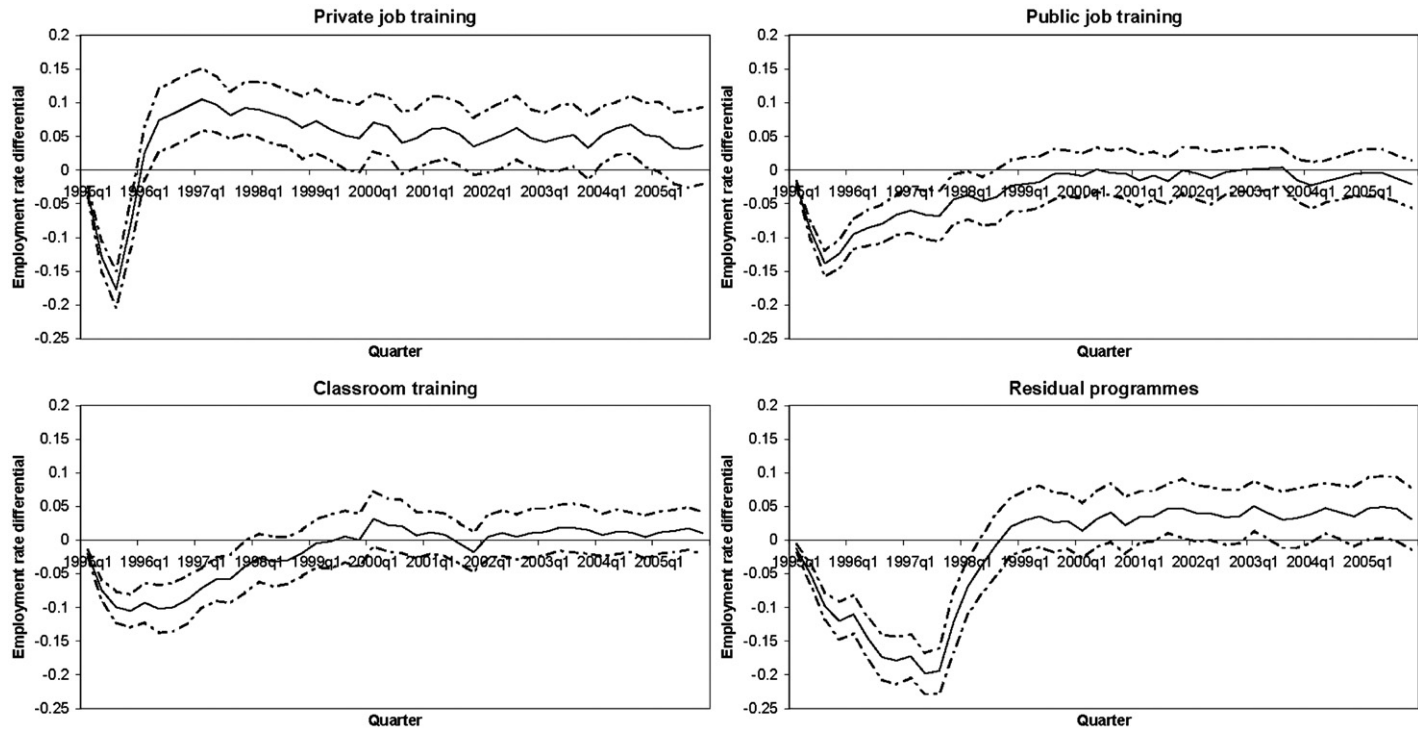
Fig. 2. Average employment effects of programmes (solid) with corresponding 95% confidence bounds (dashed). Effects found by matching. See footnote of Table 5 for technical details.

best in terms of improving the employment prospects of the participants, which is consistent with the short-term results found in Munch and Skipper (in press).

