13 June 2008

The Impacts of Labor Market Policies on Job Search Behavior and Post-Unemployment Job Quality

Simen Gaure, Knut Røed, Lars Westlie*

Abstract

We examine empirically the impacts of labor market policies – in terms of unemployment insurance (UI) and active labor market programs (ALMP) – on the duration and outcome of job search and on the quality of a subsequent job. We find that time invested in job search tends to pay off in the form of higher earnings once a job match is formed. More generous UI raises expected unemployment duration, while improving the quality of the resultant job. Participation in ALMP raises the probability of finding a job and the level of expected earnings, but at the cost of lengthening job search.

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 and participants at the ESOP research seminar for valuable comments.

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 endogenous 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 in the UI literature is that higher UI replacement ratios yield longer unemployment durations, and that the probability of escaping unemployment increases as UI entitlements are exhausted. The empirical evidence is less abundant when it comes to the potential impacts of UI on the quality of the resultant job match. Economic theory suggests that UI may encourage job seekers to wait for – and firms to create – 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 poor 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; see, e.g., Addison and Blackburn (2000), Belzil (2001), and Van Ours and Vodopivec (2008).

The empirical literature regarding effects of ALMP is huge, but also somewhat confusing; see Kluve (2006) and Kluve *et al.* (2007) for recent overviews and meta-analyses. One source of confusion is that ALMP not only affects actual participants, but also potential participants through anticipation or “threat” effects (Black *et al.*, 2003), as well as the population at large through various general equilibrium effects. But even when it comes to the participants’ direct causal effects of ALMP, the lack of consensus among researchers is conspicuous. One reason for this is that ALMP participation involves a series of (potentially conflicting) impacts – on search effort, fastidiousness, stigma, and human capital – which play out differently over time. For example, we may expect that the job-finding rate declines during the participation period (lock-in effect), while it increases afterwards (post-treatment effect). We may also hypothesize that completed program participation improves human capital and thereby the quality of subsequent job matches in terms of earnings and job security. The large variation in reported treatment effects may then simply reflect that different evaluation schemes blend these mechanisms differently. To our knowledge, no empirical analysis has yet attempted to examine all the causal impacts of ALMP participation within a unified modeling framework that facilitates an evaluation of overall treatment effects. One aim of the present paper is to fill this gap. In addition, we exploit a major policy shift in the Norwegian UI system’s emphasis on activity requirements to assess the overall impacts of activity requirements.

Based on the timing-of-events approach (Abbring and Van den Berg, 2003), we set up a combined multivariate hazards and earnings model to analyze the transition from unemployment to three alternative destination states: i) employment, ii) ordinary education and iii) non-participation (with social security benefits). During the unemployment spell the job seekers are non-randomly sorted into ALMP. We examine the causal impacts of job search conditions and of actual 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 non-parametric 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, given unemployment duration, an increase in the maximum duration of UI benefits causes reservation wages to increase. As a result, it also causes expected unemployment duration and realized earnings to increase. Third, 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 5-6 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 of actual participation 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 discusses alternative model specifications and robustness issues, and Section 6 concludes.

2. Data and institutional background

The data used in this paper encompass 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 according to the number of spells in data window	
One unemployment spell only	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. While the old UI system offered an initial maximum UI duration of 80 weeks, in some cases allowing for an additional period at a somewhat reduced benefit level (also of 80 weeks maximum duration) if no employment or suitable ALMP activities could be found, the new UI system offered an uninterrupted UI period of 156 weeks for most job seekers; see Røed and Westlie (2007) for details. As we explain in the next section, these exogenous changes in the job search environment are crucial for identification of some of the key parameters in our statistical model.

The 1997 UI reform implied a transition from a relatively activity-oriented to a more benefit-oriented UI system, which largely explains the decline in ALMP participation shown in Figure 1. This reflects an intimate structural relationship between UI design and the usage of ALMP in Norway, arising from the fact that there exists – for all practical purposes – a lower bound on the income or utility level that can be offered to unemployed job seekers, regardless of their observed search behavior; see Pavoni (2007) for a discussion of this assumption and its consequences for optimal insurance design. Consequently, UI exhaustion does not automatically imply the termination of all economic support. Paid activation then stands out as way of ensuring a minimum living standard while minimizing moral hazard problems.

