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Do Labour Market Programmes Speed up the Return to Work?*

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Abstract

We evaluate the impact of Labour Market Programmes on unemployment durations in Norway, by means of a distribution-free mixed proportional competing risks hazard rate model. We find that programme participation, once completed, improves employment prospects, but that there is often an opportunity cost in the form of a lock-in effect during participation. The average net effect of programme participation on the length of the job search period is found to be around zero. For participants with poor employment prospects, the favourable post-programme effects outweigh the negative lock-in effects.

Keywords: Unemployment duration, labour market programmes, treatment effects

JEL Classification: C41, J24, J64

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I. Introduction

In many countries, unemployed job seekers are likely to participate in Labour Market Programmes (LMP) during parts of their unemployment spells. One of the major goals of LMP is to speed up the process of getting the job seekers into ordinary jobs. A straightforward way of evaluating the extent to which LMP contribute to this aim is to evaluate their direct effects on the transition rate from unemployment to employment. In many programmes, participants spend time and effort to accumulate skills that may be useful during the subsequent job search period. During the participation period, however, this learning activity leaves less time for active job search, and some participants may even prefer to complete the learning activity rather than accepting a second-class or temporary job. We therefore expect LMP to have widely different effects during and after participation. An informative evaluation strategy has to take both these 'on-programme' and 'post-programme' effects into account and assess their net impact on overall unemployment duration (including the participation period). Surveys of labour market policy evaluations, Heckman *et al.* (1999) and Kluve and Schmidt (2002), show that the literature focuses almost entirely on post-programme outcomes, implicitly ignoring the potential opportunity cost that accrues during the participation period. Many programme evaluations (Card and Sullivan, 1988; Gritz, 1993; Bonnal *et al.*, 1997) have been restricted to the possible impacts of programme participation on subsequent labour market states or labour market transition rates. Even in the evaluation literature that explicitly focuses on the timing of programme participation within ongoing unemployment spells (Lubyova and Van Ours, 1999; Van Ours, 2001; Richardson and Van den Berg, 2001; Lalive *et al.*, 2002), it is not standard practice to distinguish clearly between on-programme and post-programme effects. Instead, it is either assumed that the participation period does not belong to the unemployment spell at all, or that a common effect is assumed for the participation and the post-participation periods. Another simplification is that the effect only depends on the current participation status through its correlation with the time that has elapsed since the moment of entry into LMP.

In the present paper, we estimate the effects of LMP on individual transition rates from unemployment to employment within a less restrictive model, explicitly distinguishing between on-programme and post-programme effects. There exists no simple mapping of on-programme and post-programme effects into an overall 'unemployment duration effect', since the impact of LMP on unemployment duration will depend on the level of the initial hazard rate, as well as on the timing and the duration of the programme activity. This problem is clearly confounded by heterogeneity among job seekers. Even if the on-programme and post-programme hazard rate effects happened to be the same for all individuals, the resulting impact on unemployment duration would vary according to individual employment prospects and the timing/duration of LMP. However, given a particular sample of actual participants, it is possible to transform on-programme and post-programme effects into an expected duration effect by means of model simulation.

The fundamental problem facing any programme evaluator is that of unobserved heterogeneity (Heckman *et al.*, 1999). Except within carefully designed social experiments, programme participation is not a random event. The propensity to participate is affected by the individuals themselves (self-selection), as well as by the caseworkers' assessment and priorities (administrative selection). Although a lot of individual heterogeneity can be sorted out with the aid of observed explanatory variables (age, education, nationality, work-experience etc.), the eventual difference in labour market performance between participants and (observationally equal) non-participants may reflect systematic differences in unobserved characteristics rather than a causal treatment effect. The problem arises from the possible dependence between unobserved characteristics that affect the hazard rate into employment and the hazard rate into programme participation, respectively. In the present paper, we address the selection problem by means of identifying the effects of unobserved heterogeneity on each hazard rate, and then control for these effects in essentially the same way as we control for observed explanatory variables. This is done within the framework of a 'timing of events approach' (Abbring and Van den Berg, 2003), which basically aims at exploiting the information embedded in the timing of treatments within spells. Our modelling tool is a combined competing risks and single risk Mixed Proportional Hazard (MPH) rate model.

Although treatment effects are non-parametrically identified within the MPH model (Abbring and Van den Berg, 2003), we have of course no guarantee that the true effects are recovered in actual applications. In practice, it is well known that deviations from proportionality and/or unjustified parametric assumptions on the structure of the model or on the distribution of unobserved heterogeneity may inflict unpredictable bias on the estimated treatment parameters. In the present paper, we attack this lack of robustness in two ways. The first is to collect data that provide ample variation in variables deemed to contribute to non-parametric identification. We argue that time-varying explanatory variables may be particularly useful for this purpose. As pointed out in a similar context by Eberwein *et al.* (1997, p. 663), time-varying variables naturally provide an exclusion restriction in the sense that past values of these variables affect the current transition probabilities only through the selection process. For example, conditioned on the authorities' overall capacity to provide treatments, previous time variation in the treatment capacity does not have any causal effect on the current probability of being selected for treatment. However, it has clearly affected the selection of persons currently at risk of making that transition. As a result, mixed hazard rate models may be non-parametrically identified even in the absence of the proportionality assumption (McCall, 1994; Brinch, 2000). The second part of our strategy towards robust estimation of causal effects is that we actually locate the Non-Parametric Maximum Likelihood Estimator (NPMLE) (Heckman and Singer, 1984); i.e., we neither impose prior restrictions on the pattern of duration dependence nor on the number of mass-points in the simultaneous distribution of unobserved heterogeneity. We are not aware of prior applications within the programme evaluation literature in which this has been done.

Our analysis takes advantage of a Norwegian register-based dataset containing more than 1.4 million unemployment spells (experienced by 750,000 different individuals), out of which around 280,000 spells involved participation in a labour market programme. This data-source allows the econometric models to be developed and estimated in a virtually non-parametric fashion, thereby minimising the risk of unjustified functional form restrictions driving the results. We find that there are substantive impacts of labour market programmes. Once completed, LMP raises the job hazards for the large majority of participants. However, due to the lower job hazard that some participants experience during the participation period, the net effect on unemployment duration is not always favourable. There is a large individual as well as cyclical variation in LMP effects.

The next section gives a brief description of the data and the use of LMP in Norway. Section III presents the econometric model and discusses the identification issues. Section IV presents the results in terms of estimated causal parameters and statistical significance, as well as by means of model simulations, providing evidence on substantive impacts of labour market programmes. Section V provides a discussion of the results and policy implications.

II. The Data and the role of LMP in Norway

The data that we use comprise all fresh insured (UI) unemployment spells recorded in Norway during the period from March 1989 to June 2002.[1] Unlike the other Nordic countries, unemployment insurance in Norway is compulsory, with eligibility determined by preceding work experience. We focus on benefit claimants because they have strong pecuniary incentives to keep on registering until they get a job. No serious selection bias arises from this restriction, although our results may not be representative for programmes targeted at persons without unemployment insurance (youth programmes).