In addition to the employment effects of Fig. 2, programme effects on individual earnings capture effects on hours worked and the hourly wage rate, which is relevant for a cost-benefit analysis. Table 5 shows annual earnings effects, and it is clear that for participants in private and public job training there is an initial rise in earnings because of wage income during participation. This effect then declines but for private job training it is seen to stabilise at a high level. The earnings gain constitutes a rise of 8.9% in 2005 for the participants in private job training, and this must be regarded as a high number. Participants in classroom training have a positive earnings effect after three years, but it is not significantly different from zero. Finally residual programmes have negative earnings effects throughout the period 1995–2005.

One property of our measure for labour earnings - which helps explain the high initial earnings effects for job training - should be emphasized. The registered annual labour income consists of all taxable wage income of the individual, which means that wages earned while enrolled in subsidised private and public job training as well as some employment programmes in the group of residual programmes are included (benefits received while in e.g. classroom training are not included because this is an income transfer, not labour income). Clearly this explains why there is an immediate positive earnings effect of e.g. private job training while at the same time there is a negative employment effect due to locking in, cf. Fig. 2. Put differently, participation in job training increases annual labour earnings but not quarterly employment rates, and the earnings effects should be interpreted with this in mind.

How does this feature of the data affect the treatment effects which are to be used in the cost benefit analysis? To the extent that output produced during participation in subsidised job training programmes has a value equal to the wage earned, this is in fact precisely the earnings measure we

Table 5
Estimated Treatment Effects

Year	Private job training [b]		Public job training [c]		Classroom training [d]		Residual programmes [e]	
	Coeff.	Std.err.	Coeff.	Std.err.	Coeff.	Std.err.	Coeff.	Std.err.
1995	**0.422**	0.032	**0.400**	0.019	**−0.131**	0.020	**−0.080**	0.024
1996	**0.456**	0.050	**0.312**	0.033	**−0.097**	0.038	**−0.184**	0.038
1997	**0.281**	0.053	**0.150**	0.034	0.000	0.045	**−0.184**	0.047
1998	**0.245**	0.059	**0.087**	0.038	0.037	0.046	**−0.232**	0.057
1999	**0.243**	0.074	0.032	0.041	0.050	0.052	**−0.239**	0.061
2000	**0.176**	0.073	0.039	0.039	0.084	0.055	**−0.239**	0.061
2001	**0.204**	0.072	0.031	0.042	0.086	0.050	**−0.140**	0.063
2002	**0.177**	0.074	0.027	0.038	0.088	0.054	**−0.156**	0.063
2003	**0.164**	0.066	0.047	0.040	0.080	0.057	**−0.140**	0.057
2004	**0.179**	0.065	0.018	0.042	0.054	0.054	−0.123	0.069
2005	**0.148**	0.071	−0.003	0.046	0.037	0.049	−0.142	0.074

Dep. variable: Annual earnings in 100,000 DKK [a]

[a] Estimated using kernel based matching. Kernel type and bandwidth were selected using cross validation. The overlapping support regions were determined using a 2% trimming rule. Std. errors are based on 399 bootstraps with 100% resampling. See Heckman et al. (1998) and Black and Smith (2004) for technical details. Std. errors appear in parentheses. Bold numbers indicate significance at the 5% level.
[b] Biweight kernel used with $h=0.0596$. 488 participants and 5389 matched comparison units used.
[c] Biweight kernel used with $h=0.0477$. 1182 participants and 5148 matched comparison units used.
[d] Biweight kernel used with $h=0.0518$. 1215 participants and 4019 matched comparison units used.
[e] Biweight kernel used with $h=0.0716$. 729 participants and 4317 matched comparison units used.

desire - production during participation is, of course, a benefit. However, we believe that the value of output produced may not have a value equal to the paid wage. It seems reasonable to assume that a lower bound for the value of production is the paid wage minus the subsidy received by the employer, because otherwise it would not be beneficial to the employer to accommodate participants. Therefore, in the following we choose a conservative approach and value output produced as the difference between the wage and the subsidy. That is, the subsidy received by employers must be counted as a cost, and these subsidies are relatively straightforward to calculate. The subsidies per unit of time per participant can be deducted from the annual reports from the Danish Labour Market Agency, since they state total expenditure on subsidies to private and public employers as well as the number of full time equivalent persons being employed with a subsidy. Given that we know exactly how many weeks the unemployed spend in job training (also for employment programmes within the group of residual programmes), it is now easy to calculate the total subsidies received by employers as a result of taking in participants in the sample.