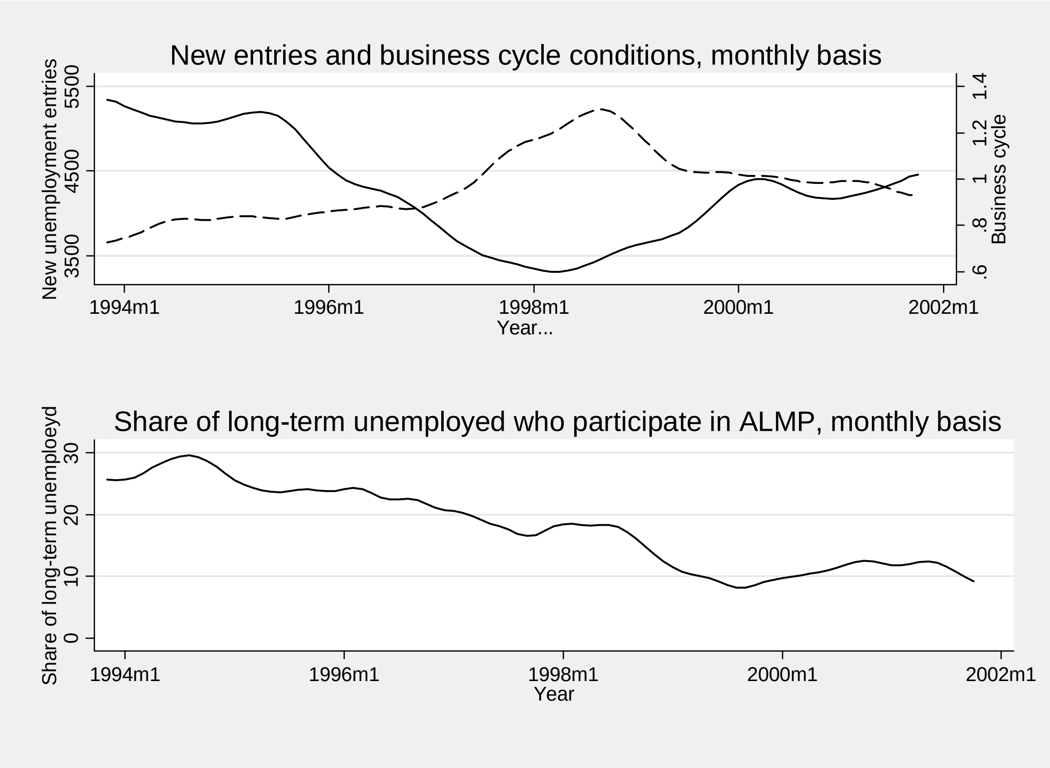

Figure 1. Labor market tightness (dotted line) and the number of new entrants (solid line) to unemployment (upper panel) and the share of long term unemployed participating in labor market programs (lower panel).

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.

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 a non-random event, and it is assumed to induce shifts in all hazard rates, both during the participation period and afterwards. The sizes of the shifts may depend on gender, initial human capital, program duration, and business cycle conditions. 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 UI status, as reflected by initial as well as currently remaining UI entitlements. For individuals who make a transition to employment, it is assumed that the initial earnings level and the subsequent job-loss 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 non-parametric 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 (in the absence of unemployment termination) 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 durations. This should be kept in mind when interpreting results regarding the impacts of ALMP duration.

