19 June 2009

Job Search Institutions and Job Match Quality

Simen Gaure, Knut Røed, Lars Westlie*

Abstract

We examine empirically the impacts of time-limited unemployment insurance (UI) and active labor market programs (ALMP) on the duration and outcome of job search. We find that time invested in job search normally pays off in the form of higher earnings once a job match is formed. But jobs accepted close to UI exhaustion are of significantly lower quality than jobs accepted earlier in the search process. Participation in ALMP raises the probability of eventually finding a job as well as the level of expected earnings, but at the cost of lengthening job search. The programs are cost-effective under realistic assumptions regarding the value of within-program production.

Keywords: Multivariate hazards, job search, job quality, timing-of-events, NPMLE, MMPH
JEL classification: C14, C15, C41, J64, J65, J68

* This research has received financial support from the Norwegian Research Council through the research project "Labor market exclusion", and from the Norwegian ministries of Labor and Social Inclusion and of Finance through the "Strategic Institute Program on Labor Market and Pension Research". It is also part of the research at the ESOP Centre at the Department of Economics, University of Oslo. Thanks to Erik Biørn, Bart Cockx, Ott Toomet, and anonymous referees for comments. Thank also to participants at the ESOP research seminar in Oslo, the Second Joint IZA/IFAU Conference on Labor Market Policy Evaluation in Bonn, and the QMSS research seminar on life course analysis in Oslo. Correspondence to: Knut Røed, the Ragnar Frisch Centre for Economic Research, Gaustadalléen 21, 0349 Oslo, Norway. E-mail: knut.roed@frisch.uio.no.

1. Introduction

In this paper, we set up a comprehensive simultaneous equations model accounting for i) the duration and outcome of individual unemployment spells; ii) the subsequent employment stability; and iii) the earnings level associated with the first job after unemployment. The model is designed to examine short- and long-term impacts of external job search conditions as well as of selective treatment interventions. It is estimated on Norwegian administrative register data covering all new unemployment spells from 1993 to 2001.

It is a well known fact that job search conditions – as reflected in, e.g., unemployment insurance (UI) and active labor market programs (ALMP) – affect the opportunity cost of continued job search, and, hence, a job seeker's fastidiousness and search effort. Participation in ALMP potentially also affects human capital and, hence, the distribution of available job opportunities. A number of empirical studies have examined how these effects play out with respect to the duration and outcome of unemployment spells. Typical findings are that higher UI replacement ratios yield longer unemployment durations, and that the probability of escaping unemployment increases as UI entitlements are exhausted. However, in a recent review of the literature, Card *et al.* (2007) show that the estimated behavioral responses tend to be much smaller when the spells are measured by the time to next job than when they are measured by the time spent in the UI system. The empirical evidence regarding impacts of UI on job match quality is sparse. Economic theory suggests that UI may encourage job seekers to wait for more productive jobs; see Marimon and Zilibotti (1999) and Acemoglu and Shimer (1999; 2000). If credit markets are imperfect, UI insurance also involves a non-distortionary income (liquidity) effect (in addition to the distortionary substitution effect), reducing the pressure on credit-constrained individuals to accept suboptimal job matches (Chetty, 2008). The relatively sparse existing empirical evidence does not, however, provide any overwhelming evidence that increased UI generosity actually improve job matches. Addison and Blackburn (2000) report evidence of a weak favorable impact of UI on the post-unemployment wage, while Belzil (2001) and Centeno (2004) report evidence of a small favorable impact on job duration. Van Ours and Vodopivec (2008), however, concludes from a "natural experiment" in Slovenia that shortening the duration of UI benefits does not affect either post-unemployment wages or job duration.

The empirical literature on the effects of ALMP participation is extensive, but also subject to large variations in research design, data, and the selection of outcome measures; see Kluve et al. (2007) for a recent overview. There is a sort of Atlantic divide in the approach to treatment evaluations, in the sense that US studies tend to focus on earnings (or wage) effects while European studies tend to focus on employment effects. Studies investigating the impact of program participation on subsequent employment propensity are typically modeled within a duration analysis framework, while the few European treatment evaluations investigating earnings effects are based on matching techniques. To our knowledge, no empirical evaluation analysis has yet attempted to model the duration of the job search process and the quality of its outcome within a unified simultaneous equation modeling framework, such that, e.g., favorable earnings effects can be traded off against higher job search costs due to adverse lock-in effects during the participation period. A key aim of the present paper is to fill this gap, and thereby to facilitate a direct comparison of costs and benefits.

Based on the timing-of-events approach (Abbring and Van den Berg, 2003), we set up a multivariate hazards model to analyze transitions out of registered unemployment; to employment, as well as to ordinary education and to inactivity. During the unemployment spell some job seekers are sorted into ALMP. We examine the causal impacts of participation in ALMP on the duration and outcome of job search and on the quality of a resultant job. The latter is measured in terms of monthly earnings and employment duration. In addition to controlling for a rich set of observed explanatory variables, we allow for jointly distributed unobserved heterogeneity by means of the nonparametric maximum likelihood estimator (NPMLE). Our preferred model contains a discretely distributed six-dimensional vector of unobserved heterogeneity with 27 distinct support-points.

The key findings of our paper are the following: First, during its first six months, the job search process is productive in the sense that the expected earnings increase significantly with the time spent searching. On the other hand, the probability of actually obtaining an acceptable job offer declines quite sharply with unemployment duration. And after one year of job search, expected earnings also start to decline. Second, reservation wages decline sharply in the run-up to UI exhaustion, causing the job hazard to rise and the expected earnings level to decline in this period. And finally, participation in ALMP initially reduces the employment hazard (lock-in effect), but the impact becomes favorable after around 6 months of participation. For most participants and program durations, the employment hazard is also significantly higher after participation than it was before entry into the program. In addition, participation in ALMP tends to improve subsequent earnings. Based on model simulations, we summarize the various treatment effects in terms of a comprehensive earnings (value of work) measure, covering a five-year period after the start of unemployment. Even though program participation raises both the probability of eventually finding a job and the level of earnings given that a job is found, it contributes to reduce overall earnings derived from ordinary jobs during the first five years after entry into unemployment. The reason is that it also tends to increase the duration of the overall job search period (including the participation period). Given that ALMP also involves some administrative costs, this implies that it is difficult to defend the programs from a cost-benefit point of view when considering the impacts on subsequent employment performance only. However, many of the program activities (around 60 percent) involve some form of subsidized employment. The condition for a simple five-year cost-benefit analysis to deliver a favorable result is that the economic value of subsidized work is, on average, at least 35 percent of the participants' predicted earnings from non-subsidized work.

The next section presents the data and the institutions from which they are generated. Section 3 describes the empirical methodology and discusses identification, and Section 4 presents the results. Section 5 concludes.

2. Data and institutional background

We use administrative data encompassing all new entrants into registered unemployment in Norway during the period from October 1993 to September 2001. The term "new" is defined as not having had any unemployment experience during the past three years prior to the *first* spell in our data window (we use registers back to 1989 to implement this condition for early entrants). We focus on new entrants in this analysis in order to model the complete unemployment history for each individual, realizing that there might be causal linkages between subsequent spells and their outcomes. Given that our data window covers 8 years, the delimitation to new entrants does not imply that long-term unemployed and individuals with repeated spells are disregarded. Even the longest unemployment careers have to start at some point, and given that they start during the period spanned by our data, we model the subsequent employment and unemployment experiences until October 2001.

Table 1 offers some key descriptive statistics. There are 373,065 individuals included in our analysis with 413,988 "new" entries into unemployment. Approximately 41,000 individuals (11 percent) have more than one new entry during the 8 year long data-window. In the statistical analysis, multiple new unemployment spells will be treated as *causally* unrelated. But, as we explain in the next section, they will be related through the assumed persistence of unobserved covariates. In total, around 124,000 individuals (33 percent) experienced more than one unemployment spell. Repeated unemployment spells starting less than three years after the end of a previous spell will be treated as related both through a causal effect (lagged duration dependence) and through the persistence of unobservables.

