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23 April 2003

The Effect of Programme Participation on the Transition Rate from Unemployment to Employment

By Knut Røed and Oddbjørn Raaum*

The Ragnar Frisch Centre of Economic Research

Abstract

We use Norwegian register data from 1989 to 2002 to estimate the causal effects of programme participation on the transition rate from unemployment to employment, by means of a dependent risks hazard rate model. The separate roles of causality and unobserved heterogeneity are non-parametrically identified on the basis of variation in 'lagged' explanatory variables. Active labour market programmes tend to reduce the transition rate to ordinary work during the participation period, and increase it afterwards. The average net effect on total unemployment duration for the treated is around zero. There are favourable net effects for adults with poor employment prospects.

Keywords: Unemployment duration, labour market programmes, treatment effects

JEL Classification: C41, J24, J64

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1 Introduction

In many countries, unemployed job seekers are likely to participate in active labour market programmes (ALMP) during parts of their unemployment spells. One of the major goals of ALMP is to speed up the process of getting the job seekers into ordinary jobs. A straightforward way of evaluating the extent to which ALMP work as intended, according to this aim, is to evaluate their direct effects on the transition rate from unemployment to employment. But programme participation is likely to have different effects during a programme ('on-programme effect') and after the programme period has elapsed ('post-programme effect'). An informative evaluation strategy has to take both effects into account and assess the net impact on unemployment duration. In this paper, we identify and estimate treatment effects on individual transitions from unemployment to work associated with participation in Norwegian labour market programmes in the period from 1989 to 2002. The estimated effects are then used to assess the impact of the programmes on total unemployment during these 14 years, by means of model simulation.

The fundamental problem facing any programme evaluator is that of unobserved heterogeneity (Heckman et al, 1999). Programme participation is obviously not a random event. The propensity to participate is strongly affected by the individuals themselves (self-selection), as well as by the caseworkers' assessment and priorities (administrative selection). And even though a lot of individual heterogeneity can be sorted out with the aid of observed explanatory variables (age, education, nationality, work-experience etc.), one never really knows whether the eventual difference in labour market performance between participants and (observationally equal) non-participants reflect a causal treatment effect or a systematic difference in unobserved characteristics. The problem arises from the possible dependence between unobserved characteristics that affect the transition rate into employment and the transition rate into programme participation, respectively. This motivates the strategy of social experiments, i.e. random selection of participants who satisfy certain eligibility conditions (Dolton and O'Neill, 1996; Eberwein et al, 1997). However, random assignment typically interfere with the parts of the selection procedures that are thought to be essential for the success of the programmes (e.g. the case worker's assessment of the best type –and dosage of treatment for a particular individual in a particular situation); see Heckman and Smith (1995) for a discussion of 'randomisation bias' and other problems of social experiments. Moreover, although programme participation *offers* can be randomised, actual participation cannot. Random assignment to labour market programmes has been tried only once in Norway, and Raaum and Torp (2002) illustrate a number of problems with this experiment.

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 in a non-parametric fashion within the framework of what we could label a 'cooperative risks' hazard rate model, by which we mean a type of a competing risks model where a transition to one destination state does not remove the subject from the risk of making a transition to another destination state, but may alter the corresponding transition probability. The set-up is as follows: Each entrant into ordinary (open) unemployment is from the start of the spell subject to the risks of both *employment* and *labour market programme participation*. If the first of these processes is realised first, the spell ends. But if the the person enters into a labour market programme (i.e. second process is realised first) the unemployment spell continues in the form of a single risk model, since the person is still considered to be at risk of making a job transition. However, the employment hazard may change as a result of the ongoing programme participation. If the programme is completed without an employment transition, the person returns to ordinary unemployment, and is once more subject to risks of both employment and programme participation. Again, the employment hazard may change as a result of the by now completed programme. Both hazard rates are affected by observed as well as unobserved covariates. The identification of treatment effects depends of course critically on our ability to identify the process of *selection on unobservables*, i.e. the nature of the stochastic dependence between unobserved characteristics that affect the programme propensity and unobserved characteristics that affect the employment propensity. The basic idea pursued in the present paper is that *the influences that unobserved heterogeneity exert on current exit rates are revealed through variation in past exit rates* (earlier in the same spell). A person who, according to his/her path of past explanatory variables, has been subject to a high probability of making a particular transition, *without actually making it*, will on average have unobserved characteristics that are relatively unfavourable for making that transition. The bottom line of the identification strategy is that while the past hazard rates cannot reasonably have a causal impact on today's hazard rates, they have certainly had an impact on the selection process among the unemployed. Hence, they provide the information necessary for identification of this selection process and the associated unobserved heterogeneity.

Our methodology is similar to the *timing of events approach* advanced by Abbring and van den Berg (2003) and utilised on Swedish data by Richardson and van den Berg (2002) and on Swiss data by Lalive et al (2002). However, our foundation for identification is completely different. While Abbring and van den Berg (2003) explore identification under the assumption of time-invariant explanatory variables, it is precisely the time variation in explanatory variables that provides identification in our case. This implies that our ability to sort out *causality* from *selection* does not hinge entirely on assumptions such as mixed proportional hazard rates or fixed person-specific heterogeneity across different (repeat) unemployment spells. Neither do we have to rely on distributional assumptions regarding unobserved heterogeneity or the shape of the duration distribution.