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 determined as the product of the hourly wage rate and the number of hours worked. Unfortunately, the data do not provide sufficient information for identifying these two variables 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 yearly 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, 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 quite evenly distributed between education, benefit

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

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 "other" transition are right-censored. Another important point to note is that employment obtained after an unemployment period 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 who get a job are around 26.000 NOK (3.250 EURO). 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 rates 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}:

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

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

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

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:

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

where s_{it} is a vector of calendar month dummy variables (one for each calendar month occurring 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 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

[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.

the business cycle through the interaction of spell duration with business cycle conditions. The parameters associated with the spell duration dummy variables (λ_{kdt}) reflect duration dependence under "normal" cyclical conditions. A more detailed description of the model (and its variables) will be given as we present the estimation results in the next section.

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 unemployment spell, $\overline{r}_i$ 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

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

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

A point to note is that all the variables explaining expected acceptable earnings (5) are also assumed to have direct effects on the various hazard rates. Hence, given the unrestricted correlation between unobserved covariates, the level of expected earnings is implicitly included in all the hazard rates.

3.2 Identification

Our model is non-parametrically identified not only from the proportionality assumption and the existence of repeat spells (Abbring and Van den Berg, 2003), but also from 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. Our data also 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 that a reform was implemented in Norway in 1997, extending the standard UI period from 80 to 156 weeks, introducing an exogenous break in the otherwise strong positive correlation between unemployment duration and UI exhaustion. 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. We can also identify the impacts of this reform on the initial hazard rates (before exhaustion effects kick in) and on the subsequent job quality. However, in order to disentangle these effects from the impacts of calendar time, we need the additional identifying restriction that pure calendar time changes (business cycle fluctuations) had the same impact on UI claimants (who were affected by the reform) as they had on non-claimants (who were not affected by the reform); see Røed and Westlie (2007) for evidence that this was indeed the case.

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}, $k=1,\ldots,5,$ 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:

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

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 first entry. However, for interval censored data of the type used here, this assumption is violated; see Gaure *et al.* (2007). 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 entrances 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*. It can then be shown (Gaure *et al.*, 2007) that the probability of surviving the inflow month – i.e., of being included in the analysis population – is 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

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

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 non-parametrically by means of the non-parametric 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

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

where $\{v_l, q_l\}$, $l = 1,2,...Q$, are the location vectors and probabilities characterizing the heterogeneity distribution, and the functions $E_i(.)$, $S_i(.)$ are defined in (6) and (7), respectively. A detailed description of our optimization algorithm is provided in Gaure *et al.* (2007).

4. Main Results

Our model contains around 1,700 estimated parameters. Most of them are included solely for control purposes and are unimportant for the topics discussed in this paper. Hence, we do not present the results in any detail. A complete list of estimation results is posted on our website www.frisch.uio.no/docs/job_search.html. Some alternative models and robustness checks are provided in the next section. In this section, we focus on key results related to duration dependence, UI institutions, ALMP treatment effects, and other results of economic interest. The results are presented in terms of individual parameter estimates (relative hazard rates) and full simulation exercises. We only present the results from the preferred model, which was selected on the basis of the Akaike information criterion (AIC); see Gaure *et al.* (2007) for a justification of this choice. This model required 27 support points in the heterogeneity distribution. We were able to obtain small increments in the likelihood function for further expansions of the heterogeneity distribution up to as much as 35 support points, but, as it turned out, the vector of estimated parameters hardly changed at all after the AIC was satisfied.

4.1 Duration dependence and the impact of past unemployment spells

We start out presenting the so-called baseline hazards for the four events potentially occurring during job search; see Figure 2. The graphs are normalized on unity for the first duration month and display the degrees of duration dependencies during a *first* unemployment spell under "normal" (average) business cycle conditions. The baseline hazards are computed net of any direct UI exhaustion effects (see next subsection). There is clearly negative duration dependence in the employment, education, and ALMP participation hazards. The other-benefit-hazard is relatively stable, with weak negative duration dependence initially, followed by positive duration dependence. 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 job 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.[3]