Table 1
The Data – Descriptive statistics corresponding to the time of first entry into unemployment

Number of individuals	373,065
Number of new unemployment entries 1991.9-2001.9*	413,988
Mean age at first entry	28.22
Mean number of years of work experience at first entry (conditional on >0)	4.20 (9.03)
Percent of entrants female	52.25
Percent of entrants with immigrant (non-OECD) background	9.62
Percent with UI at first entry	55.40
Percent of individuals with	
One unemployment spell only in data window	66.73
Two spells	21.28
Three spells	7.50
Four spells or more	4.49

* A “new” entry is defined as becoming unemployed after at least three years without any unemployment.

The time period covered by our analysis was characterized by substantial changes in external job search conditions. First, labor demand fluctuated substantially. This is illustrated in the upper panel of Figure 1, where we report a labor market tightness indicator for Norway measuring the time-path of the monthly job transition probability controlled for observed and unobserved individual characteristics, spell duration, and seasonal fluctuations; see Gaure and Røed (2007) for details. Employment prospects improved steadily until the autumn of 1998. During the recovery period from the trough in December 1992 (outside our data window) to the peak in September 1998, a typical job seeker’s monthly probability of finding work doubled, ceteris paribus. From the autumn of 1998, employment prospects again deteriorated. As can also be seen from the graph, the cyclical fluctuations embodied in the labor market tightness indicator correlate well with the pattern of new inflows to unemployment observed in our own data. Second, the overall scale of ALMP also changed substantially. This is illustrated in the lower panel of Figure 1, where we show how ALMP intensity – defined as the fraction of long-term unemployed job seekers participating in ALMP – developed over time. The figure clearly indicates that the frequency of ALMP was scaled down during the late 1990’s, reflecting new political priorities. Third, in the middle of our data period (January 1997), the Norwegian UI system was reformed. The old UI system offered an initial maximum UI duration of 80 weeks which could be extended by 13 weeks, after which an additional 93 week period could be granted at a somewhat reduced benefit level if no employment or suitable ALMP activities could be found; see Røed and Westlie (2007) for details. The new UI system offered an uninterrupted UI period of 156 weeks for most job seekers.

Throughout the period, the UI replacement ratio has remained stable at 62.4 percent of previous earnings, up to an annual earnings ceiling around 65.000$ (in 2009). Eligibility requires that earnings in the year prior to the year of entry into unemployment (or the average of the past three years) exceeded approximately 11.000$, and that unemployment resulted from involuntary job loss; see Røed and Zhang (2003) for details. Hence, labor market entrants are not eligible for UI, which explains why only around 55 percent of the job seekers in our data claim benefits.

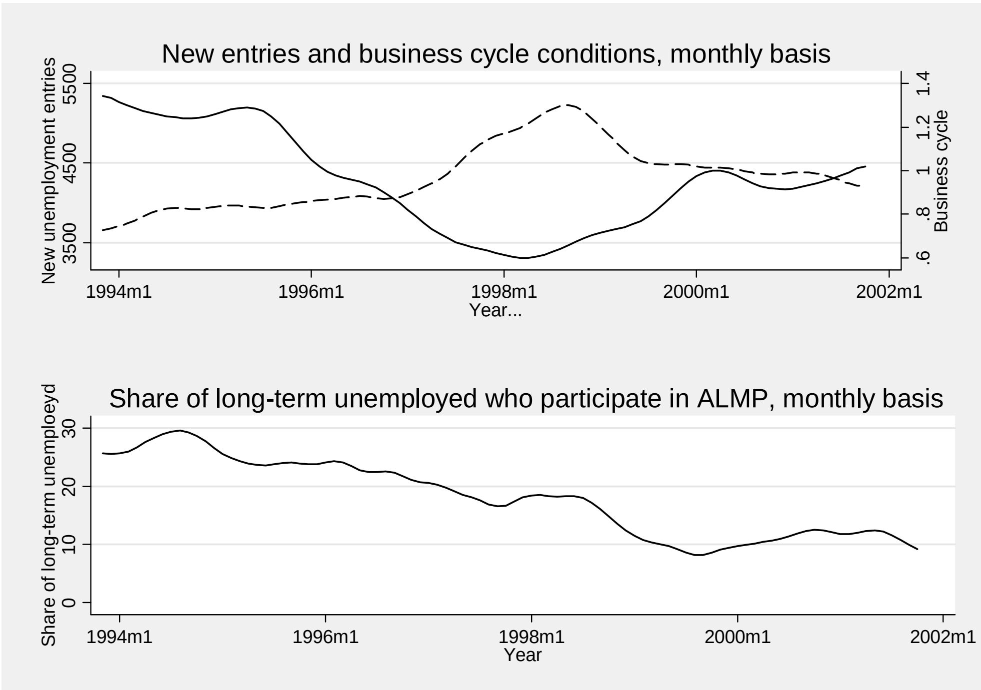

Figure 1. Upper panel: Labor market tightness (dotted line) and the number of new entrants (solid line) to unemployment. Lower panel: The share of long term unemployed (more than 6 months) participating in labor market programs.
Note: The monthly series are smoothed with X11ARIMA. The labor market tightness indicator is collected from Gaure and Røed (2007). It is normalized on June 2000 (representing a "normal" cyclical condition) and can be interpreted as relative changes in the monthly job transition rates over time, conditional on observed and unobserved characteristics and on unemployment spell duration.

Note that we do not interpret the 1997 reform primarily as an increase in the overall length of the maximum UI duration. The *absolute* duration limit was actually higher in the old than in the new regime. The main content of the reform was that the UI system changed from focusing on *activation* to focusing on *income insurance and job search*. This change in focus largely explains the decline in ALMP participation shown in Figure 1. The reform illustrates an intimate structural relationship between UI design and activation policies in Norway, arising from the dominant view that there exists a lower bound on the income level that can be offered to unemployed job seekers who lost their previous work involuntarily, regardless of their observed search behavior. In practice, this implies that credible UI termination threats can be made only to the extent that paid activation is offered instead. Hence, the reduction in ALMP-intensity and the removal of the “soft” duration constraint after 80 weeks of job search can be viewed as two sides of the same coin.

Norwegian labor market programs come in four different forms; i) labor market training, ii) temporary public employment, iii) temporary wage subsidies targeted at the private sector, and iv) work practice schemes. Røed and Raaum (2006) show that there are significant differences in the selection of program types across different demographic groups. Labor market training and temporary wage subsidies are the most commonly used programs for adults, whereas work practice schemes are almost exclusively reserved for youths. The duration of the participation periods vary somewhat across the different programs; mean *completed* treatment durations in our data are 4.8 months for training programs, 8.2 months for public employment, 4.1 months for wage subsidies, and 6.0 months for work practice schemes. Participants in ALMP receive payments similar to typical UI benefit levels, and UI claimants do not draw on their benefit entitlements while participating (unless they prefer to maintain UI benefits instead of ALMP payment). The allocation of program slots results from a combination of administrative and individual sorting; the initiative may be taken by the caseworker as well as by the job seeker. For UI claimants, ALMP is frequently offered as a sort of work-test, and rejections may lead to termination of benefit payments. For non-claimants, ALMP participation sometimes represents the only available earnings option.

3. Methodology

Starting with the flow of first-time entrants into the state of unemployment, we set up a multivariate mixed semi-proportional hazard rate model (MMSPH), expanded to comprise a log-linear earnings equation for those who get an ordinary job. The model accounts for transitions to employment, to ordinary education, and to social security benefits that do not require continued job search (sickness benefits, rehabilitation benefits, disability benefits or social assistance). During the unemployment spell, transitions to ALMP may occur. ALMP participation is modeled as an endogenous event, and it is assumed to induce shifts in all hazard rates, both during the participation period (on-treatment effects) and afterwards (post-treatment effects). The sizes of the shifts may depend on gender, initial human capital, program duration, and business cycle conditions. Note, however, that we do not distinguish between different types of programs; rather we view the matching of a particular unemployed person to a particular program activity as an integral part of the active labor market policy that we intend to evaluate. All hazard rates are potentially affected by the duration of the ongoing spell, as well as by the duration and outcome of previous spells. All hazards are also affected by the duration of remaining UI entitlements. For individuals who make a transition to employment, it is assumed that the initial earnings level and the subsequent employment termination hazard, depend on the conditions under which the job was accepted (in terms of, e.g., remaining UI entitlements at the time of the job transition) and on previous ALMP participation. All hazard rates as well as earnings are assumed to depend on observed and unobserved characteristics and on calendar time. The various unobserved characteristics (random effects) are allowed to be interrelated in an unrestricted fashion, implying that the parameters of the model are recovered by means of the nonparametric maximum likelihood estimator (NPMLE); see Heckman and Singer (1984) and Gaure *et al.* (2007).