The analysis presented in this paper takes advantage of a Norwegian register-based dataset (The Frisch Centre Database), 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 unique 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; see Røed and Raaum (2003) for a general discussion of potential for identification of causal effects embedded in administrative registers. The next section gives a brief description of the data and the use of ALMP in Norway. Section 3 presents the econometric model and discusses the identification issues. Section 4 presents the results, both in terms of estimated causal parameters and statistical significance, and in terms of simulation exercises and substantive significance. Section 5 provides a discussion of the results and Section 6 concludes.

2 The Data and the use of ALMP in Norway

The data that we use comprise all new insured (UI) unemployment spells recorded in Norway during the period from March 1989 to June 2002.[1] We focus on benefit claimants for the reason that they have strong pecuniary incentives to keep on regis-

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

tering until they get a job. Unlike the other Nordic countries, UI insurance in Norway is compulsory. Thus, there are no serious selection problems associated with this limitation, except that persons without previous work experience and persons who voluntarily quit their last job are non-eligible and thereby excluded. As a consequence, our analysis is focused on ordinary labour market programmes for involuntarily unemployed persons, and not the set of particular youth programmes established to aid persons without previous work-experience or programmes aimed at job seekers with limited work capacity.

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. Spells that start and end within the same month are not recorded. 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[2], or if some kind of ordinary work is recorded directly in the unemployment register files. 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. This is a reasonable job transition concept to use in a programme evaluation context if the ‘first

[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.

employer contact' is considered to play 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 type of unemployment status (such as loss of benefits or a reclassification into disability or rehabilitation), the spell is censored. The way we identify job transitions entails an element of measurement error, as some of the assumed employment transitions may in reality be transitions into the educational system. This is particularly the case for younger job seekers. But since youth programmes often aim at re-integrating participants into the regular educational system, this transition can be seen as a success in line with employment. Transitions to other out-of-the-labour-force states are more problematic. For example, we may erroneously classify persons who leave unemployment to take care of, or give birth to, children, as job entrants. We also fail to pick up programme entries if they are followed by an employment transition within the same month. This may contribute to an underestimation of treatment effects.

The stated aims of active 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 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[3]: i) Training , typically in classrooms, which provide occupational skills thought to be demanded by potential employers in the region; 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

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

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 aimed at providing the job seekers with basic job qualifications. While on programme, training participants maintain their unemployment benefits or receive a training allowance. Other programme 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). They are required to continue active job search even during their participation periods.

In the econometric analysis, we estimate separate models for eight different demographic groups, according to gender, age and immigrant status. The main reason for this is that these groups have been subject to different programme structures, e.g. in the form of separate youth and immigrant programmes. 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 per cent 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[4], and its development during the estimation period is depicted in Figure 1. 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

3 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). This paper acknowledges the fact that unemployed persons frequently move into and out of active labour market programmes, and addresses the effects of *ongoing* as well as *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 programme structure that we seek to 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. Training is the major programme, including about half of the spells with programme participation, see Table 1. The distribution of programme types varies across groups. Naturally, work practice scheme typically recruit youths and immigrants with short labour market experience. Among adults, men are more

[4] See Johansen and Eika (2000) for a description of the methodology.

likely to participate in employment subsidy programmes than women, while the opposite gender differential is found for training.

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 (i.e. the timing of the return to open unemployment) 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).

Let $\varphi_k(t, d, x_{it}, z_{it}, v_k)$ 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. $z_{it} \neq (1,1)$).

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. Note, however, that we do not invoke the proportionality assumption as an important part of our identification strategy (see below). Assume also 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 more in detail below). Note that $\varphi_p(t,d,x_{it},z_{it} = (1,.),v_{ik})$ is not defined, as agents by logic cannot 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 in different age groups, but the precise specification vary somewhat between the different datasets). There is also a set of dummy variables indicating calendar month of entry (one for each of the 12 months in the year), and a scalar variable indicating the business cycle situation in Norway at the moment of entry (see section 2). 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 some restrictions on the way treatment effects α_{iktz} vary across individuals and across time. We allow for heterogeneous treatment effects across job seekers who are observationally different. 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 programme was completed (s_{it}.) Furthermore, we assume that these relationships are of a simple linear structure. The period-specific treatment effects can then be written as

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

Participation affects the employment hazard from the start of the treatment period and onwards, but (2) distinguishes between on-programme effects and post-programme effects. Although we focus on employment effects, equation (2) also includes treatment effects in the participation hazard. However, since $\varphi_p(t,d,x_{it},z_{it}=(1,.),v_{ik})$ is not defined, $\alpha_{p11},...,\alpha_{p14}$ are not defined either.