The data have a point-in-time structure, such that unemployment status is updated at the end of each calendar month. This implies that we know the entry and the exit months for each spell, conditioned on the spell being active at the end of at least one month. The first potential exit month is the month after the month of entry, since spells which start and end within the same month are not recorded (around 5-10 % of the spells are lost for this reason). We also know the calendar months in which persons enter into or move out of labour market programmes. The treatment status variable is updated accordingly in the month following just after each transition. We assume that a job is obtained during a month *t* if an insured person fails to register as unemployed at the end of this month, and do not return to the register in the subsequent month, or if some kind of ordinary work is recorded directly in the unemploy-

[1] 'Fresh' indicates that the person has not registered as unemployed for at least two months.

ment register files.[2] The latter implies that any kind of ordinary paid work is recorded as a job transition, even though it may be limited in terms of work-hours or duration, and even though the person may still be searching for a better job and claim (partial) unemployment benefits. This is a reasonable job transition concept to use in a programme evaluation context, especially if the ‘first employer contact’ plays a pivotal role in the process of regaining a foothold in the labour market after a period of unemployment. If an insured spell is terminated by another unemployment status (such as loss of benefits or a reclassification into disability or rehabilitation), the spell is censored.

The stated aims of labour market programmes in Norway are to enhance the participants’ prospects for taking up ordinary paid work, to improve their qualifications, and to dampen the negative consequences of being out of work in terms of discouragement and loss of self-esteem. The programmes are administered by local public employment offices, and can basically be divided into four main groups: i) Labour market training, typically in classrooms, which provide occupational skills viewed to meet the needs for qualified labour among potential local employers; ii) Temporary public employment, which amounts to carrying out some presumably useful tasks in the local community, iii) Employment subsidy, which is a wage subsidy (for a limited period of time) paid out to private employers who are willing to try out persons that are selected for this kind of treatment (with no obligation in terms of offering a permanent job); and iv) Work practice schemes, which is a job placement programme in the public and private sector aimed at providing the job seekers with basic job qualifi-

[2] If the person returns to the register in the subsequent month, the spell continues. In that case, the month of absence is censored and the process time ‘clock’ is stopped accordingly.

cations.[3] Training and employment subsidies are the largest programmes, particularly among the prime aged; see Table 1. Work practice is typically offered to youths and immigrants with little work-experience. There have been substantial changes in the composition of programme types over time (not shown in the table). In particular, the temporary public employment programme was almost terminated towards the end of the 1990's.

While on training, participants maintain their unemployment benefits or receive a training allowance. While in employment programme or work practice, participants typically receive an income support or a wage. With a possible exception for employment subsidies, this wage is typically substantially lower than the normal rate. All LMP participants are required to continue active job search during their participation period, and accept suitable job offers.

The duration by which UI benefits can be maintained without some form of activation is limited in Norway. Until 1997, there was a formal limitation of 80 weeks, followed by a 13 week quarantine period, after which a new 80 (+13) week period began. In practice, generous exemption practices ensured that quarantines were rarely imposed, and hence that the true duration constraint was 186 weeks. The 80-week rule may nevertheless have been of importance because participation in LMP at this point was often required in order to escape the benefit quarantine. For spells starting after 1997 there has been a UI benefit limitation of 156 weeks (or 78 weeks for persons with little previous work-practice). But again, the length of the income support period can be extended through participation in LMP.

[3] A more thorough description of the ALMP structure in Norway is provided by Torp (1995).

In the econometric analysis, we estimate separate models for eight different demographic groups, distinguished by gender, age and immigrant status. The main reason for this is that these groups have been subject to different programme structures and separate models allow for different selection and causal mechanisms across groups. The administrative registers provide information about standard individual variables, such as gender, age, country of birth, residential county, marital status, children, and educational attainment. Table 1 gives a summary of the micro data used for analysis. In total, the data contain 749,596 individuals. During the 14 years long observation period, these individuals experienced 1,422,280 unemployment spells containing 8,013,990 monthly unemployment observations. Almost half of the individuals contributed with more than one spell. Programme participation occurred in around 20 % of the unemployment spells. The average length of a spell, including time spent on labour market programmes, was 5.6 months.

Table 1 around here

We add macro information in the form of a quarterly national business cycle indicator provided by Statistics Norway. This indicator measures the percentage deviation of actual GDP from its trend, and its development during the estimation period is depicted in Figure 1.[4] It can be seen that Norway experienced a deep recession in the first part of the 1990's. From 1993 to the autumn of 1998, there was a strong recovery, after which a new downturn began.

Figure 1 around here

[4] See Johansen and Eika (2000) for a description of the methodology. The measure correlates well with a labour market tightness indicator based on the calendar time variation in employment hazard rates, Gaure and Røed (2003).

III. Econometric Approach

Programme evaluations typically define a baseline period where eligible potential participants are split into a treatment and a non-treatment group by some assignment procedure. Treatment effects are commonly defined in terms of earnings gain or increased probability of labour market success during a post-programme period (or status at a given date). Our empirical model reflects that unemployed persons frequently move into and out of labour market programmes, and addresses the effects of ongoing as well as of elapsed participation. We do not distinguish between different types of programmes; rather we see the matching of particular unemployed persons to particular programme activities as part of the active labour market policy that we evaluate. Hence, the causal effects that we identify are relevant for the structure of programmes and the associated matching procedures that prevailed during the data-generating period. An alternative modelling strategy would have been to estimate treatment effects of different types of programmes separately, allowing the selection mechanism to differ between programme types, but probably at the cost of more restrictive assumptions regarding unobserved heterogeneity. Essential assumptions underlying the timing of events approach are that the unobserved covariates have the same proportional effects on the hazard rates throughout a spell, and that the entry cohort distribution of these covariates have remained constant over the estimation period. Given the relatively large changes that have occurred in the composition of programme types over time, we find these assumptions to be more adequate the more aggregated is the LMP state space. Estimation of separate treatment effects would also be complicated by the fact that many spells involve participation in more than one type of programme.

We follow individuals from the month they register as full time unemployed. From this state of open unemployment the individual can make two possible transitions; to employment and to programme participation. Programme participants are considered to be at risk for employment both during the programme and after having returned to 'open unemployment'. The programme entry hazard cannot be assumed statistically independent of the employment hazard, since unobserved characteristics that affect one of these hazards almost certainly affect the other as well. We assume, however, that the duration of a labour market programme is exogenous, except (of course) when a job is obtained during the programme period. Hence, persons are switching between a standard competing risks model (while openly unemployed) and a single risk model (while participating in a programme).[5]

Let $\varphi_k(t, d, x_{it}, z_{it}, v_{ik})$ denote the monthly integrated hazard rate (i.e. integrated over the observation intervals of calendar months) governing the transition to state k=e,p (employment, programme participation) during calendar month t and spell duration month d in a spell i, given the vector of observed explanatory variables x_{it} and the unobserved scalar v_{ik}, and given the treatment status z_{it}. The treatment status has two dimensions as captured by the indicator variables $z_{it} = (z_{i1t}, z_{i2t})$. The variable z_{i1t} is equal to 1 during programme participation (and 0 otherwise), while z_{i2t} is equal to 1 after a treatment is completed (and 0 otherwise). Note that previous participation is assumed to have no effect while a person is enrolled again, (i.e. $z_{it} \neq (1,1)$).

[5] Our modelling strategy with respect to on-programme and post-programme effects is similar to a 'sensitivity analysis' exercise provided by Van den Berg et al (2004) in a different setting (regarding the effects of punitive sanctions on the transition rate from welfare to work). Bolvig et al (2003) also focus on what they label 'during'- and 'after'-effects, studying how active social policies affect transitions from welfare to employment.