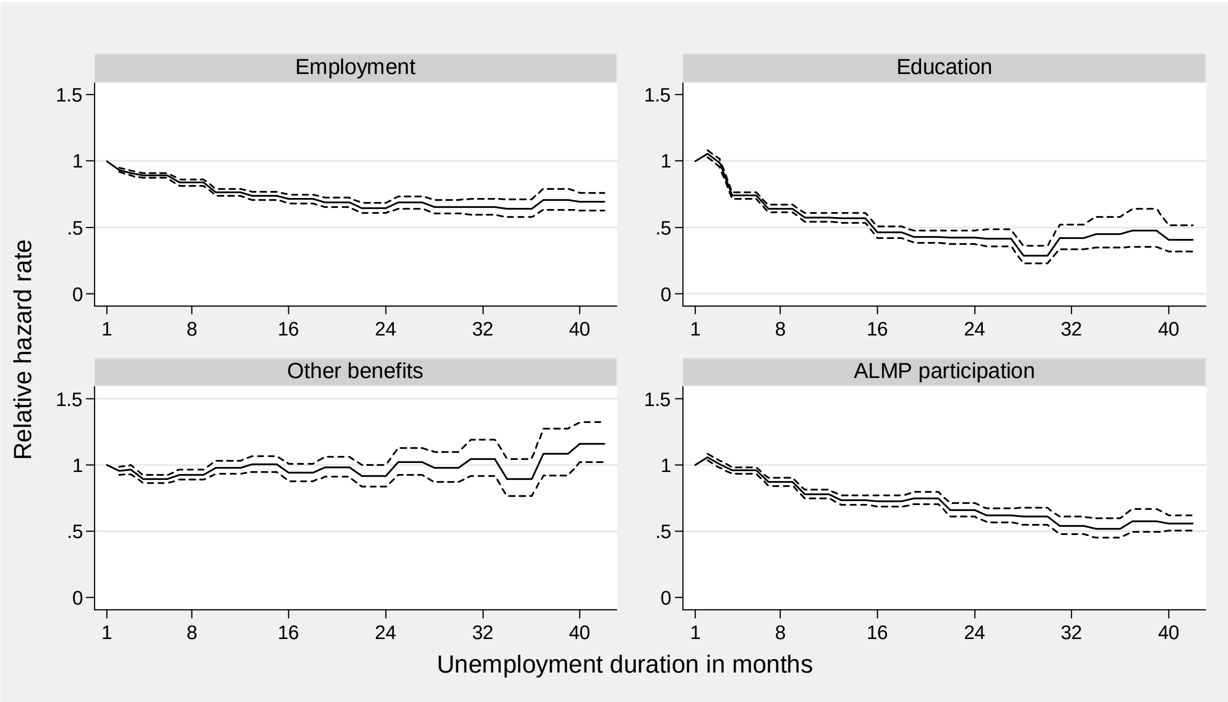

Figure 2. The estimated unemployment duration effects on the hazard rates out of unemployment, with 95 percent confidence intervals.
Note: All effects are normalized on the first month and reflect relative changes in hazard rates as duration increases, ceteris paribus. The reported duration effects in panels 1-5 apply for a new entrant to unemployment (with no previous unemployment during the last three years) under "normal" business cycle conditions.

Job search duration may also affect the quality of the expected job match. Figure 3 displays the estimated impacts of job search duration on subsequent earnings and employment

[3] 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.

stability. A key finding is that a longer job search period pays off in terms of higher expected earnings once a job is obtained. This is consistent with the notion that job search is a productive endeavor. However, there is no additional earnings gain associated with unemployment durations beyond approximately 6 months, and after 15 months the impact of lengthening the job 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.