3.1 Model specification

We set up a multivariate mixed semi-proportional hazard rate model with five events $k=1,...,5$, together with an earnings equation. The five events are:

1. Termination of the unemployment spell with transition to employment
2. Termination of the unemployment spell with transition to ordinary education
3. Termination of the unemployment spell with transition to other benefit (that does not require continued job search)
4. Entry into ALMP (does not terminate the unemployment spell)
5. Termination of a subsequent employment spell

All the five hazard rates, as well as expected earnings level, are tied together through the joint distribution of unobserved heterogeneity. While we model the entry into ALMP ($k=4$) as an endogenous event, we treat the *potential* duration of program participation (i.e., the duration in the absence of any transition out of unemployment) as exogenous. This is a questionable strategy. Although the length of each ALMP activity is indeed predetermined, we cannot rule out a systematic (unobserved) sorting process into programs of different potential durations. Previous evidence also indicates that job subsidies (which tend to be short-lasting in our data) are more effective than temporary public employment (which tend to be long-lasting); see Kluve *et al.* (2007). These factors may bias the results regarding the impacts of ALMP *duration*. Based on existing knowledge regarding the sorting process (Røed *et al.*, 2000), we hypothesize that the bias will be negative, in the sense that job seekers allocated to programs of longer duration perform poorer than job seekers allocated to shorter programs, both as a result of unobserved sorting and as a result of changes in the composition of program types.

For each transition into ordinary employment ($k=1$), we also include an earnings equation designed to explain the level of earnings derived from the first full month of employment. Monthly earnings are computed by dividing the annual earnings from a given job on the number of months worked. Unfortunately, the data do not provide sufficient information for identifying the number of work-hours and the hourly wage rate separately. Note, however, that all the job seekers included in our analysis have declared interest in a full-time job. High monthly earnings may therefore be viewed as a desirable job characteristic, even when it results from a large number of hours rather than a high hourly wage. Note also that we treat self-employment as a transition to employment. The initial earnings level for self-employed are computed from annual tax records (based on the assumption that earnings were equally distributed across the non-unemployment months).

When a job spell is terminated ($k=5$), the worker may return to unemployment, in which case a new unemployment spell is started off. Otherwise, e.g., if an employment spell is followed by other welfare benefits or education, the event history is terminated at this point.[1] The model is proportional, in the sense that unobserved as well as most observed covariates are assumed to affect individual hazard rates multiplicatively. However, as we explain below, the model is a generalization of the standard MMPH model, since it allows for interactions of duration dependencies and the impact of some observed explanatory variables. This is why we use the term “semi-proportional” – MMSPH – to describe it.

Table 2 provides a descriptive overview of the events recorded in the data. A key point to note is that only 47 percent of the completed spells end with a transition directly to employment. The remaining transitions are evenly distributed between education, benefit shifting, and other (non-modeled) transitions. The latter include child-birth (for females), military service (for males), self-supported withdrawal from the labor force, emigration, and death. Spells with these outcomes are right-censored. Another important point to note is that employment obtained after a period of unemployment is fragile; 41 percent of the employment spells are terminated within two years of employment, and 43 percent of these employment terminations lead directly back to the unemployment pool. Mean monthly earnings for those

[1] Note that it is the length of the employment status that we model. Switches between different jobs are disregarded.

who get a job are around 26,000 NOK (4,000 $). The variation is large, however, with a standard deviation around 60 percent.

Table 2
Overview of events/outcomes recorded in the data

Number of unemployment spells	608,126
Percent of unemployment spells completed before the end of the observation period	94.21
Mean duration of completed spells (months)	5.23
Percent of unemployment spells ending in:	
Employment	46.59
Education	16.87
Other benefit (sickness, rehabilitation, disability, or social assistance)	16.71
Other (right censored transitions)	19.83
Percent of completed unemployment spells involving ALMP	17.12
Percent of employment spells completed within two years	41.13
Percent of completed employment spells ending in unemployment	43.03
Mean monthly earnings from employment in the first months after unemployment (2006 NOK)	26,292
Standard deviation log monthly employment earnings	0.602

Since we observe labor market status by the end of each calendar month only, we set up the statistical model directly in terms of grouped hazard rates (Prentice and Gloeckler, 1978; Meyer, 1990). We write the integrated period-specific hazard rate associated with destination state k for individual i in month t, φ_{kit}, as functions of observed (time-varying) variables and unknown parameters represented by index functions f_{kit}, and (time-invariant) unobserved individual characteristics v_{ki}:

$$\varphi_{kit} = \int_{t-1}^{t} \theta_{kis} ds = \exp\left(f_{kit} + v_{ki}\right), \quad k = 1,...,5, \tag{1}$$

where θ_{kis} is the underlying continuous-time hazard rate, assumed to be constant within each month t. In addition, we specify monthly earnings at the start of the new job as

$$w_{it} = \exp\left(f_{6it} + v_{6i} + \varepsilon_i\right), \tag{2}$$

where f_{6it} is an index function of observed explanatory variables, v_{6i} is an unobserved individual characteristic, and ε_i is an error term reflecting genuine randomness in earnings outcomes at the individual level. The latter is assumed to be normally distributed with mean zero and variance σ^2. We write the index functions for the transitions from unemployment as follows:

$$f_{kit} = \tau_{kt} s_{it} + \lambda_{kd} d_{it} + \lambda_k^* \log(d_{it}^{scal}) c_{it} + \delta_k r_{it} + \alpha_{kit} z_{it} + \beta_k x_{it}, \quad k = 1,...,4, \tag{3}$$

where s_{it} is a vector of calendar month dummy variables (one for each calendar month in our data), d_{it} is a vector of spell duration dummy variables (including a representation of lagged duration from recent previous spells), d_{it}^{scal} is a spell duration *scalar* variable, c_{it} is a monthly business cycle indicator (see Figure 1, Section 2), r_{it} is a vector of dummy variables reflecting UI status/regime and the length of remaining UI entitlements, z_{it} is a vector of dummy variables recording already realized endogenous events (on-going and completed treatment and outcome of previous unemployment spells), and x_{it} is a vector of individual characteristics (age, education, work-experience, previous income, the level of UI benefits, family status, nationality, and business cycle conditions at the time of first entry).[2] Note that the effects of endogenous events (α_{kit}) vary over individuals as well as time. The reason for this is that we allow the causal effects of ALMP to depend on some key individual characteristics (gender and education), on the duration of ongoing and completed treatment, and on the current business cycle conditions. The impacts of spell duration are to some extent allowed to vary over the business cycle through the interaction of spell duration ($\log(d_{it}^{scal})$) with business cycle conditions (c_{it}). The parameters associated with the spell duration dummy variables (λ_{kd}) reflect the impact of having been unemployed for d months under "normal" cyclical conditions $(c_{it} = 0)$.

The index function for the transition from employment is written as:

$$f_{5it} = \lambda_5 \overline{d}_i + \delta_5 \overline{r}_i + \lambda_5^* d_{it}^* + \alpha_{5it} \overline{z}_i + \psi_5 \ln \overline{w}_i + \tau_5 c_t + \beta_5 x_{it}, \tag{4}$$

where $\overline{d}_i$ is the duration of the completed job search period, $\overline{r}_i$ reflects the remaining UI entitlement at the time of the job transition, d_{it}^* is the duration of the ongoing employment spell, $\overline{z}_i$ is a vector of indicators for realized treatment and part-time work, and $\overline{w}_i$ is the the realized level of monthly earnings.

The index function for monthly earnings is written as

$$f_{6it} = \lambda_6 \overline{d}_i + \delta_6 \overline{r}_i + \alpha_{6it} \overline{z}_{\boldsymbol{i}} + \tau_6 c_t + \beta_6 x_{it}, \tag{5}$$

where t here refers to the month of transition into employment.