6. The net social return to Danish ALMPs

The previous section showed that the programmes analysed have significant effects on individual earnings and employment. To assess whether the programmes are desirable from society's point of view it is necessary to estimate the value of those benefits and other benefits of the programmes and compare the benefits to the costs. Following the dominant approach in the evaluation literature (see Heckman et al. (1999)) we measure the net social return as the change in aggregate output attributable to the programmes by subtracting the programmes' discounted costs from their discounted stream of benefits.

Starting with the benefit side, the discounted earnings impact is derived from the treatment effects on annual earnings from Table 5. This benefit is in most cost-benefit analyses found to be of much larger magnitude than other measured benefits, cf. Heckman et al. (1999). We also take into account the value of output produced during participation in job training, so as discussed above we need to subtract subsidies received by employers accommodating participants.

Among potential benefits not included are possible effects on the labour market behaviour of the unemployed prior to participation. It may be that the prospect of ALMP participation encourages the unemployed to intensify their job search before entering the programmes in order to avoid participation. Geerdsen (2006) estimates such ex ante effects for unemployed members of Danish UI funds and finds a positive effect. It may also be that the prospect of ALMP participation lead unemployed persons, who are not genuinely interested in finding a job, to leave the labour force and stop collecting UI benefits.

On the cost side we take into account direct operation costs of the programmes, which include purchase of education materials, teacher time etc. related to classroom training and administration costs related to each programme. The direct operation costs are calculated using the public annual accounts of the Danish Labour Market Agency, and they are stated per full time equivalent participant. Individuals in the sample potentially participate in several programmes during the observation window. Thus to calculate costs per participant in a particular programme we first calculate the average amount of time spent in different programmes at different points in time. The obtained full time equivalents are multiplied by the cost per full time equivalent for each programme in each year.

It is likely that the scale of the programmes in Denmark implies that there are significant general equilibrium effects. One such general equilibrium effect, which is taken into account in this paper, is the deadweight loss of taxation to finance benefits, subsidies and operation of programmes, see e.g. Browning (1987). First of all this implies that the direct resource costs of the

programmes should be multiplied by a factor greater than one to capture the distortions arising from financing the costs by raising tax revenue. In addition, transfers in the form of UI benefits and subsidies to employers are not costly to society per se, since they are just transfers of consumption possibilities from one group of citizens to another. However, these transfers must be financed by raising tax revenue, thereby causing a deadweight loss. Thus, if the programmes have an impact on the overall level of transfers, there will also be a change in the resulting deadweight loss of taxation. For example if the programmes are successful in improving the job opportunities of participants, the society incurs savings in deadweight losses due to reduced taxes required to pay participants' future unemployment benefits. Another example is that the subsidies paid to private employers only cover a part of the participants' wages; the remaining part is paid for by the employers, which reduces public sector expenses and thus leads to savings in the deadweight loss of taxation.

Estimates of the size of the deadweight loss vary greatly from one empirical study to another. In our baseline scenario we assume a deadweight loss of 75% of the change in public expenses due to ALMP. This figure is chosen as the midpoint in the range of estimates for Denmark provided by Kleven and Kreiner (2006).

Some potentially important costs, which are not considered in this paper, are the effects on participants' available leisure time. Danish ALMPs have significant locking-in effects, so the loss of leisure time may lead to a significant loss of welfare. Greenberg (1997) stresses that failing to account for this cost will bias evaluations in the direction of more positive evaluations of programmes which increase participants' hours of work. Furthermore, it is likely that the job training programmes, which involve a wage subsidy, lead to a displacement effect of non-subsidised workers as described by e.g. Dahlberg and Forslund (2005). They find evidence of displacement effects of Swedish ALMPs of about 65% in programmes whose main mechanism is wage subsidies. There may also be important effects on the macroeconomic wage formation because the search activity of the unemployed is reduced during participation. However, Danish Economic Council (2002) finds no evidence of such effects in the Danish labour market.