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

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

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

Non-parametric identification of the model is ensured by a substantial exogenous variation in time varying covariates (see McCall (1994) and Brinch (2000), for a formal discussion of the use of time varying covariates to identify hazard rates model with unobserved heterogeneity). In our data, it is the *time itself* that provides the required variation; or more precisely, the parts of the *hazard rate changes over time* that are unrelated to the composition of the population at risk (as captured by σ_{kt}). The most obvious sources of exogenous variation in employment hazards are *business* and *seasonal cycles*[5] (see Figure 1). These cycles also produce variation over time in the programme entry hazard rates, since policy adjustment lags prevent enrolment capacity from adapting continuously to changes in the labour market situation. Moreover the ‘activity stance’ has typically been a contentious issue in Norway, implying that a series of government changes over time have induced a random-assignment-type variation in programme enrolment hazards. The variation that occurs in ‘lagged hazard rates’ (i.e. hazard rates experienced earlier in an unemployment spell), *given* the ‘current’ hazard rates, ensures that the role of unobserved heterogeneity can be identified: The higher the hazard rates regarding a particular transition have been earlier in

[5] Note that this is essentially the same source of variation that Abbring et al (2002) use to identify structural duration dependence in escape rates from unemployment based on *aggregate* outflow data (from different duration classes).

an unemployment spell, the lower is the expected value of the unobserved covariate relevant for that transition, ceteris paribus. If the two unobserved covariates are correlated, it will also be the case that the current expected value of each covariate depends on past values of the competing hazard rate.

We use *spells*, rather than *individuals* as the basic unit for allocation of the two unobserved covariates. Apparently, we then ignore potentially valuable 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 could obviously have been strengthened substantially if we were ready to assume that the unobserved characteristics were fixed at the individual level across different unemployment spells; see e.g. Bonnal et al (1997) for an application based on this idea. In that case, we would have had a sort of fixed-effect-type foundation for identification of causal effects (Abbring and Van den Berg, 2003). There are two reasons why we nevertheless stay away from this strategy. First, we do not believe that unobserved individual characteristics are constant across spells. There may be causal linkages between spells in basically the same way as there are duration effects within spells. In particular, it is likely that there may be strong across-spell linkages in the transition pattern into labour market programmes[6]. Anyway, it seems problematic to identify the within-spell effects of past unemployment on the basis of the *identifying assumption* that there are no between-spells effects. Second, the usage of repeat spells for identification purposes entails some rather awkward selection issues. Within a

[6] We could of course have modelled and estimated the causal linkages between consecutive spells. However, 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.

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.

Rather than imposing a particular statistical assumption on the distribution of unobserved heterogeneity, we approximate the heterogeneity distribution in a non-parametric fashion with the aid of a discrete distribution (Lindsay, 1983; Heckman and Singer, 1984). 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

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

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 it is no longer possible to obtain 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 be noted that the exact location of the mass-points and their associated probabilities are not directly interpretable. The reason for this is that different combinations of mass-point locations and probabilities sometimes produce observationally indistinguishable models. Although this implies that there is a fundamental lack of identification regarding the exact heterogeneity distribution, it is our experience that indistinguishable models have the same parameters attached to all observed covariates (including spell duration indicator variables), and typically also the same lower order moments of the heterogeneity distribution. The latter point suggests that we may interpret the estimated correlation between the two unobserved scalars as reflecting the true pattern of *selection on unobservables*.

Maximisation of (4) is a huge computational task. This is probably the reason why most applications in this area are based on a pre-determined number of mass-points, usually two or three. In order to solve the computational problems associated with full-scale estimation, we have used an optimisation programme tailored for type of data we use[7] (characterised by a huge number of indicator variables). The models were estimated on a supercomputer at the University of Oslo. Our results suggest that the models are far from saturated with two or three support points, and that important parameters change substantially as more support points are included. In some cases, however, there are indications that that the likelihood criterion for model selection results in a kind of over-parameterisation (the typical symptom being ever-increasing positive duration dependence as more support points are included). We identified this problem in two of the models (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.

4 Results

Due to the basically non-parametric estimation strategy, the models that we estimated contained around 4,500 unknown parameters altogether. Some basic model properties

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[7] The program is developed by Simen Gaure at the Centre for Information Technology Services, University of Oslo and the Ragnar Frisch Centre for Economic Research.

are provided in Table 2. The number of mass-points in the distributions of unobserved heterogeneity required to maximise the likelihood functions varied from 5 to 11. The correlation coefficients between the two unobserved variables were negative for all groups, suggesting *negative selection on unobservables to labour market programmes* in our data[8]. 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. Finally, we simulate the overall impact labour market programmes on the distribution of unemployment spells and report summary statistics.

4.1 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[9]. Since we identify the roles played by unobserved heterogeneity, we are also able to identify the degree of *structural duration dependence* embedded in the hazard rates.

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[8] 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. 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.

[9] 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 per cent confidence intervals) to predict the developments in transition rates over spell duration.

Figure 2 around here

Figure 3 around here

The typical pattern revealed by Figure 2 is that there is negative structural duration dependence in the employment hazard during the first part 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 again rise substantially. Our results at this point are in accordance with previous findings reported by Røed and Zhang (2002, 2003). The rise in the employment hazard associated with benefit exhaustion is of course in line with predictions from standard search theory. 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. Hence, the limited duration of benefits can to some extent be viewed as a sort of 'activation requirement', i.e. it is not the income support per se that is cut off, but rather the *passive* income support[10]. This phenomenon can clearly be seen in Figure 3. The transition rate to labour market programmes typically doubles around the time of potential (temporary) benefit exhaustion. 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 *anticipation effect* with respect to required programme participation, although we cannot without additional information or additional assumptions separate these effects appropriately from other sources of struc-

[10] For most of the period covered in this paper, there have also existed various exemption practices that have allowed a number of unemployed persons to maintain passive benefits beyond the prescribed period of 18 months.

tural duration dependence[11]. Hence, our results confirm previous findings reported by Black et al (2002) and Røed et al (2002), indicating that compulsory programme participation (in exchange for benefits) can counteract some of the moral hazard problems associated with unemployment insurance.