A point to note is that all the variables explaining expected earnings (5) are also assumed to have direct effects on the various hazard rates. Hence, given the unrestricted correla-

[2] The business cycle condition at the time of first entry is included as an individual covariate to capture the potential sorting in the inflow to unemployment over the cycle.

tion between unobserved covariates, the level of expected earnings is implicitly included in all the hazard rates.

Due to the large number of modeled events and the strategy of representing most variables nonparametrically (i.e., with a dummy for each possible value), the model contains more than 1,500 unknown parameters attached to observed explanatory variables. We have chosen such a comprehensive model not because we aspire to provide a complete assessment of the job search process in a single research paper, but rather because we have found that the particular causal mechanisms we seek to recover are more reliably identified the better we are able to control for other potentially related mechanisms. In particular, in order to correctly recover the impacts of job search duration on the employment hazard and on job match quality, it is essential to control for the other time-dimensions represented in the data (calendar time and UI exhaustion) without relying on (arbitrary) functional form restrictions. We have also found that modeling the transitions to education and other benefits contributes to the identification of unobserved heterogeneity in the employment hazard and in the job match quality outcomes. To assume that these transitions result from an exogenous right-censoring process would simply represent a serious misspecification of the model.

3.2 Identification

Even though we apply a (semi) proportional hazard rate model, we emphasize that nonparametric identification does *not* rely solely on the proportionality assumption.[3] Additional sources of identification are the existence of repeat spells (Abbring and Van den Berg, 2003) and, more importantly, the abundance of exogenous time-varying covariates (McCall, 1994; Brinch, 2007; Gaure *et al.*, 2007). Of particular value for identification purposes is the substantial calendar time variation in both labor market tightness and in the scale of labor market programs; see Section 2. As pointed out 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 outcomes only through the already realized selection process. Hence, they facilitate the disentanglement of causal treatment and duration effects from impacts of unobserved sorting. Note that we do *not* require the calendar time variation in the treatment propensity to be independent of the cyclical variation in, e.g., the employment hazard. Since we include a full set of calendar time dummy variables in all hazard rates, the non-independent

[3] Note that since the semi proportional model is fully proportional in unobserved heterogeneity, existing identification results based on the mixed proportional hazard structure hold even for our model.

variation is fully (nonparametrically) controlled for in the model. The identification of treatment effects also relies on the "no anticipation assumption" (Abbring and Van den Berg, 2003), requiring that individuals do not anticipate the realization of the stochastic process determining treatment events. Since treatments are typically implemented quickly once the relevant decision is made, we view this assumption as defensible. Note that the no anticipation assumption does *not* rule out behavioral responses towards an approaching "threat" of activation insofar as this threat is captured by the systematic part of the model. Agents may anticipate a modeled rise in the treatment probability due to, e.g., imminent UI exhaustion, but they are assumed to have no private information ex ante regarding their own treatment outcome.

Our data make it possible to identify separately the degree of intrinsic duration dependence related to discouragement and/or statistical discrimination and the impact of UI exhaustion. An important source of identification for these parameters is the 1997 UI reform, which introduced an exogenous break in the otherwise strong positive correlation between unemployment duration and UI exhaustion (see Section 2). Participation in ALMP also contributes to the separation of duration and UI exhaustion effects, since many participants do not draw on their UI entitlements while participating in a program activity.

The earnings equation represents a potential identification problem, since unobserved characteristics affecting job search duration are unlikely to be independent of unobserved characteristics affecting expected earnings. This may generate a spurious relationship between job search duration and realized earnings. We are not aware of any formal identification results that can be called upon to claim nonparametric identification of these distinct causal mechanisms. Recall, however, that the distribution of unobserved characteristics directly affecting job search duration (v_1, v_2, v_3, v_4) is identified through the event history part of the model. Intuitively, their correlation with the unobserved earnings potential (v_6) can then be traced out through the observed distribution of realized earnings *conditional on the duration of job search* (and other explanatory variables). In addition, the large fraction of individuals with multiple job search spells in our data window (33.3 percent) implies that many individuals are observed with different job matches that were accepted at different durations of the job search period. This adds a sort of fixed-effects-type foundation for the nonparametric identification of the earnings equation.

3.3 The likelihood function

Let K_{it} be the set of feasible events for individual i in month t, i.e., $K_{it} = \{1,2,3,4\}$ when openly unemployed, $K_{it} = \{1,2,3\}$ when participating in ALMP, and $K_{it} = \{5\}$ when employed. Let y_{kit}, $k=1,...,5,$ 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, let w_{it} be observed initial earnings for individual i who made an employment transition at time t, and let Y_i be the complete set of outcome indicators available for individual i (potentially collected from multiple spells with multiple earnings observations). The contribution to the likelihood function formed by the event pattern of a particular individual, conditional on the vector of unobserved variables $v_i = (v_{1i}, v_{2i}, v_{3i}, v_{4i}, v_{5i}, v_{6i})$ can then be formulated as:

$$P_i(v_i) = \prod_{y_{kit} \in Y_i} \left[\begin{array}{l} \prod\limits_{k \in K_{it}} \left[\left[\left(1 - \exp\left(-\sum\limits_{k \in K_{it}} \exp(f_{kit} + v_{ki}) \right) \right) \dfrac{\exp(f_{kit} + v_{ki})}{\sum\limits_{k \in K_{it}} \exp(f_{kit} + v_{ki})} \right]^{y_{kit}} \right] \\ \times \left[\exp\left(-\sum\limits_{k \in K_{it}} \exp(f_{kit} + v_{ki}) \right) \right]^{\left(1 - \sum\limits_{k \in K_{it}} y_{kit}\right)} \\ \times \left[\dfrac{1}{\sigma\sqrt{2\pi}} \exp\left(-\dfrac{(\ln w_{it} - f_{6it} - v_{6i})^2}{2\sigma^2} \right) \right]^{y_{1it}} \end{array} \right]. \quad (6)$$

In order to arrive at the marginal likelihood, we need to integrate the six-dimensional vector of unobserved heterogeneity v_i out of Equation (6). Standard techniques for doing this rest on the assumption that the unobserved covariates are orthogonal to all other explanatory variables in the model at the time of first entry. However, for interval censored data of the type used here, this assumption is violated. The reason for this is that the interval censoring creates a left-truncation problem, i.e., some individuals with only very short spells - those starting and ending in the same month – are never recorded. Consequently, we have a selected sample, in which unobserved heterogeneity cannot be assumed independent of either observed covariates or calendar time, since the impact of unobserved heterogeneity during the first (censored) month depends on the values of all other explanatory variables. The solution to this problem is to set up the likelihood function conditional on the first spell surviving to the first observation point, and use Bayes' theorem to derive the appropriate distribution of unobserved heterogeneity. We assume that the entries to the origin state are uniformly distributed within each calendar month. Let $\bar{t}_{1i}$ be the first inflow month of the first spell for individual *i*. The probability of surviving the inflow month – i.e., of being included in the analysis population – is then equal to

$$S_i(v_i) = \frac{1-\exp\left(-\sum_{k\in K_{i\bar{t}_{1i}}} \exp\left(f_{ki\bar{t}_{1i}} + v_{ki}\right)\right)}{\sum_{k\in K_{i\bar{t}_{1i}}} \exp\left(f_{ki\bar{t}_{1i}} + v_{ki}\right)} \,. \tag{7}$$

If $f(v_i)$ denotes the unconditional heterogeneity density function (at the time of first entry into unemployment) it follows from Bayes' theorem that

$$f(v_i \mid \text{survival of entry month}) = \frac{S_i(v_i)}{E\left[S_i(v_i)\right]} f(v_i)\,. \tag{8}$$

To ensure that our estimation results to the largest possible extent are driven by the data and not by unjustified restrictions on the heterogeneity distribution, we introduce unobserved heterogeneity nonparametrically by means of the nonparametric maximum-likelihood estimator (NPMLE). In practice, this implies that the vectors of unobserved attributes are jointly discretely distributed (Lindsay, 1983) with the number of mass-points chosen by adding location vectors until it is no longer possible to increase the likelihood function (Heckman and Singer, 1984). Assuming that the unobserved covariates are jointly discretely distributed with Q number of support points, we can write the data likelihood function as

$$L = \prod_{i=1}^{N} \frac{\sum_{l=1}^{Q} q_l \left(P_i\left(v_l\right) S_i(v_l)\right)}{\sum_{l=1}^{Q} q_l S_i(v_l)}, \quad \sum_{l=1}^{Q} q_l = 1, \tag{9}$$

where $\{v_l, q_l\}$, $l = 1,2,...Q$, are the location vectors and probabilities characterizing the heterogeneity distribution, and the functions $P_i(.)$, $S_i(.)$ are defined in (6) and (7), respectively.