The underlying hazard rates are proportional in the effects of calendar time, spell duration, observed heterogeneity, unobserved heterogeneity and treatment. This restriction ensures that the model parameters have convenient interpretations and that the number of unknown parameters is kept at a manageable level. It is also assumed that the calendar time and spell duration effects are constant within each month. The integrated monthly hazard rates φ_k can then - without further loss of generality - be written as

$$\varphi_k(t, d, x_{it}, z_{it}, v_{ik}) = \exp\left(x_{it}'\beta_k + \sigma_{kt} + \lambda_{kd} + \alpha_{iktz} + v_{ik}\right),\ k = e, p\ , \tag{1}$$

where σ_{kt} and λ_{kd} are the month-specific calendar time and duration parameters, respectively, and α_{iktz} is the treatment effect corresponding to treatment status z_{it} (treatment effects are explained in more detail below). Note that $\varphi_p(t, d, x_{it}, z_{it} = (1,.), v_{ik})$ is not defined, as agents cannot by logic transit to a state they already occupy. The vector of explanatory variables, x_{it}, contains sets of indicator variables that measure age (one dummy for each year), educational attainment (one dummy for each of five educational attainment categories), county of residence (one dummy for each of the 19 counties in Norway), marital status and children (the dummy variables describe martial status and responsibility for children, but the precise specification vary somewhat between the different demographic groups). There is also a set of seasonal dummy variables indicating calendar month of entry (12 dummy variables) and a scalar variable indicating the business cycle situation in Norway at the moment of entry (see section II). These latter variables are intended to capture systematic seasonal or business cycle patterns in the composition of the inflow cohorts.

In practice, we have to impose restrictions on the way treatment effects α_{iktz} vary across individuals and over time. The treatment effects are assumed to be the same for observationally identical individuals. We allow for heterogeneous treatment effects, assuming that interactions between effects and observed covariates have a simple linear structure. The period-specific treatment effects are modelled as

$$\begin{aligned}\alpha_{iktz} = {} & (\alpha_{k11} + \alpha_{k12}a_{it} + \alpha_{k13}e_i + \alpha_{k14}c_t)z_{i1t} \\ & +(\alpha_{k21} + \alpha_{k22}a_{it} + \alpha_{k23}e_i + \alpha_{k24}c_t + \alpha_{k25}s_{it} + \alpha_{k26}r_{it})z_{i2t}, \\ k = {} & e, p\end{aligned} \tag{2}$$

Participation affects the employment hazard from the start of the treatment period and onwards, but (2) distinguishes between on-programme effects (where $z_{i1t} = 1$) and post-programme effects (where $z_{i2t} = 1$). The variation in treatment effects across individuals and time is explained by age (a_{it}), by years of education (e_i), by the current state of the business cycle (c_t), by the duration of the (completed) programme (r_{it}), and by the time that has elapsed since the (last) programme was completed (s_{it}). Although we focus on employment effects, equation (2) also includes treatment effects in the programme participation hazard.

Each unemployment spell contributes to the analysis with a number of observations equal to the number of months at risk of making a transition of some sort. Each monthly observation is described in terms of calendar time, spell duration, the value of explanatory variables and an *outcome*. Let K_{it} be the set of feasible transition states for spell *i* at time *t* and let y_{itk} be an outcome indicator variable which is equal to 1 if the corresponding observation month ended in a transition to state *k*, and zero otherwise. Furthermore, let N_i be the set of monthly observations observed for spell *i*. The contribution to the likelihood function formed by a particular spell, conditional on the vector of unobserved variables $v_i = (v_{ie}, v_{ip})$ can then be formulated as

$$L_i(v_i)=\prod_{t\in N_i}\left[\begin{array}{l}\prod_{k\in K_{it}}\left[\left[\left(1-\exp\left(-\sum_{k\in K_{it}}\varphi_k(t,d,x_{it},z_{it},v_{ik})\right)\right)\frac{\varphi_k(t,d,x_{it},z_{it},v_{ik})}{\sum_{k\in K_{it}}\varphi_k(t,d,x_{it},z_{it},v_{ik})}\right]^{y_{itk}}\right]\\ \times\left[\exp\left(-\sum_{k\in K_{it}}\varphi_k(t,d,x_{it},z_{it},v_{ik})\right)\right]^{\left(1-\sum_{k\in K_{it}}y_{itk}\right)}\end{array}\right] \quad (3)$$

where $K_{it}=\{e,p\}$ when $z_{i1t}=0$ and $K_{it}=\{e\}$ when $z_{i1t}=1$.

Non-parametric identification of treatment effects in this type of duration model is proved by Abbring and Van den Berg (2003). The potential for actually uncovering these identified effects in any given application depends, of course, on the information in the data. We believe that the data used in the present application constitute an unprecedented opportunity for robust identification. The reason for this is that the data cover all entries and exits into unemployment in Norway during a 14-year period characterised by large changes in employment prospects as well as in the probability of being offered LMP. The exogenous variation in hazard rates is represented in the model by calendar time dummy variables (with their associated parameters σ_{kt}), representing business and seasonal cycles (see Figure 1) and changes in political and administrative priorities regarding the volume of LMP. Time varying covariates enhance the scope for identifying the influences of unobserved heterogeneity, since their past values can only affect current hazard rates through the selection process.

We use *spells*, rather than *individuals* as the basic unit for allocation of the two unobserved covariates. Apparently, we then ignore potentially valuable panel information embedded in the data, namely that some of the spells are indeed generated by the same persons. The scope for identification of the spell duration patterns and treatment effects would obviously improve if we were ready to assume that the unobserved characteristics are fixed at the individual level across different unemployment spells; see, e.g., Bonnal *et al.* (1997) for an application based on this idea. Spell-invariant heterogeneity provides a sort of fixed-effect-type foundation for identification of causal effects (Abbring and Van den Berg, 2003). However, in the present model, we find it inappropriate to assume that unobserved individual characteristics are constant across spells. The main reason is the existence of non-modelled causal linkages between different spells. In particular, there are probably strong across-spell linkages in the transition pattern into labour market programmes, where persons with extensive unemployment experience have high programme participation propensities. In addition, previous unemployment experience is likely to affect the employment hazard in basically the same way as there is duration dependence within spells.[6] A second reason for not relying on repeat spells for identification purposes is that it entails some rather awkward selection problems. Within a given observation window, the probability of experiencing more than one unemployment spell is higher the earlier the first spell occurred and the shorter it was, ceteris paribus. And persons who experience a repeated spell are not likely to be representative for the population of unemployment entrants at large.[7]

A possible restriction of fixed unobserved characteristics at the individual level can be 'informally' tested, since the model is identified without it. Although we have no Hausman-test for our non-parametric maximum likelihood estimator, we

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[6] We could of course have modelled and estimated the causal linkages between consecutive spells. However, it is not obvious how such a linkage should be modelled, and how the associated initial conditions should be set up. Moreover, the information required to identify repeat spells improves over time, since we have no information about unemployment spells prior to 1989. In order to avoid possible biases related to asymmetric information regarding the various entry cohorts, we do not pursue this idea in the present paper. This is, however, subject to ongoing research at the Frisch Centre.

[7] Similar selection problems arise if one only includes the first spell for each person in the analysis. The reason for this is that time window available for identifying previous spells is larger the later a spell starts.

would clearly view large changes in parameter estimates resulting from imposing the restriction as indicative evidence against it. When we estimate the model (for all the demographic groups) with the additional assumption of fixed individual heterogeneity, treatment effects are significantly, but not dramatically altered (we return to this point in the results section below). Together with large and significant changes in other parameter estimates, particularly those related to duration dependence, these findings support our view that the restriction is invalid.