4.2 On-programme and post-programme effects

The parameter estimates regarding effects of ongoing and completed programme participation are reported in Table 3. In each panel, I-III, the constant term reports the estimated effects for a 'reference spell' (which is defined in Table 3) within each demographic group. The associated proportional change in the hazard rate is equal to exp(parameter estimate). For example, for a prime aged native 'reference' woman, the point estimate suggests that current programme participation reduces the employment hazard according to the factor exp(-0.485)=0.62, i.e. by 38 per cent, and that a completed programme raises the hazard by the factor exp(0.313)=1.37, i.e. by 37 per cent. The interaction terms report the estimated changes in the effects that occur when certain observation characteristics are modified (the ranges of variation in these characteristics are reported at the bottom of the table). The bottom line of the results presented in Table 3 is that the effect of an ongoing programme on the employment hazard is negative for four of the eight groups, while the effects of a completed programme are positive for all. This suggests that the programmes do have their intended effect of improving the job prospects of the participants, but that there is a cost in terms of lower employment transition rates during the period of actual participation. The latter opportunity cost seems to be much larger for adult women than for adult

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[11] There have been a number of small changes over time in regulations and practices regarding the maximum benefit duration period in Norway and the associated use of labour market programmes, and these changes may provide the additional information required to identify benefit exhaustion effects more precisely. This is the topic of ongoing research at the Frisch Centre.

men. Actually, the on-programme effect is close to zero for men, with an exception for the young. The post-programme effects are largest immediately after programme completion (as $\widehat{\alpha}_{e25}$ is negative), but it typically takes more than a year to wipe out the effects entirely. 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.

There are substantial heterogeneities in individual effects. A clear pattern is that favourable effects are larger - and opportunity costs lower - the poorer are the individual employment prospects to start with. Both programme effects are negatively related to educational attainment and they are higher for Non-OECD immigrants than for natives. Older workers seem to gain less as the post-programme effects are systematically declining in age. It is also worth noting that the post-programme effects tend to be larger the better are the business cycle conditions, in line with findings in Raaum, et al. (2002a). 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.

We find strong positive post-programme-effects on the programme (re)entry hazard rate. Hence, the event of having participated once substantially increases the probability of participating in the near future also.

Table 3 around here

The precision by which individual programme effects can be established vary somewhat from group to group. The standard errors in these predictions are typically around 0.02-0.04 for the largest groups (young and prime-aged natives), 0.05-0.06 for the medium sized groups (older natives and immigrant men), and 0.10 for the smallest group (immigrant women).

In order to evaluate the distribution of programme effects, Table 4 reports the average and the standard deviation of the predicted effects for actual participants, as well as for randomly selected entrants. Panel I is based on the estimated programme effects distributions of the actual participants (i.e. calculating the individual effect associated with the individual and business cycle characteristics of the actual participants). The average post-treatment effects are quite substantial for all groups, and the standard deviations indicate that this effect is favourable for most participants. We find more variation in on-programme effects. Women have the largest opportunity costs in terms of reduced employment hazards during the programme period.

Table 4 around here

There are in general small differences between the effects predicted for actual participants and randomly selected entrants, compare panel I and II in Table 4. In some of the groups, it seems that the effects for actual participants are smaller than they would have been for randomly selected entrants. One reason for this is that the scale of labour market programmes has been much larger during bad times than during good times, while the positive effects are larger in good times than in bad times. For *given business cycle conditions*, see panel III and IV, there is indeed a weak tendency for actual participants to have larger positive effects than random entrants. There nevertheless seems to be plenty of scope for improving the mix of participants into ALMP in terms of maximising the effect on employment transition rates for actual participants. However, an aim of maximum effect on the treated may obviously conflict with the use of ALMP as a work-test to diminish the moral hazard problems embedded in the unemployment insurance system.

4.3 Overall effects on unemployment durations of overall program activities

The translation of the effects on transition probabilities during and after programmes into a concept that measures the impact on total unemployment exposure is not trivial. Programme effects in terms of, say, change in the expected duration of unemployment, cannot be solved analytically, since we do not know the future development of all explanatory variables (such as business cycles). In order to illuminate the impact on total unemployment of the estimated treatment effects, we simulate the progression of our unemployment spells, *given their actual starting dates*, under alternative assumptions about existence of labour market programmes. We then use the simulated distributions of unemployment spells to assess how the active labour market policy in Norway has affected the degree of unemployment in 1989-2002 through the on-programme and post-programme effects.

We first use the 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 per cent (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 or loss of benefits) that we have tried to reproduce in a similar fashion. Despite these differences, the model produces spell distributions that are very similar to observed data. We proceed by simulating the progression of unemployment spells under the counterfactual assumption that no labour market programmes exist (or equivalently, that they have no causal effects), but that all other parameters are unaffected.

The main results from these simulation exercises are provided in Table 5. The overall effects of the programmes, in terms of reduced unemployment duration, have been favourable for adult men and for immigrants. For females and for young job seekers, it appears that the lock-in effects during participation on average outweigh the favourable effects afterwards with respect to total spell duration. The overall effects are modest for all groups, ranging from a reduction in total unemployment exposure of 4.11 per cent (immigrant men) to an increase of 3.31 per cent (prime aged women).