3.4 The Optimization algorithm

Given that we seek to select the number of support points (Q) to arrive at the largest possible likelihood, optimization of the likelihood function in (9) is not a trivial exercise. In the existing literature, most applications based on discrete mixture models either rely on a pre-specified (typically very low) number of support points or on a stopping rule based on computational capabilities rather than on a properly verified maximum likelihood criterion. Gaure *et al.* (2007) show that such ad-hoc procedures may lead to seriously biased estimates. We therefore do seek to locate the genuine nonparametric maximum likelihood estimators in the present application.

The algorithm we use starts out estimating a null-model without unobserved heterogeneity (Q=1), and then expands the model step by step with one additional support point in each round. Each time, we identify a candidate for a new support point by assigning a new point with probability zero and select its location vector such that the derivative in the direction of positive probability is positive. For this we use a simulated annealing approach. We then maximize in three steps; first with respect to the probabilities, then with respect to the entire heterogeneity distribution, and finally with respect to all parameters in the model simultaneously. For the maximizations we use a combination of BFGS, a Newton method with line-search, and a trust-region method. For the latter two we use the Fisher matrix rather than the Hessian. The Fisher matrix is relatively easy and inexpensive to compute from the individual gradients, and it is definite by construction, even in regions far away from the maximum. Standard errors are lifted from the diagonal of the inverse of the (negative) Fisher matrix. This is a quite large matrix, but it is positive definite, thus we may invert it by Cholesky-factorization, a method which has good numerical stability properties.

An important point to note is that the model outlined in Section 3.1 introduces unobserved heterogeneity in the form of type-specific *vectors*, rather than by following the standard practice of allocating mass-point locations to each outcome equation separately and then estimate the probabilities of all possible combinations of these locations. Although these 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 outcome-specific mass-point locations implies that the number of *potential* locations are added in fairly large steps (in our six-dimensional case as 1, 2^6=64, 3^6=729,…,) rather than one by one. In principle, this should perhaps be irrelevant, since it is always possible to attach the probability zero to empirically irrelevant combinations. In practice, however, the prevalence of many zero-probabilities generates insurmountable numerical problems. By contrast, our method ensures that mass-points are introduced one by one, and that the problem of zero-probabilities is typically not encountered until the likelihood no longer can be improved by adding additional points.

We estimated the model with up to 35 distinct support points. The model converged nicely, in the sense that all the parameters gradually approached their “final” values, with almost no changes occurring during the process of adding the last 10-15 support points (except in the estimated heterogeneity distribution itself). Estimated standard errors also increased gradually (and slowly) as more support points were added, suggesting that these were numerically stable and, hence, reliable as tools for statistical inference. The number of support points estimated in this application is clearly an order of magnitude larger than what has commonly been reported in the literature on mixture models. However, the only attempt to estimate a six-dimensional mixture distribution nonparametrically that we are aware of is the one provided by Røed and Westlie (2007), and their model ended up requiring as much as 41 support points. Our experience suggests that the appropriate number of support points tends to increase with the dimensionality of the heterogeneity vector and with the number of observations, while it seems to be of minor importance whether the outcomes are discrete (events) or continuous (earnings).[4] The present model was estimated on a high-performance computer cluster. With access to an average of around 150 CPU's, it took approximately 250 hours to fully implement the algorithm.

3.5 Characteristics of the chosen model

Following recommendations provided by Gaure *et al.* (2007), we used the Akaike Information Criterion (AIC) for model selection. This criterion ended up requiring 27 distinct support points, and the results presented in the next section are all based on this model. It may be noted, however, that the reported results are highly robust with respect to the exact number of support points, at least as long as the number lies somewhere between 15 and 35. From a methodological point of view, it may be of some interest to examine the estimated distribution of unobserved heterogeneity as it appears in the selected model. The probabilities are fairly evenly distributed across a large number of support points. As much as 19 of the 27 points have a probability mass above one percent, and the highest probability mass attached to a single point is 21.3 percent. Only one of the mass-points involves a defective risk (in the transition to education), and this mass-point is attributed a probability of 0.7 percent. In Table 3, we report the estimated correlation structure for the six unobserved covariates. We report rank correlation (Kendall's τ) to avoid the excess influence that low-probability extreme (and imprecisely estimated) locations would have on standard correlation measures.[5] There seems to

[4] The 41-point distribution reported by Røed and Westlie (2007) was estimated for a transition model with discrete outcomes only (no earnings equation), and on a dataset with approximately twice as many observations as in the present paper.

[5] Kendall's τ is computed on the basis of all possible pairs of individuals *(i,j)* that can be formed on the basis of the estimated heterogeneity distribution. A pair $\{(v_{ki}, v_{li}), (v_{kj}, v_{lj})\}$ said to be concordant with respect to variables *(k,l)* if $(v_{ki} - v_{kj})(v_{li} - v_{lj}) > 0$ and discordant if $(v_{ki} - v_{kj})(v_{li} - v_{lj}) < 0$. Let c_{kl} be the number of concordant pairs and let d_{kl} be the number of discordant pairs. We then compute Kendall's τ as $\tau_{kl} = (c - d)(c + d)^{-1}$. Note that we disregard the fraction $\sum_{s=1}^{Q} q_s^{\,2}$ of identical pairs drawn from the same location vector.

be a positive unobserved selection into ALMP in the sense that the treatment propensity correlates positively with employment propensity. As expected, the unobserved employment propensity also correlates positively with earnings and negatively with the employment termination propensity.

It may also be noted that unobserved heterogeneity explains a substantial fraction of earnings dispersion across individuals. From Table 2, we recall that the overall standard deviation of log earnings is 0.602. By including all observed covariates in a log-normal earnings regression, the standard deviation is reduced to 0.539 (not shown). Through the inclusion of unobserved heterogeneity, the estimated standard deviation in the person-specific log earnings distribution is further reduced to 0.386 (not shown). This nevertheless implies that each individual is subject to substantial earnings variability.

Table 3
Unobserved heterogeneity - Rank correlation (Kendall's τ)

	Education	Other benefits	ALMP	Employment termination	Log earnings
Employment	-0.043	-0.066	0.323	-0.102	0.315
Education		0.227	0.404	0.256	-0.124
Other benefits			0.025	0.546	-0.250
ALMP				0.008	0.197
Employment termination					-0.244

4. Main Results

The statistical model outlined in the previous section is comprehensive, and can be used to examine a number of issues regarding transitions from unemployment to employment as well as to the two other final destination states. The presentation of results in this section, however, is limited to the key questions outlined in the introduction, and focus on the relationship between the duration of job search and program participation on the one hand, and the speed of job transitions and the quality of job matches on the other. A more elaborate presentation of results is provided in a working paper (Gaure *et al.*, 2008), and a complete list of estimation results is posted on our website www.frisch.uio.no/docs/match_quality.html. On this site, we also provide results from a number of alternative model specifications and robustness checks, all confirming the validity of the results presented in this section.