We approximate the heterogeneity distribution in a non-parametric fashion with the aid of a discrete distribution (Lindsay, 1983; Heckman and Singer, 1984; Huh and Sickles, 1994). Let *W* be the (a priori unknown) number of support points in this distribution and let $\{v_l, p_l\}$, $l = 1, 2, ...W$, be the associated heterogeneity vectors and probabilities. In terms of observed variables, the likelihood function is then given as

$$L = \prod_{i=1}^{N} \underset{v_i}{E}[L_i(v_i)] = \prod_{i=1}^{N} \sum_{l=1}^{W} p_l L_i(v_l), \quad \sum_{l=1}^{W} p_l = 1. \tag{4}$$

Our estimation procedure is to maximise (4) with respect to all the model and heterogeneity parameters repeatedly for alternative values of *W*. We start out with *W=1*, and then expand the model with new support points until we do not achieve an increase in the likelihood function value. The likelihood function (4) is not globally concave. Hence, although we do estimate the models repeatedly (with differing starting values) and check for possible likelihood improvements through local grid searches, we have found no way to determine when to stop searching for a better model that completely eliminates the influences of subjective judgement. It should also be noted that the exact location of the mass-points and their associated probabilities are not directly interpretable. The reason for this is that the discrete distribution only serves as an approximation to an unknown, and possibly continues, distribution. There may also exist other (and equally good) approximations.

Maximisation of (4) is a huge computational task, which explains why most applications in this area are based on a pre-determined number of mass-points, typically two or three. In order to solve the computational problems associated with full-scale estimation, we use an optimisation programme tailored for data characterised by a huge number of indicator variables.[8] We also introduce additional mass-points in the heterogeneity distribution in a way that deviates from common practice. Rather than allocating mass-point locations to each destination separately, and thereafter estimate the probability of all possible combinations of these locations, we add mass-point locations in the form of individual vectors (pairs). Although these two procedures in principle may end up at the same maximum, they have very different numerical properties. The reason for this is that the introduction of destination-specific mass-point locations implies that the number of potential mass-points are added in fairly large steps (1, 2^2=4, 3^2=9,…,) rather than one by one. In principle, this should perhaps be irrelevant, since it is always possible to attach the probability zero to some of the additional points. In practice, it turns out to be important. What usually happens with the standard procedure is that many of the estimated probabilities are indeed very close to zero (functional form restrictions normally prevent them from being equal to zero), and this quickly leads into numerical difficulties. By contrast, our method ensures that mass-points are introduced one by one, and the problem of zero probabilities is typically not encountered until the likelihood no longer can be improved by adding additional points. Our results indicate that this rarely happens with two or three support

[8] The elements of this program are described in Gaure and Røed (2003).

points only, and that important parameter estimates change substantially as more support points are included. In some cases, however, there are indications that the likelihood criterion for model selection results in a kind of over-parameterisation (the symptom being ever-increasing positive duration dependence as more support points are included). This problem typically arises in small samples (Baker and Melino, 2000; Gaure *et al.*, 2005). We identified this problem in two of the models, which indeed were the two models with the smallest samples (the two immigrant groups). In these cases, we adhered to the recommendations provided by Baker and Melino (2000), and applied the Hannan-Quinn information criterion to select the preferred model.

The econometric model used in this paper has been subject to a thorough Monte Carlo investigation, based on artificial data (Gaure *et al.*, 2005). The main conclusion from this analysis is that the model, and the associated optimisation routine, robustly delivers unbiased estimates of treatment effects, under widely different unobserved heterogeneity distributions. Moreover, the parameter estimates tend to be normally distributed, and the standard errors estimated using the inverse of the Hessian at the final implying step of the NPML procedure can be used to perform inference.

IV. Results

Due to the basically non-parametric estimation strategy, the models contain about 4,500 unknown parameters altogether. Some basic model properties are provided in Table 2. The number of mass-points in the distributions of unobserved heterogeneity that were required to maximise the likelihood functions varied from 5 to 11 across groups. The correlation coefficients between the two unobserved variables were negative for all groups, suggesting negative selection on unobserved characteristics to labour market programmes in our data.[9] The negative correlation is stronger for young than for old job seekers.

Table 2 around here

Given the large number of estimated parameters, it is impracticable to present complete results in this paper. Instead, we focus on the estimates of main interest. We first present the estimated transition rate profiles for a representative entrant into open unemployment in each of the eight groups. We then proceed by looking at the estimates of parameters that describe the on-programme and post-programme effects on the employment hazard. Finally, we simulate the overall impact of labour market programmes on the distribution of unemployment spells and report summary statistics.

Transition rate profiles from open unemployment

Figures 2 and 3 describe the estimated transition rate pattern to employment and programme participation, respectively, for the representative (group-specific) entrant into open unemployment.[10] Since we control for unobserved heterogeneity, we are also able to identify the degree of structural duration dependence embedded in the hazard rates.

Figure 2 around here

Figure 3 around here

[9] This is a very robust result across groups and models, and it does not hinge on the precise number of support points in the heterogeneity distribution, as long as this number is not too low. An important point to note, however, is that models with few support points in some cases produced exactly the opposite result. For example, with only two support points, positive correlation coefficients were estimated for all groups. Thus, a modelling strategy pre-specifying a small number of support points is likely to fail.

[10] These are obtained by setting the transition rate in the first duration month equal to the observed average (since no selection has taken place at this point), and then use the non-parametrically estimated spell duration baselines (with 95 % confidence intervals) to predict the developments in transition rates over spell duration.

The typical pattern revealed by Figure 2 is negative structural duration dependence in the employment hazard during the first months of the unemployment spells. Possible explanations for this phenomenon are discouragement, psychological adaptation and stigmatisation. But, as the point of potential benefit exhaustion approaches around the 18th duration month, the hazard rates rise substantially. This result is in accordance with predictions from standard search theory as well as previous findings reported by, e.g., Meyer (1990). An important point to note in our case, however, is that for most unemployed persons, exhausted benefits can be replaced by income support associated with labour market programme participation (see Section II). It therefore seems warranted to interpret the rise in the employment hazard rates in the months just prior to (passive) benefit exhaustion as a sort of 'activation pressure effect', although we cannot without additional information or additional assumptions separate that effect appropriately from other sources of structural duration dependence. Our results at this point confirm previous findings reported by Black *et al.* (2003), Røed *et al.* (2002), and Geerdsen (2006), indicating that compulsory programme participation (in exchange for benefits) can counteract some of the moral hazard problems associated with unemployment insurance.

Post-programme and on-programme effects

The effects of ongoing and completed programme participation on the employment hazard vary across individuals according to their age and education, location in the business cycle, time passed since participation and duration of the programme, see equation (2). In order to give a flavour of the main results for each group, we first report, in Table 3, the average predicted effect for actual participants, and the dispersion of these effects measured by the standard deviation. The post-programme effects are predicted for the first month after completion of the programme. Averages are then taken over all spells involving on-programme and post-programme effects, respectively.

A key result is that the average immediate post-programme effect is positive for all groups. The average predicted effect the first month after the programme is around 0.4-0.5, except for young and old men and female immigrants. As we explain below, typical standard errors for these predictions lie around 0.02-0.06, hence the effects are highly significant, from a statistical point of view. The predicted proportional change in the employment hazard rate resulting from a given predicted effect is equal to exp(predicted effect). For example, for prime aged men, LMP participation raises on average the employment hazard just after programme completion by a factor of exp(0.403)=1.496, i.e., by 50 %, compared to a spell without LMP. There is substantial variation in the effects, but the average exceeds two times the standard deviation for all groups, except for immigrants and young men. This suggests that the predicted post-programme effects are favourable for most participants.[11]

Table 3 around here

Table 3 also shows that participation is costly, as it typically lowers the exit out of unemployment during the programme period. This on-programme effect varies between as well as within groups. The job search effectiveness seems to be most severely affected for women and young men. For prime aged women, the hazard drops by 24 % when they enter a programme.