Table 5 around here

Since ALMP to a large extent is targeted at the long term unemployed, the overall effects are far from equally distributed across unemployment spell durations. But, since four out of five spells are over before programme enrolment is realized, it is obvious that ALMP cannot have a large impact on the overall distribution of spell lengths. However, given that a spell is expected to be very long without ALMP, the effect of ALMP can be quite substantial, at lest within some of the demographic groups. This point is illustrated in Figure 4, where we have plotted the predicted percentage change in the number of spells at each duration resulting from the existence of labour market programmes (recall that the total number of spells is given). Apart from young men and prime aged women (for which the negative on-programme effects dominate), there is a very clear pattern that ALMP tend to reduce the longest unemployment spells. For example, for prime aged men, the number of very long spells (i.e. those lasting three years or more) during the 1989-2002 period was reduced by around 15 per cent (from 5,429 to 4,623) as a result of ALMP.

5 Discussion

Our results indicate that active labour market programmes have relatively small effects on the total unemployment duration for most participants, when unemployment duration is measured as time until some kind of ordinary work. The simulations suggest that e.g. one month (25 days) of programme-activities on average produces a change in expected unemployment duration ranging from -0.21 months – i.e. a one-week reduction – for immigrant men, to 0.17 months – i.e. almost a one-week increase - for prime aged women. The gender difference among adults arises because the on-programme effect is, on average, negative for women but negligible for men. This may reflect the different composition of programmes for men and women, as employment subsidy (training) is more frequent for men (women). Direct employment transitions are possibly more likely to take place from programmes that involve contact with a (private) employer.

The programme effects vary substantially according to both individual characteristics and business cycle developments. To the extent that programmes are targeted at persons with particularly poor individual employment prospects (according to observed characteristics), they seem to have substantial favourable effects. Our results also indicate that the stance of active 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, the favourable effects seem to be larger 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 ALMP) increases in good times.

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 programme activity 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. (2002b). Hence, with a possible exception for male immigrants, the employment gains of active labour market programmes in Norway do not seem to exceed the direct costs. However, policy recommendations must consider a wider range of possible consequences. First, we do not observe the amount of work forgone due to programme participation, and it seems indeed likely that current programme participation has a particularly large negative effect on occasional and/or part-time work, since these kinds of activities may be difficult to combine with full time programme participation. Moreover, it may be argued that programme participation involves productive use of resources even during the course of the programmes, either in the form of skill-formation (training) or in the form of productive work (employment programmes, job subsidies). As we demonstrated in Section 4.1, there are also indications that the mere existence of programmes has a substantial 'threat' effect, which raises the employment transition rates even for persons who do not actually participate. Finally, we do not measure the impact on future employment careers and productivity.

A welfare evaluation of ALMP also has to take into account their redistribution effects. Although the effects on total unemployment exposure in the economy are small, they are (with a possible exception for immigrants) quite effectively targeted at the long-term unemployed, hence total unemployment exposure is more equally distributed among the unemployed in the ALMP-economy than in the no-ALMP-economy (see Figure 4). To the extent that long-term unemployment is considered a particularly traumatic experience, a reduction in the longest particularly traumatic experience, a reduction in the longest unemployment spells may be considered 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 taken in to account. These include the possibility of substitution (that participants obtain jobs at the expense of non-participants) and the possibility that level of ALMP affects the wage formation in the economy. They also include the possibility that the existence of ALMP affects 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.

6 Conclusions

In this paper, we have outlined a method for evaluation of treatment effects, within a hazard rate framework, that can be used on non-experimental data without reliance on fixed individual effects across different spells and without restrictive assumptions regarding functional forms of the hazard rates or the distribution of unobserved heterogeneity. The only identifying assumption we need in order to identify the *causal* effects of ongoing and completed programme participation on the employment hazard is that hazard rates experienced earlier in an unemployment spell do not have a direct *causal* effect on the current hazard rates. We have applied the method on Norwegian register data collected in the period from 1989 to 2002, encompassing more than 1.4 million unemployment spells.

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 duration until some kind of ordinary work was obtained were not always favourable.

The most important policy implications of our findings are: i) that 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 ALMP seems warranted from a social welfare point of view; ii) that the programmes should be targeted at persons with poor individual employment prospects; iii) that there are particularly large favourable effects for immigrants from developing countries; iv) that the overall welfare gains of ALMP depends on the social welfare function, as individual effects differ across skill and unemployment duration groups; (v) that ALMP 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) that the programme activity level should not accommodate business cycle changes completely.