4.1 The impacts of job search duration

As pointed out in Section 3.2, our data allow us to identify separately the impacts of job search duration and the impacts of remaining time until UI exhaustion. Figure 2 presents the estimated impacts of job search duration on the probability of obtaining an acceptable job and on the quality of that job. The graphs are normalized to unity for the first duration month and display the relative impacts of extending the job search period. While the upper graph shows the employment hazard as a function of *ongoing* job search duration (under normal cyclical conditions), the two lower graphs show realized earnings and job instability, respectively, as functions of *completed* job search duration. There is clearly negative duration dependence in the employment hazard. Other things equal, the instant probability of finding an acceptable job typically declines by around 20 percent during the first half year of job search. Recall, however, that our model also includes an interaction term between spell duration and a monthly labor market tightness indicator (see Section 2). We find that the degree of negative duration dependence in the employment hazard is stronger the tighter the labor market (not shown), indicating that stigma associated with long-term unemployment is triggered faster in good times than in bad times. Moving from the worst observed to the best observed cyclical conditions implies that the job hazard rate of a long-term unemployed (12 months) relative to that of a new entrant declines by around 3.5 percent, ceteris paribus.[6]

While the employment hazard declines with the length of job search, a longer job search period clearly pays off in terms of higher expected earnings once a job is obtained; see the lower left-hand-side panel in Figure 2. This is consistent with the notion that job search is a productive endeavor. However, there is no additional earnings gain associated with job search beyond approximately 6 months, and after 15 months the impact of lengthening the search period becomes negative. The latter finding may reflect human capital depreciation, statistical discrimination against long-term unemployment, or a reduction in reservation wages arising from learning (more realistic assessment of earnings options) or from liquidity constraints. It is also worth noting that longer job search periods do apparently not result in *safer* jobs.

[6] Unemployment experiences from previous spells are also allowed to causally affect the hazard rates out of unemployment provided that they were completed less than three years prior to the start of the ongoing spell (otherwise they are linked to the current spell only through the common vector of unobserved covariates). The impact of unemployment experience from previous spells on current hazard rates depend on the outcome of those spells. We do not report these results here, except noting that past short unemployment spells (less than 12 months) with successful outcomes (in the sense that they ended with a job) have negligible effects on the outcome of subsequent spells. Longer previous spells, and spells without a successful outcome, have more adverse effects on the outcome of subsequent spells, particularly if the spells are close in time.

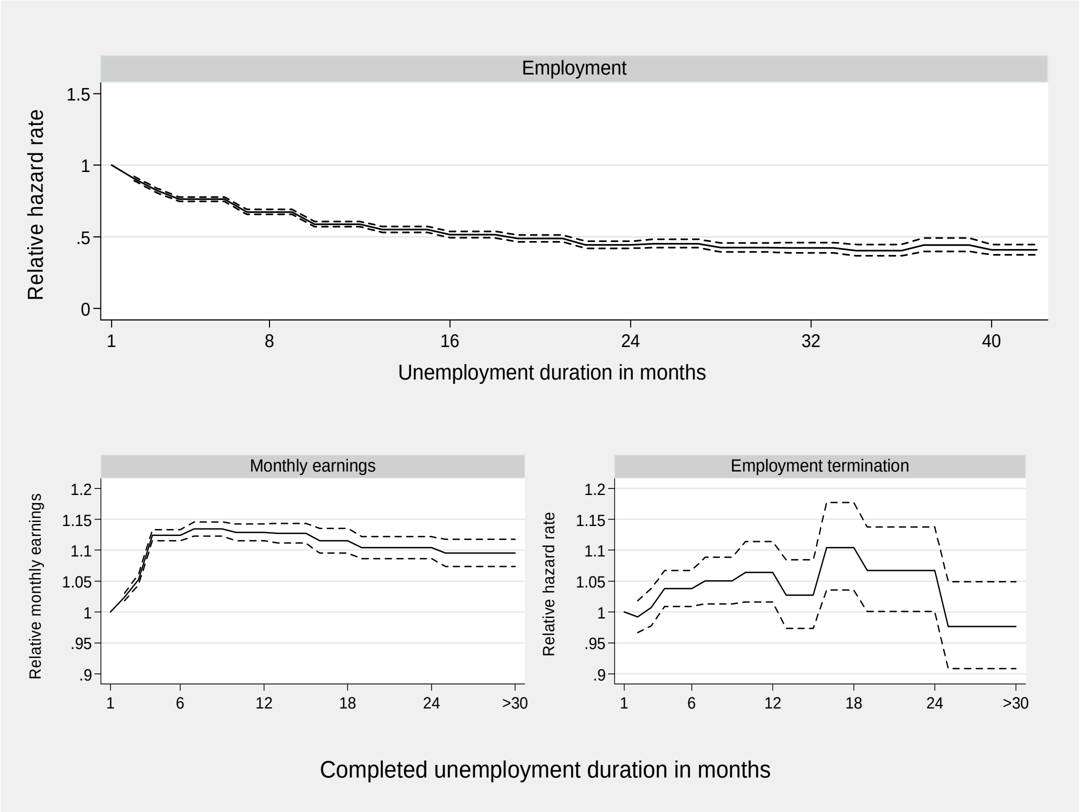

Figure 2. The estimated effect of job search duration on the employment hazard (upper panel) and on the quality of an accepted job match (lower panels).
Note: All effects are normalized on the first month and reflect relative changes in hazard rates or earnings as duration increases, ceteris paribus.

4.2 The impacts of UI exhaustion

The estimated impacts of UI exhaustion are displayed in Figure 3. The upper panel shows the estimated shape of the employment hazard in the run-up to UI exhaustion and afterwards, relative a situation with more than six months left of the insurance period. The employment hazard rises with approximately 50 percent during the very last month of the entitlement period, and it remains at the higher level after UI have been exhausted. As indicated by the lower left-hand-side panel, jobs accepted close to UI exhaustion are associated with significantly lower earnings than those accepted earlier in the job search period. For jobs accepted during the last two months of the UI period, the earnings loss (compared to a situation with more than six months left) is close to 10 percent. This indicates that the reservation wage indeed declines significantly as UI entitlements are exhausted. However, jobs accepted *after* UI exhaustion are again associated with somewhat higher earnings than jobs accepted in the run-up to exhaustion. A possible interpretation of this finding is that some UI claimants postpone the acceptance of available, but poorly salaried job offers until the option of unrestricted subsidized job search is no longer available, and that these claimants are sorted out of the unemployment pool during the UI exhaustion period.

We do not find any significant effects of UI exhaustion on the stability of accepted job matches; see the lower right-hand-side panel of Figure 3. We do identify, however, a small tradeoff between the accepted earnings level and job stability (not shown in the graph). The elasticity of the employment termination hazard with respect to the earnings level is estimated to 0.254 (with standard error 0.016), i.e., a 10 percent increase in monthly earnings implies a 2.5 percent increase in the job termination hazard, ceteris paribus. Thus, higher earnings (conditional on human capital variables) to some extent seem to compensate for insecure jobs.

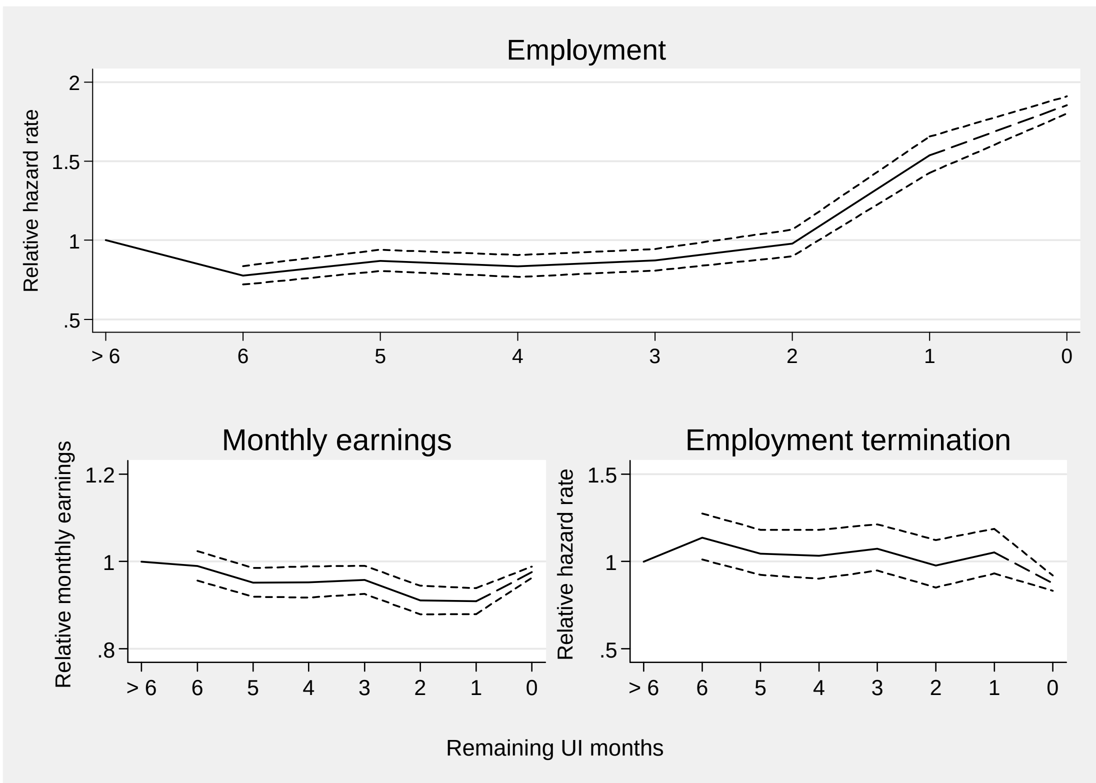

Figure 3. The estimated effect of UI exhaustion on the employment hazard (upper panel) and on the quality of an accepted job match (lower panels).
Note: All effects are normalized on a situation with more than six months left of the UI period and reflect relative changes in hazard rates or earnings as UI approaches exhaustion, ceteris paribus..