7. Costs and benefits compared

The estimated net social returns to Danish ALMPs are presented in Table 6. The first component of the net social benefits is the present discounted value of the estimated earnings

Table 6
The economic value of Danish ALMPs [a]

	Private job training		Public job training		Classroom training		Residual programmes	
	NPV	Std.err.	NPV	Std.err.	NPV	Std.err.	NPV	Std.err.
Earnings effect	**216.0**	39.0	**102.8**	21.7	12.5	28.9	**−140.1**	34.0
+Transfers	**91.7**	10.7	**78.9**	8.7	−0.8	8.5	**48.6**	11.1
−Unit costs	−5.2	5.1	5.1	3.5	**89.8**	5.1	−1.8	4.2
−Subsidy	**34.3**	2.2	**89.5**	2.8	**6.7**	1.6	1.6	2.1
Net benefit	**278.5**	48.8	**87.1**	29.5	**−84.7**	37.4	**−91.3**	41.3

[a] The stated PDVs are the sum of annual values from 1995–2005 discounted by an annual rate of 6% as suggested by Danish Ministry of Finance (1999). The deadweight loss of taxation is assumed to be 75% of the public expense on e.g. administration of the ALMP. Unit costs cover the cost of education per full time equivalent participant and costs of administration. Negative unit costs are possible to the extent that the non-participants have a higher participation rate in classroom training. All values are stated in 1000 DKK deflated to 2005 using the GDP deflator. Bold numbers indicate significance at the 5% level.

gains from Table 5. Next is the reduced deadweight loss of taxation resulting from the reduced income transfers in the form of unemployment insurance payments and various means tested benefits following from higher employment. From these gains are deducted the unit costs of administration and the unit costs of classroom training corrected for marginal cost of public funds. These direct operation costs are based on information about time spent in the different ALMPs by participants. Finally we have to adjust for the fact that the earnings measure behind the earnings effects in Table 5 includes labour income during participation in job training. As stated earlier we assume that the value of the output of participants in job training equals the difference between the wage and the subsidy, so to get a correct account of a persons' productivity we have to subtract the subsidy from the earnings effects. This is based on information about the size of the subsidy and how much time each individual spends in job training (see Section 5), and the resulting value includes the deadweight loss of taxation from financing the subsidies.

It is apparent from Table 6 that private job training performs best with a surplus of approx. 279,000 DKK per participant (around 38,000 Euro) over the eleven years from 1995 to 2005. This surplus can primarily be attributed to an earnings gain of 216,000 DKK, which is reduced somewhat after correcting for the subsidy. There is also a notable saving on tax distortions due to reduced UI benefits, which amounts to almost 92,000 DKK. Likewise, for participants in public job training there is a large earnings gain, but around 90 percent of the earnings gain is lost due to wage subsidies and deadweight losses associated with the subsidies. Here it should be recalled that we assume that production during participation has a value equal to the difference between the wage and the subsidy — an assumption that is probably more likely to be violated for public job training than for private job training. Classroom training has a deficit of about 86,000 DKK. This deficit is mainly due to the considerable direct operation costs of the programme. Finally, residual programmes end up with a big deficit of 90,000 DKK which is due mainly to a loss in earnings. Residual programmes include entrepreneurship subsidies, and for people who start up their own firm in this programme the earnings effect might be biased downwards due to tax evasion activities.

The results in Table 6 do not take into account displacement effects of job training and the cost of lost leisure. There is not much knowledge about these effects, but Dahlberg and Forslund (2005) find that displacement effects constitute some 65% of the employment effect. Greenberg (1997) finds that for relevant compensated labour supply elasticities (0.1 for Denmark) and for relevant employment effects (around 0.05 for private job training and around 0 for public job training) the surplus for the participant lies in the range of 7% to 23% of the earnings effect. If, say, 80% to 90% of the earnings effect disappears due to displacement and lost leisure, the NPV of the Danish public job training programmes would become negative, but private job training would still generate a surplus due to the effect on income transfers and the high cost-effectiveness of the programme. This is most tentative and is just an example of the potential importance of displacement and lost leisure.