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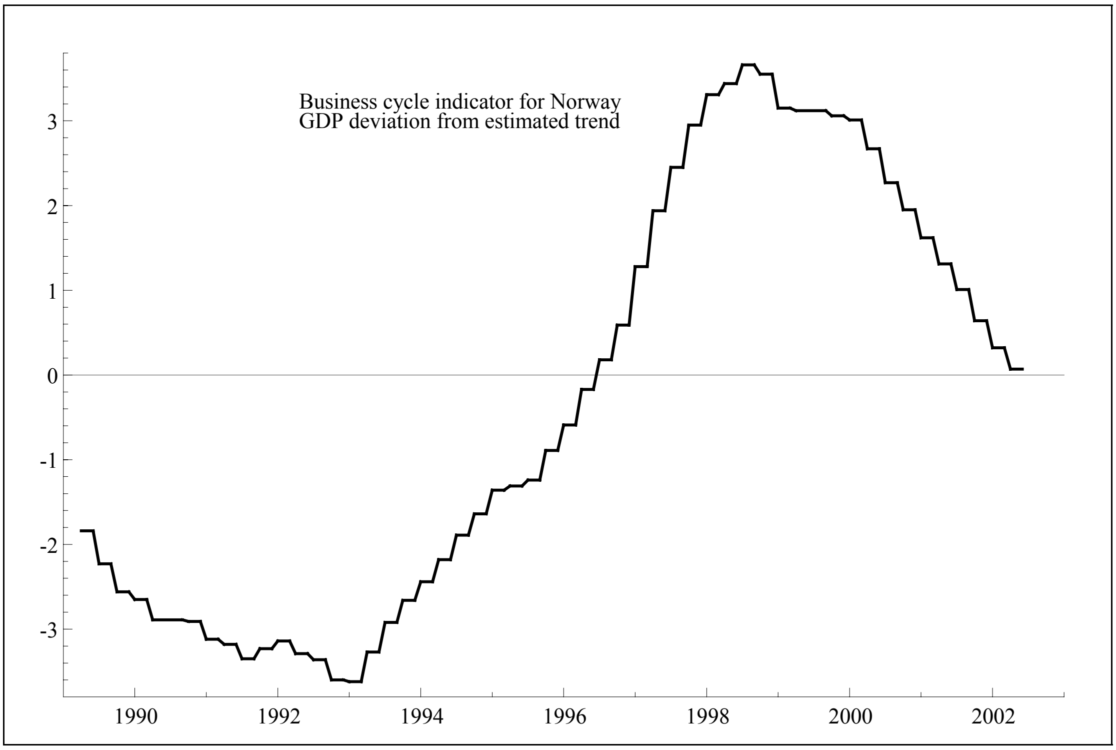

Figure 1. Business cycle developments in Norway during the estimation period, according to the GDP development.
Source: Statistics Norway

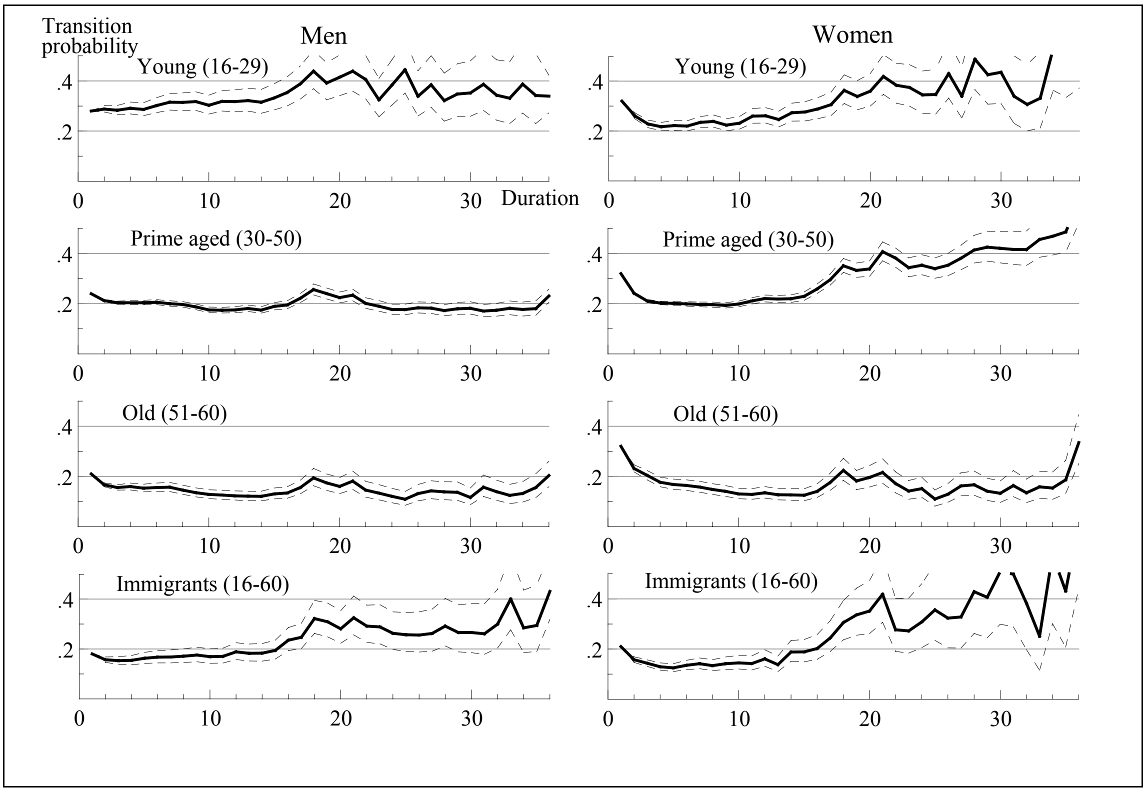

Figure 2. Estimated transition probabilities (grouped hazard rates) from open unemployment to employment for a representative entrant into open unemployment (with 95 per cent confidence intervals) as functions of spell duration (measured in months).