Table 4 reports the estimated programme effect parameters (see equation (2)). The constant terms reflect the estimated effects on the employment hazard rates for a reference vector of explanatory variables within each demographic group. The associ-

[11] We return to the issue of statistical significance when discussing the parameter estimates in Table 4.

ated standard errors measure the degree of statistical uncertainty in the prediction of individual effects. For all groups, except immigrant women, the standard errors lie somewhere between 0.02 and 0.06. The coefficients attached to the interaction terms report the estimated changes in the effects that occur when certain observed characteristics are modified (the ranges of variation in these characteristics are reported at the bottom of the table).

Table 4 around here

It is clear that the favourable post-programme effects, reported in panel I, are statistically significant for the reference spells within all groups, as the constant term estimates exceed the standard errors by a factor of five or more. For example for a prime aged male belonging to the reference group, the job hazard is raised by a factor somewhere between $\exp(0.381-1.96*0.024)=1.39$ and $\exp(0.381+1.96*0.024)=1.53$ (95 % confidence interval) just after programme completion. The favourable effects are largest for low-skill workers, as a negative interaction term for education ($\hat{\alpha}_{e22}<0$) is found in all groups. The effects are largest immediately after programme completion (as $\hat{\alpha}_{e25}<0$), but it typically takes more than a year to wipe out the post-programme effects entirely. Older workers seem to gain less as the post-programme effects are systematically declining in age. The post-programme-effects are increasing in programme duration for women and young men, but the marginal effects of increasing the length of the programmes are modest. For adult males, no programme duration effect is found. It is also worth noting that the post-programme effects tend to be larger the better are the business cycle conditions.

Moving to the on-programme effects in panel II, the constant terms are significantly negative for women and young men. The opportunity costs seem to be lower the poorer are the individual employment prospects to start with; i.e., the on-programme effects are negatively related to educational attainment and less negative for Non-OECD immigrants than for natives. More surprising perhaps, the opportunity costs are also lower the better are the business cycle conditions. This probably reflects that programmes tend to be more oriented towards direct job placements (and less oriented towards training) during good times.

Panel III reveals strong positive post-programme-effects on the programme (re)entry hazard rate, except for young women. Hence, the event of having participated once substantially increases the probability of returning to a programme later during the same spell. The effects on future participation are clearly lower when job opportunities are favourable (as $\widehat{\alpha}_{p24}<0$), but do not vary systematically across age or skill groups. Naturally, the effect on the programme hazard is weaker for programmes of long duration and declines as time since completion increases.

As explained in Section III, we also estimated the model under the additional restriction that unobserved heterogeneity is constant at the individual level (i.e. across as well as within spells). This restriction led to a statistically significant drop in the level of all estimated treatment effects for all eight groups. Post-programme effects are less positive (yet still significantly above zero), and the on-programme effects became more negative. The interaction terms were only marginally affected. As is clear from our discussion in Section III, we do not interpret these finding as indicative of lack of robustness, but as evidence against the assumption of constant heterogeneity across spells in the context of our model.

Overall effects on unemployment durations

The complexity of the programme effects implies that net effect on the unemployment duration varies, between and within groups. We cannot simply add up the hazard effects I-III in Table 4 (or 3) to get an 'overall' programme effect. This total impact of on-programme and post-programme effects among benefit claimants in Norway 1989-2002 is assessed by simulating the progression of all the unemployment spells, given their actual starting dates, under two alternative assumptions about effects of labour market programmes; i) that they are equal to zero, and ii) that they are equal to the point estimates reported in Table 4. The overall effect is given by the difference in unemployment durations produced by the two simulations.

We first use the estimated models to reproduce slightly stylised versions of the actual data. The main difference between actual and simulated data is that in the simulations, we have had to replace the actual planned (or potential) programme durations with predicted programme durations (since these durations are unobserved). We have done this in the following simple way: We assume that 10-15 % (depending on group) of ongoing programmes are terminated without transition to employment each month (in a random manner), until a programme has lasted in 20 months, at which points it always ends. These assumptions give a relatively good fit to the observed pattern of programme durations and participation fractions. In addition, there are some time-varying covariates, and some sources of censoring (entry to rehabilitation programme and loss of benefits) that we have tried to reproduce in a similar fashion. Despite these differences, the model generates spell distributions that are very similar to observed data. We proceed by simulating the progression of unemployment spells under the counterfactual assumption that the on-programme and post-programme effects are zero, but that all other parameters in the model are unaffected. This is not equivalent to the counterfactual assumption that no programmes exist, since, e.g., activation pressure effects (or other mechanisms by which the mere existence of LMP affect job search behaviour) cannot be removed without further assumptions. This highlights the fact, pointed out in a similar context by Abbring and Van den Berg (2003), that our model provides evidence on the effects of realised treatment given the information structure about treatment prospects, implying that it does not allow us to contrast outcomes of treatment to outcomes in a world without treatment. Moreover, a (hypothetical) removal of all programmes would certainly have general equilibrium effects that are not accounted for by these simulations. Thus, the simulations should be interpreted as the average net effects of participation for the participants, rather than a comparison of the (realised) LMP-economy and a counterfactual economy without any LMP.

The main results from these simulations are provided in Table 5. The overall effects of the programmes are favourable for adult men and for immigrants, since average unemployment duration is reduced. For females and young male job seekers, the lock-in effects during participation outweigh the favourable effects afterwards with respect to the total spell duration. The overall effects are modest for all groups, ranging from a reduction in total unemployment exposure of 4.11 % (immigrant men) to an increase of 3.31 % (prime aged women). The corresponding changes in total unemployment exposure produced by each month of programme participation ranges from a one-week reduction (for immigrant men) to almost a one-week increase (prime aged women). The gender difference among adults reflects that the average on-programme effect is negative for women but negligible for men. A different composition of programmes for men and women may explain the gender differential, as employment subsidy is more frequent for men; see Table 2. Direct employment transitions are possibly more likely to take place from programmes that involve contact with a private employer. Since we estimate a common effect of all programmes, we cannot tell why certain individual characteristics are associated with high or low effects, as long as the composition of programmes varies across groups.

Table 5 around here

The simulations also illustrate that the net effects of programme participation cannot easily be detected from the estimates of Table 3 (or 4). Even if the parameter estimate for the post-programme effects on the employment hazard is higher than the on-programme parameter (in absolute value), the net effect on the average unemployment duration can be positive (as is the case for prime aged women).

V. Concluding remarks

In this paper, we have assessed the effects of all LMP activities on the duration of all job search periods in Norway from 1989 to 2002. Our main findings are that the labour market programmes that were offered during this period had a significant positive effect on the transition rate to employment after the programmes were completed. But, because there were also opportunity costs associated with a reduced employment transition rate during the participation period, the net effects on the unemployment duration were not always favourable. An evaluation of employment effects that merely focuses on post-programme transitions is likely to exaggerate the extent to which labour market programmes speed up transitions to ordinary work.

On average, the direct on-programme and post-programme effects do not seem to justify the administrative costs associated with the production of programme slots. The cost of producing one month of labour market training is around 5,000 NOK, which corresponds roughly to the average income of low skilled wage earners associated with a one-week job; see Raaum *et al.* (2002). Hence, with a possible exception for male immigrants, the value of the employment gains resulting from labour market programmes in Norway do not seem to exceed the direct costs. However, the direct on-programme and post-programme effects reported in this paper may not capture the full individual returns properly. First, we do not observe the amount of work forgone due to programme participation. The job transitions that are lost during participation (the opportunity costs) are likely to be different from those gained after the completion of the programme. Ongoing programme participation is likely to have a particularly strong negative effect on occasional and/or part-time work, since such activities are more easily combined with open unemployment than with fulltime programme participation. It can also be argued that programme participation is socially productive in its own right, since participants contribute positively to the output of an employer (employment subsidies, work practice) or acquire valuable skills (training). Moreover, as indicated by the duration profile of the employment hazard, there are some activity pressure effects associated with LMP, which contribute to speed up the job transition process. The mere existence of programmes seems to have a sort of ‘threat’ effect, which raises the employment transition rates even for persons who do not actually participate. To the extent that such effects exist, they are embedded in the duration parameters and thereby not included in our reported programme effects. Finally, as we focus on the time until the first ordinary job is achieved, we ignore possible impacts of LMP on future employment careers and productivity.