Including long term effects makes a great difference to the estimated net social benefits. The long term effects are most pronounced for private job training where we find positive and significant effects on earnings and income transfers for almost all 11 years following participation. For public job training the positive and significant effects last for a shorter period than is the case for private job training. In the case of classroom training, failure to account for long run effects would give rise to a negative and significant earnings effect. With the long run effects included, the earnings effect becomes positive but insignificant. For the residual programmes, the long run effects are generally negative and significant.

7.1. Robustness checks

This section discusses a number of extensions of our base results in Table 6. Sensitivity analyses show that using deadweight losses in the range from 30 to 120 percent can change the NPV-ranking of the programmes (see Table A1 in the Appendix). For values of the deadweight loss of taxation of 30 percent and 75 percent, the ranking in descending order of NPV is 1. private job training, 2. public job training, 3. classroom training and 4. residual programmes. However, for a deadweight loss of 120%, residual programmes and classroom training switch places in the NPV ranking. This is due to large expenditure on classroom training, which becomes more important when the deadweight loss increases. Increasing the deadweight loss of taxation increases the NPV of private job training but decreases the NPV for the other programmes. Private job training and public job training come out with surplusses irrespective of the assumed deadweight loss and discount rate, while classroom training and residual programmes never have a surplus.

The results presented in Table 6 may also cover differences across sub-populations. To explore this we have performed a sensitivity analysis for different sub-populations; men and women, and three different educational groups (less than high school, high school and vocational education). The group of unemployed with a tertiary education was too small to obtain results and the same holds for unemployed with high school diploma who participate in private job training. Table A2 in the Appendix shows that men gain most from public job training, while women gain most from private job training. Also the deficit from classroom training is smallest for women. With respect to educational subgroups the most noteworthy result is that unemployed with just high school diploma appear to have a much greater surplus from participation in public job training, and they also have a small positive net benefit from participation in classroom training.

Finally we have also performed a sensitivity analysis with respect to the definition of the non-treatment group. An alternative estimation strategy, which has also been employed in the literature, is to use a window of, say, six months, where individuals who enroll in a programme are defined as treated and those that do not are defined as the non-treated. That is, the trimming procedure of Lechner (1999) is no longer applied to the non-treatment group. We have used this approach with windows of six months and one year, see Table A3 in the Appendix.

Selecting a window of six months increases the net present value of private job training with a third. This change stems from changes in all four sources with the largest change coming from saved cash transfers. Whether this is due to the change in method or change in treated population (which drops by almost 40 percent) is not possible to determine. With a window of one year the differences between the methods are less pronounced - both in terms of treated sample size and effects. The largest difference are savings in cash transfers of almost 20 percent. As for public job training no economically significant difference emerges between the methods in terms of net present value of the programme. This covers the fact that both the net present value of earnings and the savings in transfers (which should decrease the overall value of the programme to society) are smaller but so are the subsidies. The effects of classroom training turn out to be more sensitive to the estimation approach. The sample size decreases by more than 60 percent with the shorter window and the net present discounted value is now positive. The differences in value come mainly from the differences in earnings and only to a smaller degree from saved transfers. For the residual programmes the negative impact more than triples - an effect coming from the effects on earnings. Here it should be noted that even with a window of one year, the number of treated individuals is only two thirds of the population in Table 6.

8. Conclusion

This paper has estimated long-term employment and earnings effects for participants in the large scale system of ALMPs in Denmark in the period 1995–2005. The treatment effects are estimated by propensity score matching on a very detailed administrative data set, which allows us to control for much individual heterogeneity. A cost benefit analysis is done for the ALMPs analysed, taking into account the earnings effects, effects on income transfers, unit costs of the programmes and the wage subsidies associated with job training programmes, as well as the deadweight loss of taxation associated with financing the ALMPs.