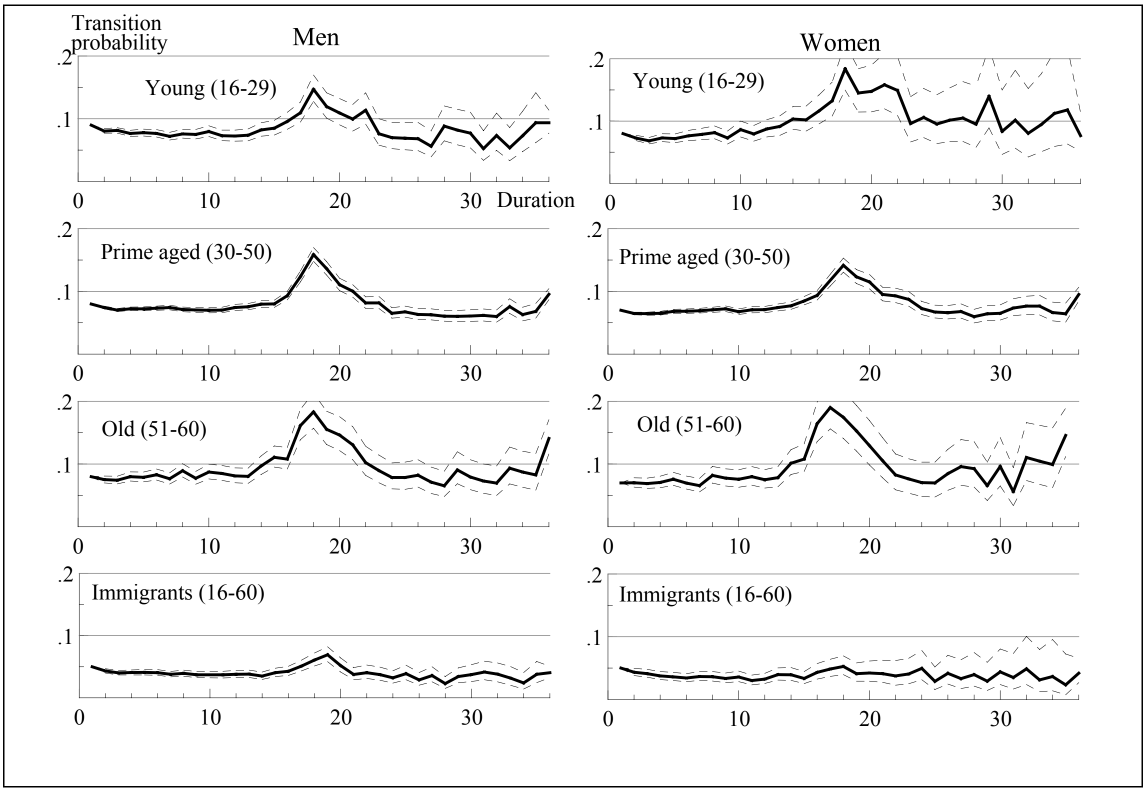

Figure 3. Estimated transition probabilities (grouped hazard rates) from open unemployment to programme participation for a representative entrant into open unemployment (with 95 per cent confidence intervals) as functions of spell duration (measured in months).

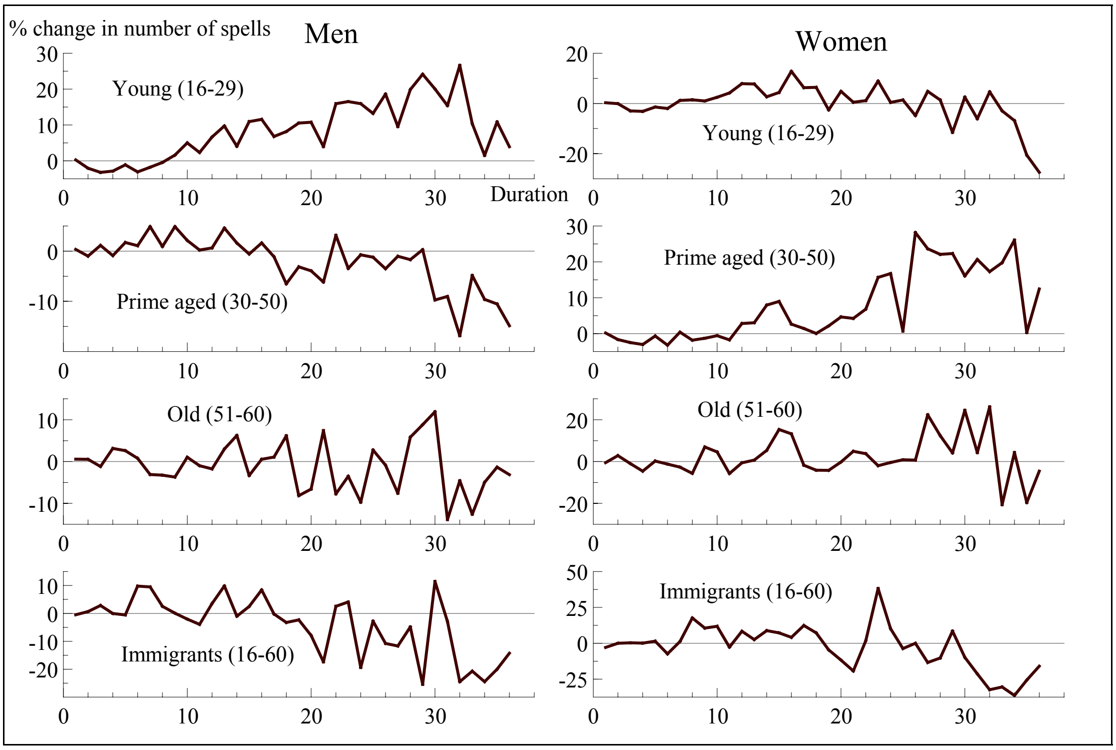

Figure 4. Predicted percentage change in the number of spells at each duration as a result of ALMP in Norway 1989-2002 (given the total number of spells in each group)
Note: The longest duration is 36 months *or more*.