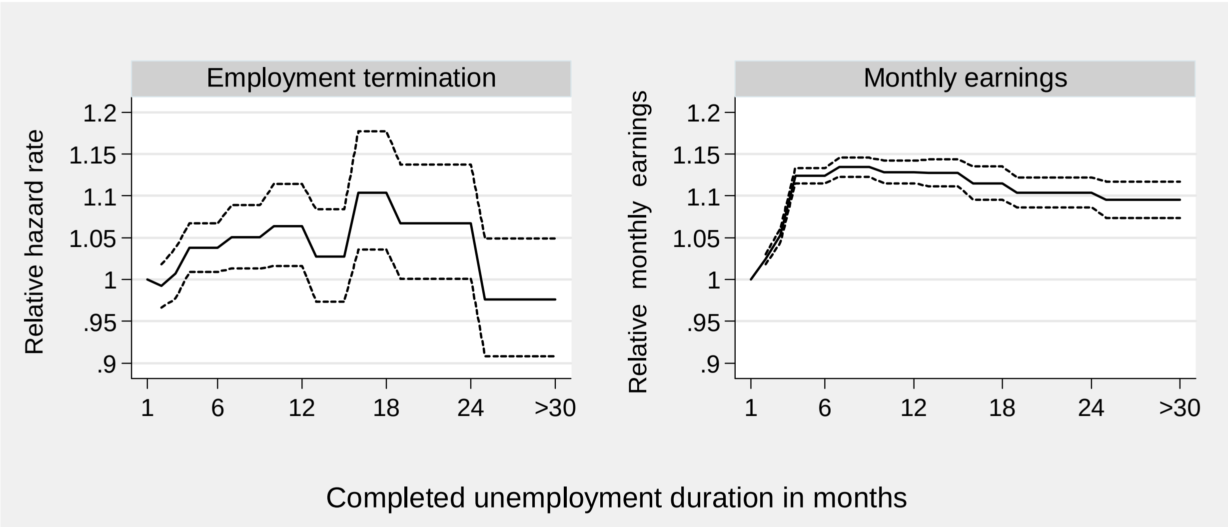

Figure 3. The impact of completed unemployment duration on earnings and employment stability, with 95 percent confidence interval

Job security improves rapidly with tenure. This is illustrated in Figure 4, where we display the estimated baseline hazard for the termination of jobs found after unemployed job search (note that we now measure employment duration, and not unemployment duration, on the horizontal axis). The monthly probability of ending a newly obtained employment status declines by around 70 percent during the first year of employment.

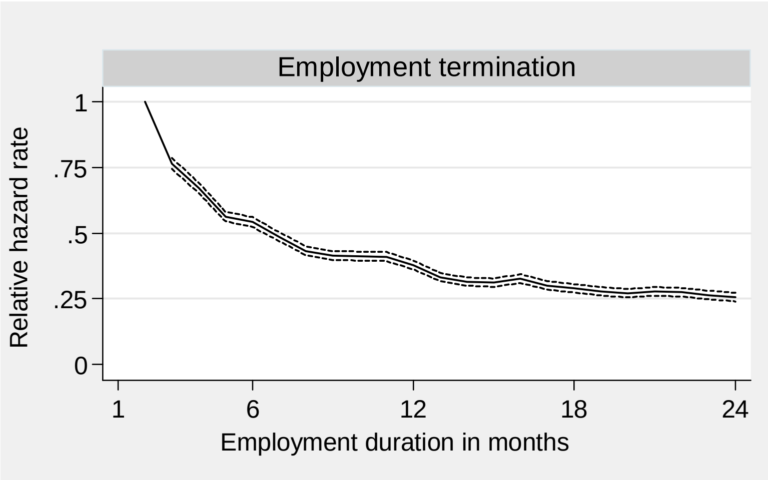

Figure 4. The estimated employment duration effect on the hazard rate out of employment, with 95 percent confidence intervals.
Note: The effect is normalized on the first month and reflect relative changes in hazard rates as employment duration increases, ceteris paribus.