We find for participants followed from 1995 to 2005 that private job training generates a very high social surplus, which is mainly due to substantially higher earnings and reduced income transfers after participation. Public job training also generates a significant social surplus due to earnings and transfer effects. Classroom training generates a significant deficit, due to weak earnings and transfer effects and substantial costs of administration and operation.

It turns out to be very important to derive long-term treatment effects, since participants in most programmes initially experience severe and long lasting negative locking-in effects due to programmes of long duration. Positive post-programme effects eventually become important, but typically not until after 1–3 years. The long-term effects are important in order to capture all the social gains and losses from the programmes, and to take into account the profile of training programmes, which tend to first yield deficits but later surplusses.

Cost-benefit analysis of large-scale programmes such as the Danish active labour market programmes involves bringing together information from many and diverse sources and the results should be interpreted with caution. First of all, some potentially important general equilibrium effects, such as e.g. the displacement effect of subsidised job training programmes, are omitted, which probably biases the results of the analysis in favour of public and private job training versus classroom training. Second, for the purposes of estimation of the programme effects, some programmes are pooled into larger categories of programmes. This makes policy recommendations potentially unreliable. Third, ex ante effects on the transition rate out of unemployment are disregarded (a neglected benefit). Fourth, using estimates of marginal cost of public funds based on one analytical framework and estimates of individual effects of active labour market programmes from another analytical framework is also potentially problematic. Nevertheless, it is necessary to combine information on both costs and benefits in order to arrive at policy relevant results. This paper has made a first attempt at performing a cost-benefit analysis of Danish active labour market programmes, and an important contribution of the paper is that it may lead to the identification of areas where further research is necessary in order to make analyses of ALMP more policy relevant.

Given the relatively scarce literature on cost-benefit assessments of ALMPs - particularly in Europe - this paper contributes with new knowledge on that front. It follows from the vast literature that confines attention to deriving treatment effects and neglect the cost side, that the success of the different programmes are at best mixed. To the extent that treatment effects alone are negative this picture can only be reinforced by taking the costs of the programmes into account. In that respect our results are more promising, and an important lesson is that long-term effects are required to arrive at a more accurate cost-benefit evaluation of the programmes.

Appendix A. The matching estimator

The different matching estimators implemented in the literature today all take on the following generic form

$$
\Delta = \frac{1}{m_{d'}} \sum_{i \in I_{d'} \cap S_p} \left(Y_i^{d'} - \sum_{j \in I_0} W(i,j) Y_j^0 \right), \tag{5}
$$

where $I_{d'}$ denotes the set of people receiving treatment d', I_0 the set of comparison units, and $m_{d'}$ denotes the number of persons in the set $I_{d'} \cap S_p$. Notice how the match for each participant $i \in I_{d'} \cap S_p$ is constructed as a weighted average over the outcomes of non-participants, where the weights, $W(i,j)$, are constructed such that they depend on the distance between $P_i^{d'|0,d'}$ and $P_j^{d'|0,d'}$, where $P_i^{d'|0,d'} \equiv P^{d'|0,d'}(X_i)$. The matching estimators we implement below differ in how the weights are constructed.

We apply seven different matching estimators: the simple nearest neighbour estimator that pairs only a single comparison unit in making the match, as well as three versions of kernel and local linear estimators respectively, that all construct matches for each participant using kernel weighted averages over multiple comparison units. We consider both a Gaussian kernel (with infinite support) as well as Epanechnikov and Biweights kernels (that both are bounded on their support).

In choosing among the seven different estimators as well as selecting bandwidths for the six kernel and local linear based estimators we follow the suggested method of Black and Smith (2004). That is we use a least squares 'leave-one-out' validation mechanism. This mechanism uses the observations in the group of non-participants to determine which one of the seven competing models fits the data best using root mean square error (RMSE) as the objective function to be minimised.