Table 1
Descriptive Statistics

	Men 16-29	Women 16-29	Men 30-50	Women 30-50	Men 51-60	Women 51-60	Immigrant[1] Men 16-60	Immigrant[1] 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[2]:								
Education/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
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
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

[1] The group of Immigrants encompasses immigrants from non-OECD-countries only. Immigrants from other countries are not included in the analysis.
[2] 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

Note: 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
Estimated Treatment 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 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)
II. 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)

Table 3
Estimated Treatment 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
Interaction with time since completion (α_{e25})	-0.021	-0.014	-0.025	-0.039	-0.016	-0.028	-0.025	-0.019
	(0.006)	(0.007)	(0.002)	(0.003)	(0.004)	(0.004)	(0.006)	(0.012)
Interaction with programme duration (α_{e26})	0.013	0.015	0.002	0.013	-0.007	0.009	-0.007	0.008
	(0.005)	(0.005)	(0.002)	(0.003)	(0.005)	(0.006)	(0.007)	(0.012)
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	-0.356	0.234	0.274	0.217	0.240	0.391	0.738
	(0.048)	(0.075)	(0.028)	(0.028)	(0.064)	(0.075)	(0.064)	(0.080)
Interaction with age (α_{p22})	0.021	0.049	-0.004	-0.004	0.017	-0.001	0.006	-0.001
	(0.007)	(0.009)	(0.001)	(0.002)	(0.009)	(0.013)	(0.004)	(0.006)
Interaction with educational level (α_{p23})	0.049	0.146	0.054	0.029	0.006	-0.050	-0.010	-0.068
	(0.022)	(0.032)	(0.011)	(0.012)	(0.021)	(0.032)	(0.023)	(0.040)
Interaction with business cycle (α_{p24})	-0.046	-0.078	-0.051	-0.044	-0.069	-0.057	-0.012	-0.028
	(0.014)	(0.016)	(0.005)	(0.006)	(0.012)	(0.015)	(0.012)	(0.019)
Interaction with time since completion (α_{p25})	-0.005	-0.030	-0.005	-0.010	-0.003	-0.016	-0.034	-0.071
	(0.005)	(0.009)	(0.002)	(0.003)	(0.004)	(0.006)	(0.005)	(0.013)
Interaction with programme duration (α_{p26})	-0.003	-0.022	-0.002	-0.009	-0.010	-0.029	0.003	-0.014
	(0.005)	(0.007)	(0.002)	(0.003)	(0.006)	(0.009)	(0.006)	(0.012)

Note: 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 4
Programme effect distributions. Average and standard deviation. Actual and randomly selected participants.

	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. Predicted effects of actual participants								
On-programme effect, average	-0.350	-0.336	-0.027	-0.278	-0.076	-0.326	0.050	-0.086
[standard deviation]	[0.224]	[0.211]	[0.213]	[0.253]	[0.164]	[0.255]	[0.331]	[0.312]
Post-programme effect[1] average	0.211	0.467	0.403	0.510	0.276	0.416	0.458	0.674
[standard deviation]	[0.151]	[0.120]	[0.135]	[0.140]	[0.092]	[0.178]	[0.269]	[0.336]
II. Predicted effects of randomly selected participants[2]								
On-programme effect average	-0.386	-0.357	-0.035	-0.306	-0.066	-0.322	0.054	-0.089
[standard deviation]	[0.229]	[0.217]	[0.219]	[0.263]	[0.174]	[0.262]	[0.338]	[0.320]
Post-programme effect[1] average	0.187	0.459	0.398	0.506	0.286	0.122	0.462	0.672
[standard deviation]	[0.154]	[0.124]	[0.135]	[0.143]	[0.091]	[0.182]	[0.274]	[0.345]
III. Average predicted effects of actual participants with business cycle at trend								
On-programme effect	-0.294	-0.263	0.053	-0.229	-0.003	-0.277	0.109	-0.078
Post-programme[1] effect	0.244	0.517	0.422	0.544	0.279	0.417	0.505	0.682
IV. Average predicted effects randomly selected participants[2] with business cycle at trend								
On-programme effect	-0.343	-0.302	0.029	-0.271	-0.007	-0.283	0.106	-0.084
Post-programme[1] effect	0.212	0.497	0.414	0.530	0.289	0.422	0.504	0.678

[1] The post-programme effect measured at the first month after the programme.
[2] Individuals with 'first month'-characteristics.

Table 5
Simulated unemployment spells in Norway 1989.3-2002.6, given actual starting dates, with- and without labour market programmes

	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								
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 labour market programmes (zero programme effects)								
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								
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 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 exposure produced by each month of actual programme participation)	0.13	-0.01	-0.14	0.17	-0.10	0.10	-0.21	-0.06