A welfare evaluation of LMP should also take into account the programmes’ potential redistribution effects. We have shown that programme effects are most favourable for persons with particularly poor individual employment prospects and long expected unemployment spells. Hence, to some extent, labour market programmes redistribute the unemployment burden from persons with very poor employment opportunities to persons with better prospects. To the extent that long-term unemployment is considered particularly harmful, a reduction in the longest unemployment spells represents a social welfare improvement, even when it is replaced by an equally sized increase in shorter spells.

In order to provide a comprehensive assessment of Norwegian active labour market policies, a number of other general equilibrium type effects also have to be taken in to account. These include the possibility of substitution, where participants obtain jobs at the expense of non-participants, and the possibility that the overall level of LMP affects the wage formation in the economy. The existence of LMP may also affect the flow into the state of registered unemployment, either because the prospect of becoming unemployed in the first place becomes less (or more) frightening, or because it affects the propensity to register at the Employment Office.

In practice, the policy question is not whether to scrap the programmes completely or not, but rather to change the number of programme slots, including adjustments to cyclical movements in non-employment. We find that the programme effects vary substantially over the business cycle. Our results suggest that the stance of labour market policies should be less cyclical than employment prospects are, implying a higher participation probability in good times than in bad times. There are two reasons for this: First, employment effects are more favourable in good times than in bad times for each given individual. And second, the fraction of individuals with poor individual employment prospects (and hence much to gain from LMP) increases in good times.

To sum up; the most important policy implications of our findings are the following; i) labour market programmes do seem to have beneficial net effects on the transition rate to employment for a large number of job-seekers, hence a substantial level of LMP seems warranted from a social welfare point of view; ii) the programmes should be targeted at persons with poor individual employment prospects; iii) particularly large favourable effects are found for immigrants from developing countries; iv) the overall welfare gains of LMP depends on the social welfare function, as individual effects differ across skill and unemployment duration groups; (v) LMP for better qualified job seekers should be seen more as a way of utilising the waiting-period (in terms of some productive activities) until some job offer arrives, than a tool for reducing the length of the waiting-period as such; and (vi) the programme activity level should not accommodate business cycle changes completely.

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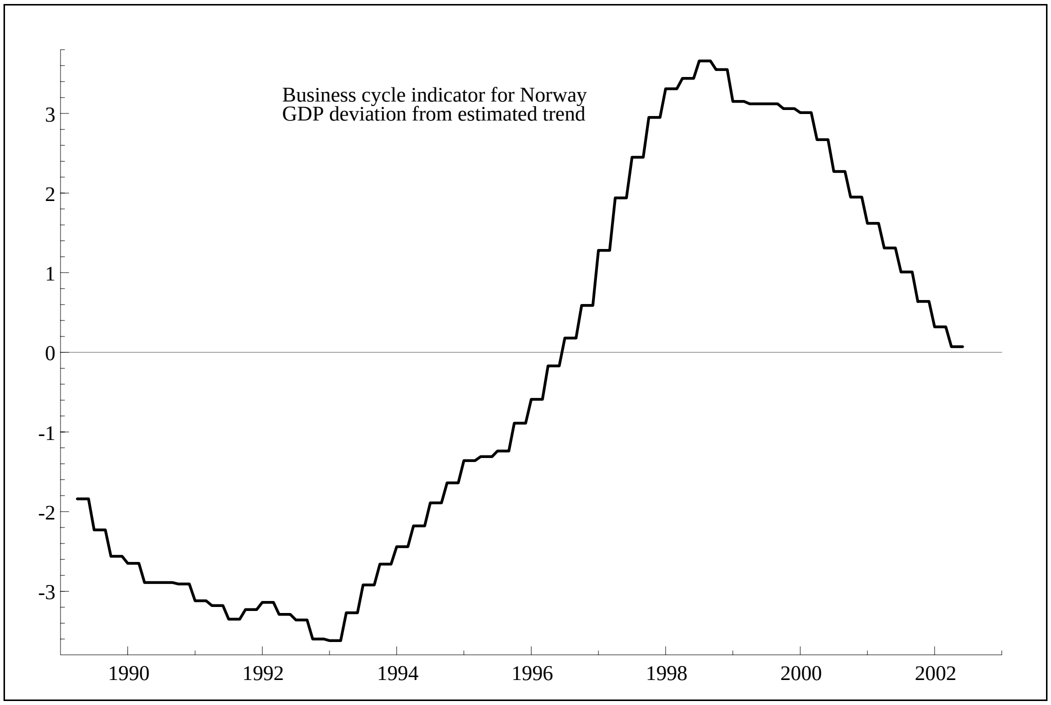

Figure 1. Business cycle developments in Norway during the estimation period, according to the GDP development.
Source: Statistics Norway

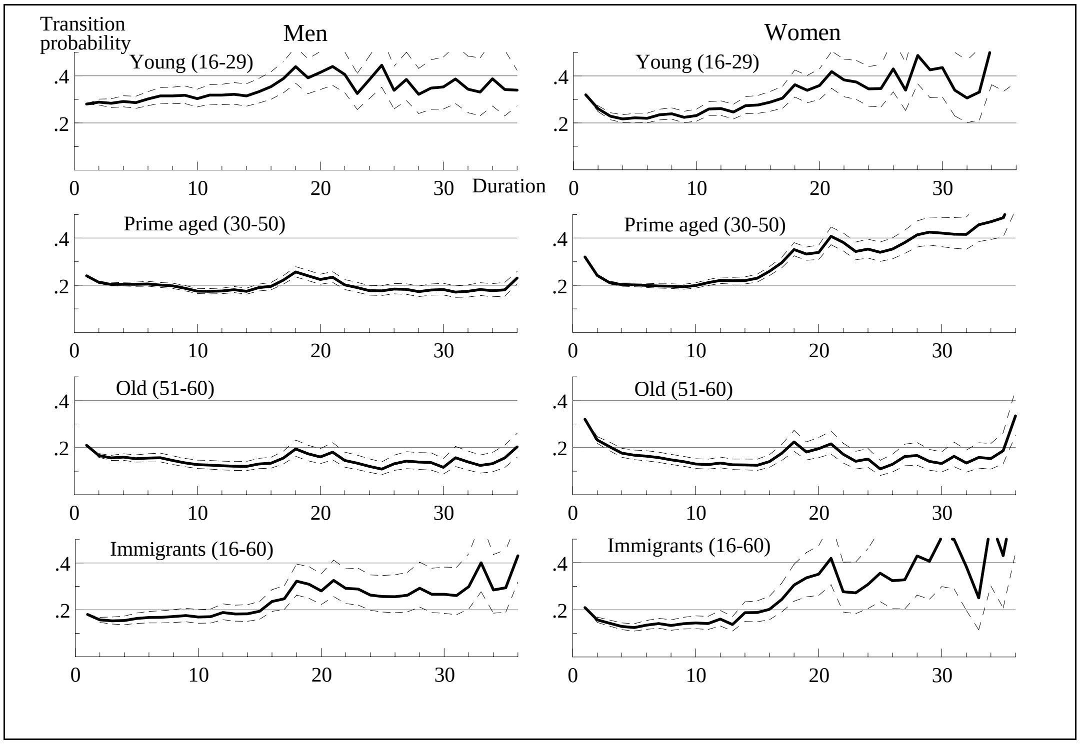

Figure 2. To employment. Estimated transition probabilities (grouped hazard rates) from open unemployment to employment for a representative entrant into open unemployment (with 95 % confidence intervals) as functions of spell duration (measured in months).