4.2 The impacts of the UI system

The UI system is represented in the model by UI regime dummy variables indicating eligibility to, and maximum duration of, benefits, and by a vector of dummy variables "counting down" to UI exhaustion during the six months just prior to benefit exhaustion.[4]

The effects of UI entitlement are presented in Tables 3 and 4. Compared with entrants eligible for 80 week UI benefits (UI claimants who entered before 1997), non-eligible entrants have lower employment and education hazards and a higher sickness/disability hazard. It is also worth noting that non-eligible job searchers tend to accept around 9 percent lower earnings than eligible job searchers, ceteris paribus. These effects must be interpreted with care since UI eligibility is not assigned randomly.[5] However, the causal impacts of the 1997 reform – extending the initial UI period for most job seekers from 80 to 156 weeks – are identi-

[4] We also include a dummy indicating close contact between job searcher and case worker. This dummy is equal to one during UI application periods. An application period occurs when an eligible individual starts a new unemployment spell, unless he/she continues to draw on existing UI entitlements (if a previous spell was concluded less than 12 months ago). An application period also occurs after exhaustion of the initial 80 week period in the pre-1997 UI system; see Røed and Westlie (2007) for details. Application periods entail a relatively close contact with the employment office, including the sorting out of potential job opportunities.

[5] UI eligibility requires that yearly labor earnings exceeded approximately 60,000 NOK in the year prior to the start of the unemployment spell (or that the average income during the past three years exceeded that amount).

fied, provided that cyclical fluctuations affected claimants and non-claimants in a similar fashion; see Section 3. It is clear from Table 3 that the reform caused negative shifts in all hazard rates out of unemployment, in line with the findings in Røed and Westlie (2007). Other things equal, the employment hazard fell by around 19 percent. It is also clear from Table 4 that the extension of the UI period caused an improvement in the quality of accepted jobs, conditional on unemployment duration. Earnings increased by around 5 percent, while the subsequent employment termination hazard declined by 3 percent.

Table 3
The effects of UI entitlements on hazard rates

	Employment		Education		Other benefit		ALMP	
	Est.	S.E.	Est.	S.E.	Est.	S.E.	Est.	S.E.
UI regime in ongoing unemployment spell								
80 weeks initial UI period (before 1997)	ref.		ref.		ref.		ref.	
156 weeks UI period (from 1997)	-0.188	0.009	-0.417	0.018	-0.357	0.017	-0.113	0.015
No UI entitlements	-0.190	0.011	-0.030	0.020	0.530	0.021	0.455	0.017

Table 4
The effects of UI entitlements on job quality

	Employment termination		Log monthly earnings	
	Estimate	S.E.	Estimate	S.E.
UI regime in completed unemployment spell				
80 weeks initial UI period (before 1997)	ref.		ref.	
156 weeks UI period (from 1997)	-0.032	0.012	0.049	0.003
No UI entitlements	0.024	0.017	-0.090	0.005

Figure 5 displays the estimated shape of the hazard rates in the run-up to UI exhaustion, relative to claimants with more than six months left of their UI period. Unsurprisingly, all hazard rates rise significantly as the moment of benefit exhaustion approaches. In addition both employment and education hazards remain at a relatively high level after exhaustion. An explanation for the drop in the hazards to other benefits and ALMP may be that those who remain unemployed after UI exhaustion also to a large extent have exhausted their options in terms of other benefits and ALMP offers.