With the large sample sizes at hand no particular differences were found between the estimators, although the nearest neighbour estimator did have the highest RMSE. Hardly any differences were found between simple kernel and local linear matching. However, the local linear matching estimator did have the highest RMSE in all of the pairwise comparisons. Finally, there was literally no differences between the kernels once the different optimal bandwidths were found, and more importantly it did not have any influence on the results of impact estimates. Therefore we chose to proceed with the biweight kernel in all of the analyses.

With respect to the common support region we followed Heckman et al. (1998) and used a 2% trimming rule. In calculating the second moments we bootstrap and re-estimate our probits for each of the single bootstraps. We do not reconsider the selection of optimal bandwidths between the bootstraps but fix this to the one found in the initial run due to computational costs. However, we conjecture this to be of less than a second order issue.

Concerning the balancing score property of our estimated propensities we investigated this informally and focused merely on the first two moments as opposed to investigating the full distribution of covariates (see also Dehejia and Wahba (1999)). The standardised differences are calculated as (see Rosenbaum and Rubin (1985))

$$
100 * \frac{(\overline{X}_1 - \overline{X}_{0M})}{\sqrt{(S_1^2 + S_{0M}^2)/2}}.
$$

All in all we conclude that the kernel based strategy produces a group of matched non-participants with characteristics near identical to that of the participants. This is achieved with just a simple linear index of the covariates ignoring any cross or higher order terms.

Appendix B

Table A1
Sensitivity analysis: Discount rates and MCF[a]

Discount rate	Marginal cost of funds	Private job training	Public job training	Classroom training	Residual programmes
0.03	0.3	239,456	44,857	−57,362	−139,690
0.06	0.3	211,697	42,024	−61,872	−120,554
0.03	0.75	314,153	94,408	−81,834	−107,284
0.06	0.75	281,613	88,879	−85,920	−90,512
0.03	1.2	388,850	143,958	−106,306	−74,878
0.06	1.2	351,529	135,734	−109,968	−60,470

[a]The stated NPVs are the sum of annual values from 1995–2005. All values are stated in 1000 DKK deflated to 2005 using the GDP deflator. See Table 6 and the text for more details.

Table A2
Sensitivity analysis: Subpopulations[a]

	Private job training	Public job training	Classroom training	Residual programmes
Men	259,314	114,954	−117,947	−101,176
Women	329,769	84,715	−89,965	−41,553
Less than high school	259,664	86,939	−98,449	−75,055
High school		232,058	27,576	−103,394
Vocational education	245,659	35,119	−87,166	−115,902

[a]The stated NPVs are the sum of annual values from 1995–2005. All values are stated in 1000 DKK deflated to 2005 using the GDP deflator. See Table 6 and the text for more details.

Table A3
Sensitivity analysis: Alternative estimation strategy[19]

	Private job training		Public job training		Classroom training		Residual programmes	
Window	1/2 year	1 year	1/2 year	1 year	1/2 year	1 year	1/2 year	1 year
Earnings effect	279.8	241.3	88.1	94.8	79.2	8.0	−300.5	−301.0
+ Transfers	124,0	108.9	70.2	73.1	19.1	−8.7	24.3	19.7
− Unit costs	−6.6	0.0	−3.5	6.0	71.3	102.4	−6.0	−2.7
− Subsidy	28,3	32.6	73.9	81.7	1.1	3.1	−5.0	−1.8
Net benefit	382.1	317.7	87.8	80.1	25.9	−106.2	−265.2	−276.8
# obs.	297	457	644	1103	475	1046	350	588

[19]The stated PDVs are the sum of annual values from 1995–2005 discounted by an annual rate of 6% as suggested by Danish Ministry of Finance (1999). The deadweight loss of taxation is assumed to be 75% of the public expense on e.g. administration of the ALMP. Unit costs cover the cost of education per full time equivalent participant and costs of administration. Negative unit costs are possible to the extent that the non-participants have a higher partition rate in classroom training. All values are stated in 1000 DKK deflated to 2005 using the GDP deflator.

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