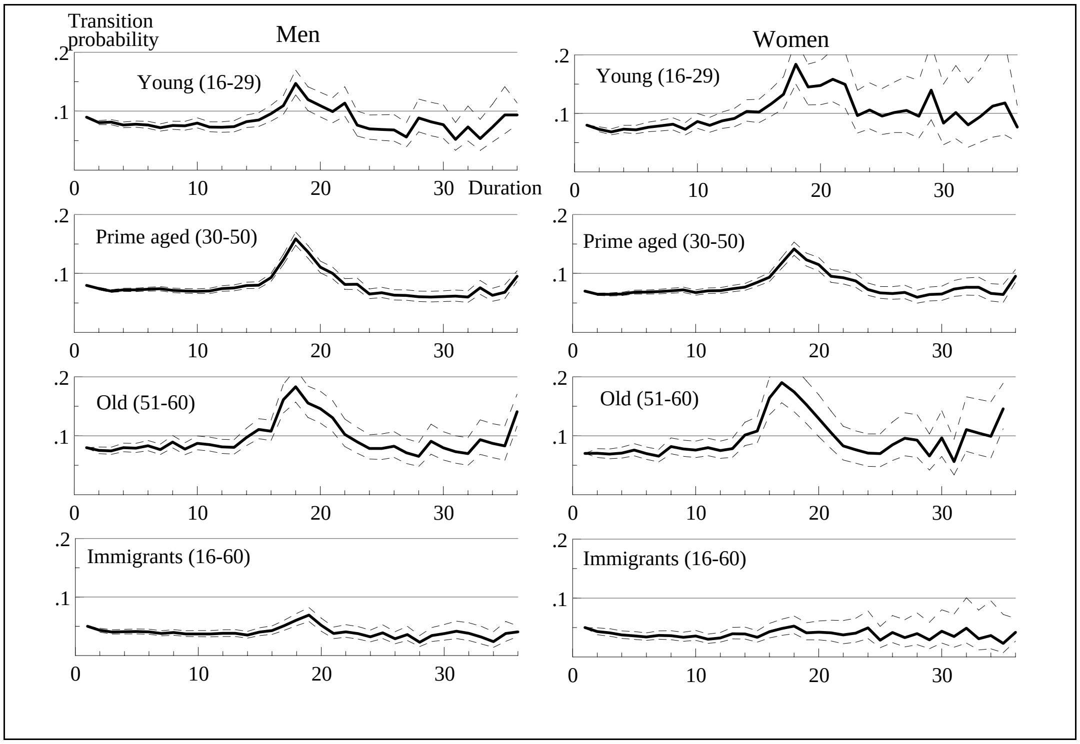

Figure 3. To programme. Estimated transition probabilities (grouped hazard rates) from open unemployment to programme participation for a representative entrant into open unemployment (with 95 % confidence intervals) as functions of spell duration (measured in months).

TABLE 1

Descriptive Statistics

	Men 16-29	*Women 16-29*	*Men 30-50*	*Women 30-50*	*Men 51-60*	*Women 51-60*	*Immigrant*[†] *Men 16-60*	*Immigrant*[†] *Women 16-60*
Number of individuals	229,425	148,223	144,021	144,285	32,213	29,177	14,724	7,528
Number of spells	423,167	271,979	285,523	282,403	58,807	55,963	30,020	14,418
Number of observations	2,110,688	1,377, 562	1,811,355	1,543,495	460,163	358,289	244,228	108,210
Fraction of spells involving programme participation	0.21	0.18	0.20	0.17	0.19	0.13	0.26	0.21
Fraction of spells involving participation in[‡]:								
Labour market training	0.42	0.43	0.48	0.54	0.39	0.48	0.62	0.68
Public employment	0.21	0.21	0.23	0.23	0.33	0.29	0.16	0.12
Employment subsidy	0.40	0.35	0.46	0.37	0.46	0.36	0.32	0.23
Work practice scheme	0.19	0.22	0.02	0.04	0.01	0.02	0.15	0.17
Average duration of completed programmes (months)	4.00	4.44	4.14	4.48	4.42	4.24	4.29	4.74
Fraction of spells censored due to:								
Transition to disability or loss of benefits	0.12	0.10	0.13	0.10	0.14	0.10	0.19	0.17
End of "observation window"	0.01	0.01	0.03	0.03	0.04	0.03	0.06	0.06
Average unemployment duration at spell completion or censoring (months)	4.99	5.06	6.34	5.47	7.82	6.40	8.14	7.51
Median unemployment duration (spells starting at least 6 months before end of "observation window")	3	3	3	3	4	3	5	5
Average transition rate to employment in first duration month	0.28	0.32	0.24	0.32	0.21	0.32	0.18	0.21
Average transition rate to programme in first duration month	0.09	0.08	0.08	0.07	0.08	0.07	0.05	0.05
Fraction of individuals with more than one spell	0.45	0.46	0.58	0.56	0.55	0.56	0.51	0.48
Average number of spells for persons with more than one spell	2.87	2.83	3.44	3.32	3.47	3.62	3.03	2.92

[†] The group of Immigrants encompasses immigrants from non-OECD-countries only. Immigrants from other countries are not included in the analysis.

[‡] The sum exceeds unity as some spells contain participation in more than one programme.

TABLE 2

Properties of the Estimated Models

	Men 16-29	*Women 16-29*	*Men 30-50*	*Women 30-50*	*Men 51-60*	*Women 51-60*	*Immigrant Men 16-60*	*Immigrant Women 16-60*
Number of unknown parameters	550	544	566	551	535	527	590	590
Number of mass-points in the heterogeneity distribution	6	9	11	6	10	8	5	5
Correlation coefficient between unobserved covariates in the two hazard rates $\left(corr\left(\exp v_{ie}, \exp v_{ip}\right)\right)$	-0.65	-0.53	-0.51	-0.49	-0.32	-0.30	-0.56	-0.47
Log-likelihood without unobserved heterogeneity	-256895.58	-212580.84	-932176.99	-837880.17	-202398.64	-164058.11	-109215.36	-47610.28
Final log-likelihood preferred model	-256523.51	-212272.99	-931160.67	-836844.24	-202152.07	-163862.24	-109108.99	-47495.22

Notes: Apart from the treatment effects that are fully described in Table 3, the following variables were included in the models: Age dummy variables (one for each year), calendar time dummy variables (one for each calendar month), spell duration dummy variables (one for each possible duration up to 35 months and for 36 months or more), county dummy variables (one for each of the 19 counties in Norway), inflow season dummy variables (one for each of the 12 calendar months of the year), a scalar variable for business cycle conditions in entry month, educational attainment dummy variables (one for each of five educational groups), marital status dummy variables (one for current marriage and one for previous marriage), children dummy variables (one dummy for children in each of the age groups 0-4, 4-6, 7-12, not all these dummy variables are relevant for all groups).

TABLE 3

Employment hazard effects for actual participants. Average and standard deviation.