4.3 The impacts of ALMP

The estimated direct impacts of ALMP participation are presented in Table 4. For both on-program and post-program effects we first report the results for a reference participant (defined at the bottom of the table). For each combination of explanatory variables, the ALMP effects on the hazard rates can be computed as the appropriate product of variables and parameter estimates reported in the subsequent rows.[7] A key finding is that ALMP participation reduces the employment hazard sharply during the initial stages of participation (lock-in effect), but that the effect gradually becomes less negative as the treatment is continued; see Column I of Table 6. For a typical participant, the employment effect becomes positive after around 6 months of participation. ALMP also raises the employment hazard after completion of the program, compared to the pre-participation period (post-program effect). A general finding is that the favorable effects of ALMP are largest for men and for persons with high education. The effects are also more favorable in a tight than in a slack labor market. The finding of a more favorable treatment effect the higher the educational attainment contrasts with the previously reported negative interaction effect reported by Røed and Raaum (2006). However, their analysis was limited to insured unemployment spells, and all exits from unemployment were aggregated into a single destination state.

Table 4
Effects of ALMP participation on hazard rates during the participation period and afterwards

	I Employment hazard		II Log monthly earnings		III Employment termination hazard	
	Est.	S.E.	Est.	S.E.	Est.	S.E.
On-program effect for reference participant	-0.363	0.014				
+ deviation from 4 month ongoing program duration (ln(duration)-ln(4))	0.805	0.010				
+ male	0.117	0.015				
+ deviation from mean education (years)	0.045	0.004				
+ deviation from mean cyclical conditions	0.323	0.057				
Post-program effect for reference participant	0.196	0.017	0.040	0.005	0.051	0.019
+ deviation from 4 month completed program duration (ln(duration)-ln(4))	0.174	0.012	0.070	0.003	-0.138	0.012
+ male	0.014	0.020	-0.019	0.005	-0.074	0.019
+ deviation from mean education (years)	0.027	0.006	0.006	0.001	-0.016	0.006
+ deviation from mean cyclical conditions	0.153	0.077	0.030	0.022	-0.025	0.068

Reference: female participant, 4 months program duration, 12 years education, and “normal” business cycle conditions.

Participation in ALMP also affects the expected quality of a subsequent job; see Columns II and III of Table 4. We find that very short ALMP’s tend to have a negative impact on both earnings and job stability. For a typical worker, the earnings effect varies from minus five percent for very short programs (one month) to plus 10 percent for long programs (nine months). Longer programs also tend to improve job stability, with a reduction in the job ter-

[7] For example, for a reference (female) participant we have that the estimated on-program effect in the employment hazard after six months is equal to -0.363+0.805*((ln(6)-ln(4))=-0.0366. The resultant proportional shift in the hazard rate is equal to exp(-0.0366)=0.964.

mination hazard of around five percent, ceteris paribus. As discussed in Section 3.1, we cannot rule out that differential results *by treatment duration* reflect sorting into programs of different lengths, which is unaccounted for in the model.

In order to evaluate the overall impact of ALMP, we perform simulation exercises with and without the effects of treatment included in the model. Each entrant is equipped with his/her true observed characteristics at the time of entry into unemployment, and a vector of unobserved intercepts is drawn from the estimated heterogeneity distribution. We then compute each job seeker's transition probabilities and perform sequences of transition lotteries (based on random number generators). We first simulate outcomes (in terms of unemployment duration, destination state, and – if the destination state is employment – earnings and employment duration) on the basis of the estimated model. We then repeat the simulation exercise on the basis of the assumption that ALMP is completely irrelevant, in the sense that the impacts on all final destination hazards are set to zero. We allow job seekers to participate in programs even in this simulation, however. This implies that we identify the group of participants even in the "no-treatment world", based on exactly the same sorting process as in the treatment world.[8] We can therefore examine the performance of treated individuals both with and without actual treatment. It also implies that we can characterize the sorting process into treatment. We first restrict attention to the outcomes of each individual's first unemployment spell, since the occurrence of repeat spells are only partly modeled. In the simulation exercises, we keep business cycles and other time-varying covariates constant (at their mean levels), implying that we can eliminate the right-censoring problem present in the real data (we follow all spells for up to five years, even if they stretch beyond our data window). In order to obtain confidence intervals for our simulation results, we use a parametric bootstrap procedure, i.e., we draw parameter estimates repeatedly from their joint normal distribution.[9] In total, we make 120 simulations under each regime, and calculate 95 percent confidence intervals for the statistics of interest.

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[8] Note that all job seekers are potential participants in this model. The actual participants are those who do not make a transition to one of the final destination states before they become treated.

[9] We perform repeated drawings form the parameters attached to observed explanatory variables only, since heterogeneity parameters are not normally distributed; see Gaure *et al.* (2007). The drawings of parameter estimates are made by means of the Cholesky decomposition; *i.e.*, let L be a lower triangular matrix, such that the estimated covariance matrix is $V = LL'$. Let z_s be a vector of drawings from the standard normal distribution collected for trial s. Let $\hat{b}$ be the vector of point-estimates. The parameters drawn for trial s are then given as $b_s = \hat{b} + Lz_s$.

Table 5
Overall effects of ALMP participation

	I	*II*	*III*	*IV*
	Non-participants	*Participants without ALMP*	*Participants with ALMP*	*Effect of ALMP (III-II) [95% CI in brackets]*
Outcomes of the first unemployment spell				
Percent of unemployment spells ending in				
Employment	55.69	47.25	49.32	2.07 [1.46, 2.79]
Education	25.72	25.10	23.52	-1.58 [-2.15, -0.93]
Other benefit	18.16	25.03	24.98	-0.05 [-0.70, 0.60]
Censored due to end of observation period	0.42	2.62	2.18	-0.43 [-0.63, -0.25]
Mean duration of unemployment spells	5.19	13.95	15.18	1.23 [1.04, 1.41]
Share of population	84.31	15.69	15.69	0.00 [-0.16, 0.19]
Outcomes of the first employment spell				
Mean monthly earnings first employment spell (NOK)	27,967	25,265	25,908	642 [288, 1,043]
Percent of employment spells terminated within first year after employment transition	29.63	35.12	36.54	1.42 [0.40, 2.58]
Overall earnings and costs first five years after entry into unemployment				
A. Total mean earnings generated per participant in ordinary (non-subsidized) jobs (NOK)	1,056,245	700,667	689,739	-10,928 [-20,851, 371]
Share of population	73.45	26.55	26.55	0.00 [-0.20, 0.18].
Mean number of months in ALMP per participant	-	-	5.73	
Mean number of months in unemployment (including ALMP participation)	9.85	19.33	20.47	1.14 [0.99, 1.23]
Mean number of months in ordinary employment	38.39	27.72	26.73	-0.99 [-1.21, -0.71]
B. Total mean economic value generated through program participation (subsidized jobs) per participant (NOK)		0	45,130	45,130
C. Total mean operating cost of ALMP per participant (NOK)		0	21,086	21,086
D. ALMP net surplus per participant (A+B-C) (NOK)				13,116 [3,193,24,415]

Note: Sum earnings are calculated on the basis of the assumption that the earnings levels remain constant within employment spell. All amounts are reported in 2008-value. Effect measures *per participant* (Column III) are calculated by dividing the difference between the ALMP and the non-ALMP worlds on the fraction of actual participants in the world with ALMP. Outcomes for ALMP participants in the non-ALMP world are computed by subtracting the effect (Column III) from the outcome with ALMP (Column I).