	Men 16-29	*Women 16-29*	*Men 30-50*	*Women 30-50*	*Men 51-60*	*Women 51-60*	*Immigrant Men 16-60*	*Immigrant Women 16-60*
First month post-programme effect, average [standard deviation]	0.211 [0.151]	0.467 [0.120]	0.403 [0.135]	0.510 [0.140]	0.276 [0.092]	0.416 [0.178]	0.458 [0.269]	0.674 [0.336]
On-programme effect, average [standard deviation]	-0.350 [0.224]	-0.336 [0.211]	-0.027 [0.213]	-0.278 [0.253]	-0.076 [0.164]	-0.326 [0.255]	0.050 [0.331]	-0.086 [0.312]

TABLE 4
Estimated Programme Effect Parameters
(standard errors in parentheses)

	Men 16-29	*Women 16-29*	*Men 30-50*	*Women 30-50*	*Men 51-60*	*Women 51-60*	*Immigrant Men 16-60*	*Immigrant Women 16-60*
I. Effects of completed programme on employment hazard								
Constant term (effect in the first month after completion of a programme lasting four months, for a person with 12 years education with age at the group mid-point, being unemployed when GDP is at its trend level) (α_{e21})	0.204 (0.040)	0.497 (0.044)	0.381 (0.024)	0.313 (0.022)	0.277 (0.055)	0.336 (0.053)	0.444 (0.056)	0.618 (0.107)
Interaction with age (α_{e22})	-0.001 (0.007)	-0.011 (0.007)	-0.010 (0.002)	-0.022 (0.002)	-0.030 (0.008)	-0.036 (0.009)	-0.011 (0.004)	-0.034 (0.006)
Interaction with educational level (α_{e23})	-0.176 (0.022)	-0.123 (0.027)	-0.118 (0.010)	-0.095 (0.011)	-0.033 (0.020)	-0.150 (0.025)	-0.159 (0.026)	-0.139 (0.041)
Interaction with business cycle (α_{e24})	0.018 (0.011)	0.030 (0.010)	0.014 (0.004)	0.044 (0.005)	0.004 (0.010)	0.001 (0.010)	0.086 (0.013)	0.074 (0.022)
Interaction with time since completion (α_{e25})	-0.021 (0.006)	-0.014 (0.007)	-0.025 (0.002)	-0.039 (0.003)	-0.016 (0.004)	-0.028 (0.004)	-0.025 (0.006)	-0.019 (0.012)
Interaction with programme duration (α_{e26})	0.013 (0.005)	0.015 (0.005)	0.002 (0.002)	0.013 (0.003)	-0.007 (0.005)	0.009 (0.006)	-0.007 (0.007)	0.008 (0.012)
II. Effects of ongoing programme on employment hazard								
Constant term (effect for a person with 12 years education with age at the group mid-point, being unemployed when GDP is at its trend level) (α_{e11})	-0.338 (0.034)	-0.285 (0.040)	0.011 (0.022)	-0.485 (0.021)	-0.029 (0.052)	-0.376 (0.052)	0.033 (0.053)	-0.155 (0.102)
Interaction with age (α_{e12})	0.010 (0.005)	0.008 (0.005)	0.001 (0.001)	-0.009 (0.001)	0.002 (0.006)	0.034 (0.008)	-0.011 (0.003)	-0.027 (0.005)
Interaction with educational level (α_{e13})	-0.260 (0.017)	-0.237 (0.020)	-0.175 (0.008)	-0.101 (0.009)	-0.061 (0.016)	-0.176 (0.022)	-0.199 (0.022)	-0.164 (0.034)
Interaction with business cycle (α_{e14})	0.031 (0.008)	0.044 (0.008)	0.059 (0.003)	0.093 (0.004)	0.070 (0.008)	0.067 (0.010)	0.108 (0.010)	0.070 (0.017)

TABLE 4
Estimated Programme Effect Parameters
(standard errors in parentheses)

	Men 16-29	*Women 16-29*	*Men 30-50*	*Women 30-50*	*Men 51-60*	*Women 51-60*	*Immigrant Men 16-60*	*Immigrant Women 16-60*
III. Effects of completed programme on the programme hazard								
Constant term (effect in the first month after completion of a programme lasting four months, for a person with 12 years education with age at the group mid-point, being unemployed when GDP is at its trend level) (α_{p21})	0.234 (0.048)	-0.356 (0.075)	0.234 (0.028)	0.274 (0.028)	0.217 (0.064)	0.240 (0.075)	0.391 (0.064)	0.738 (0.080)
Interaction with age (α_{p22})	0.021 (0.007)	0.049 (0.009)	-0.004 (0.001)	-0.004 (0.002)	0.017 (0.009)	-0.001 (0.013)	0.006 (0.004)	-0.001 (0.006)
Interaction with educational level (α_{p23})	0.049 (0.022)	0.146 (0.032)	0.054 (0.011)	0.029 (0.012)	0.006 (0.021)	-0.050 (0.032)	-0.010 (0.023)	-0.068 (0.040)
Interaction with business cycle (α_{p24})	-0.046 (0.014)	-0.078 (0.016)	-0.051 (0.005)	-0.044 (0.006)	-0.069 (0.012)	-0.057 (0.015)	-0.012 (0.012)	-0.028 (0.019)
Interaction with time since completion (α_{p25})	-0.005 (0.005)	-0.030 (0.009)	-0.005 (0.002)	-0.010 (0.003)	-0.003 (0.004)	-0.016 (0.006)	-0.034 (0.005)	-0.071 (0.013)
Interaction with programme duration (α_{p26})	-0.003 (0.005)	-0.022 (0.007)	-0.002 (0.002)	-0.009 (0.003)	-0.010 (0.006)	-0.029 (0.009)	0.003 (0.006)	-0.014 (0.012)

Notes: The interaction terms vary as follows: The educational attainment variable varies from –2 to 2, with 0 corresponding to 12 years of education. The business cycle indicator varies from around –3.5 to +3.5, and is equal to 0 when GDP is in accordance with its estimated trend. The age variable varies from the lowest to the highest age in each group, subtracted by the group midpoint. Time since completion is equal to zero in the first month after completion. Programme duration is equal to the duration of the programme minus 4 months (which is the median duration in most groups).

TABLE 5

Simulated unemployment spells in Norway 1989.3-2002.6, given actual starting dates, based on estimated on-programme and post-programme effects and on the assumption that both these effects are equal to zero

	Men 16-29	*Women 16-29*	*Men 30-50*	*Women 30-50*	*Men 51-60*	*Women 51-60*	*Immigrant Men 16-60*	*Immigrant Women 16-60*
I. Simulated "true" model with programmes								
Total number of unemployment months	2,123,670	1,354,503	1,864,218	1,544,895	457,182	346,115	252,228	108,229
Average spell duration (months)	5.02	4.98	6.53	5.47	7.77	6.18	8.40	7.51
Number of programme participation months	517,624	308,897	336,473	291,493	73,825	42,829	52,297	20,799
II. Simulated model without programmes (i.e programme effects set to zero)								
Total number of unemployment months	2,056,786	1,358,371	1,911,766	1,495438	464,480	341,820	263,046	109,529
Average spell duration (months)	4.86	4.99	6.70	5.30	7.90	6.11	8.76	7.60
III. Impact measures (I - II)								
Change in total number of unemployment months due to programme effects	66,884	-3,868	-47,548	49,457	-7,298	4,295	-10,818	-1,300
Percentage change in total unemployment months due to programme effects	3.25	-0.28	-2.49	3.31	-1.57	1.26	-4.11	-1.19
Ratio of causal effects (change in total unemployment months produced by each month of actual programme participation)	0.13	-0.01	-0.14	0.17	-0.10	0.10	-0.21	-0.06