The results are provided in Table 5. The first two columns summarize the outcomes for non-participants and participants *in the absence of any treatment effects*; hence, the differences between these two columns are due to *sorting into ALMP only*. The results indicate that there is strong negative selection into ALMP. The likelihood of ending up in employment is on average 8.4 percentage points higher for non-participants than for participants, and their earnings are around 11 percent higher, given that they do find a job. Non-participants' unemployment spells are also on average almost 9 months shorter than those of participants, but this primarily reflects that the participation probability rises with the time at risk. The causal impacts of ALMP are assessed by comparing the outcomes for the group of participants in the treatment and the non-treatment worlds, see Columns II-IV. They show that program participation increases the probability that a job search period ends with a job by approximately 2 percentage points. It also increases the level of participants' realized monthly earnings by around 640 NOK (100 $), or 2.5 percent (all monetary impacts are measured in 2008 value, with nominal wage growth used as deflator). However, these favorable effects come at the cost of an increase in expected unemployment duration (including the participation period) of around 1.2 months, or around 9 percent.

In order to compare program benefits with program costs over a longer period of time, we simulate the progression of unemployment and employment spells for a full five-year period after entry into unemployment. In this exercise we also include repeat spells. Repeat spells start endogenously whenever a job termination is simulated. Individuals making transitions to education or other benefits are allowed to return to unemployment later on according to drawings from lotteries based on observed return-frequencies from these states. A simple measure of the overall program effect is obtained by adding up all earnings generated from ordinary employment in the treatment and no-treatment worlds, respectively. This exercise indicates that over a five-year period, the adverse treatment effects (longer unemployment durations) dominate the favorable effects (higher employment and higher monthly earnings).[10] However, some ALMP's clearly involve work of direct economic value. Around 60 percent of the program activities involves employment in which presumably useful work is carried out, and the economic value of this work should be included in a cost-benefit evaluation; see Jespersen *et al.* (2008). It is of course difficult to assess this value, but since the program provider does not face the total wage cost, it is probably well below the participants' full earnings potentials. Wage subsidies to private sector jobs are typically limited to a maximum of 50 percent of the wage bill, suggesting that the value of the work is likely to exceed 50 percent of actual earnings for these jobs. For work training schemes, the subsidy may be as high as 100 percent. The calculations provided in Table 5 are based on the assumption that subsidized

[10] We have for simplicity assumed that the real wage growth rate equals the discount rate over the relevant five-year period, so that these two factors cancel out in the net present value calculations.

work on average is worth 50 percent of the earnings level predicted for non-subsidized work. ALMP also involves administrative costs. Cost assessments made by the Public Employment Service (PES) suggest that the mean cost of providing ALMP in Norway – excluding all transfers to the participant – amounts to 3,620 NOK (550 \$) per month.[11] Taking both the value of work within programs and the administrative costs of providing them into account, a simple comparison of costs and benefits during a five year period (upon entering unemployment) suggests that the programs are cost-effective. However, this conclusion is highly sensitive to the valuation of work carried out within employment programs. For the cost-benefit analysis to yield a positive result, this value must on average exceed around 35 percent of expected earnings in non-subsidized jobs.

The existence of activation requirements may clearly affect job search behavior even in the absence of actual participation, e.g., through the so-called "threat effect" (Black *et al.*, 2003). Effective time-limits on unemployment insurance payments are in Norway intimately related to the welfare state's ability to offer paid activation instead. As explained in Section 2, the Norwegian 1997 UI reform implied a significant reduction in the use of labor market programs resulting from the extension of the maximum duration by which UI benefits could be claimed without activity requirements. Empirically, it is difficult to separate the impacts of this reform from the impacts of changes in the macroeconomic environment. However, if we consider job seekers without UI entitlement as a sort of control group – assuming that these job seekers were subject to the same calendar time effects in their employment hazards as the UI claimants – we find that the reform indeed caused as much as a 17 per cent drop in the claimants' employment hazard and a 5 per cent increase in the level of their accepted earnings, ceteris paribus.[12] Although we cannot rule out that this estimate also captures other (cyclical) mechanisms that may have changed the outcomes for UI-claimants relative to those for non-claimants (the reform almost coincided with a cyclical peak; see Figure 1), we interpret the large estimated effects as fairly convincing "circumstantial" evidence that activation requirements do encourage active job search and/or reduce reservation wages, and hence that the "threat effect" is empirically important in Norway.

[11] The average monthly operating cost is stipulated to 662 NOK for work training and 8,208 for classroom training. Work training amount to 60.8 percent of all programs in our data window which makes the average cost 662*0.608+8208*0.392 = 3,620 NOK per month.

[12] We do this by including a separate reform-dummy in the model which is equal to one for all UI claimants entering unemployment after January 1997, and otherwise zero. Since the monthly time dummy variables that are also included in the model are assumed to have the same impacts on claimants and non-claimants, the coefficient on the reform-dummy is identified by the differential time-effects for these two groups (before/after reform).

5. Conclusion

An important feature of a UI system is the maximum duration by which job seekers are allowed to claim benefits without being forced into some form of activity. We have found that the determination of this parameter involves a number of tradeoffs. It is clearly the case that the longer benefits can be claimed without activity requirements, the higher the reservation wage and longer the time a typical job seeker uses to find a job. However, this is not only waste of time. Job search turns out to be a productive activity, and expected earnings derived from the first job match increase with as much as 13 percent during the first half year of job search. Moreover, generous job search conditions imply that fewer job search spells are terminated without a job being found at all. Fastidiousness declines significantly during the months just prior to UI exhaustion. This is mirrored in a 50 percent rise in the job hazard as well as in a 10 percent decline in the level of accepted earnings, ceteris paribus.

Participation in labor market programs also involves some conflicting mechanisms. There is initially an adverse unemployment lock-in effect that needs to be traded off against the apparently favorable human capital effects that come into play when the program has lasted for some time and/or is completed. On average, our findings suggest that program participation causes a 1.2 month increase in overall unemployment duration (including the participation period). However, it also causes a 2 percentage point increase in the probability that the unemployment spell eventually ends with a job. Program participation also tends to improve the quality of the job match. On average, program participation yields an earnings bonus of around 2.5 percent. Nevertheless, in terms of total earnings generated during the five year period after entry into unemployment, we find that the adverse unemployment duration effect dominates the favorable employment and earnings effects. In addition, programs are costly to administer. Hence, if we consider the time spent in program as being without economic value other than through the earnings it potentially generates later on, a cost-benefit calculation is bound to conclude that the programs are not worth their price. However, many programs (around 60 percent) involve some form of subsidized employment. If we assume that subsidized work has an economic value of at least 35 percent of non-subsidized work and abstract from general equilibrium effects, the cost-benefit analysis over a five-year period comes out with a favorable conclusion.

We conclude that in order to justify the high level of labor market program activity in Norway one cannot focus exclusively on programs as a means to promote the participants' human capital and later employment careers. The most important benefits of ALMP's actually seem to come from two other sources. First, they offset the moral hazard problems embedded in unemployment insurance systems. Activity requirements effectively reduce the leisure associated with being a UI claimant and, hence, encourages active job search and discourages excessive “choosiness”. Although we have shown in this paper that the latter of these effects implies a reduction in the level of accepted earnings from the first job, a quick entry into ordinary employment may provide a stepping stone towards better paid jobs. Second, active programs represent an alternative way of exploiting the “waiting time” until an ordinary job can be found. Many program participants contribute directly to the production of valuable goods and services, and a short increase in overall unemployment duration (including the participation period) may be considered a price worth paying for this benefit.

Even though the analysis provided by this paper incorporates a number of potential program effects that to our knowledge have never previously been examined simultaneously in the empirical literature, our contribution clearly falls short of full welfare analysis. In order to provide a comprehensive assessment of Norwegian active labor market policies, a number of 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 ALMP affects the wage formation in the economy. The existence of ALMP 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.

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