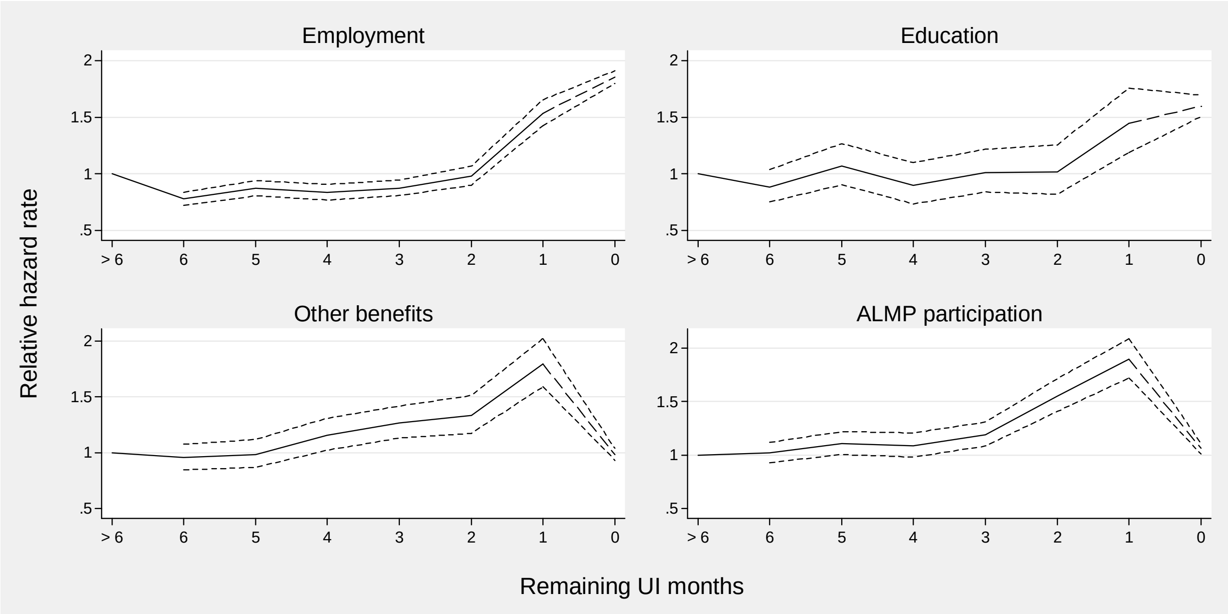

Figure 5. The impacts of UI exhaustion on hazard rates out of open unemployment, with 95 percent confidence intervals
Note: The graphs are normalized on a situation with more than six months left of the UI period.

Exhaustion of UI benefits also affects fastidiousness and reservation wages; see Figure 6. As expected, we find that realized earnings are significantly lower for jobs accepted in the run-up to UI exhaustion than for jobs accepted earlier in the unemployment spell. 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 and employment stability than jobs accepted in the run-up to exhaustion. A possible interpretation of this finding is that the impact of UI exhaustion on the reservation wage is really heterogeneous across individuals (in contrast to the model's assumption of a homogeneous effect), and that individuals with the largest responses are sorted out of unemployment during the exhaustion period.

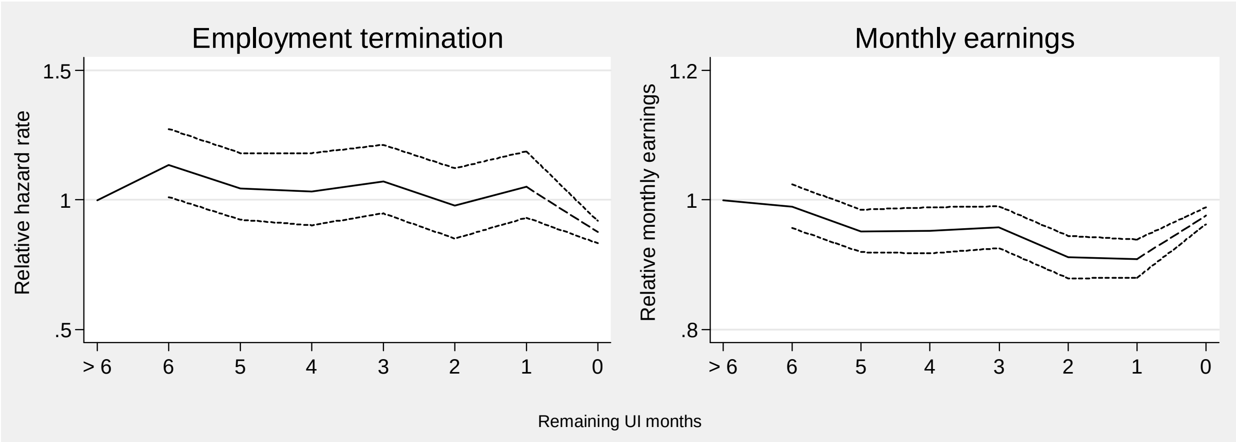

Figure 6. The impacts of UI exhaustion on the quality of the accepted job, with 95 percent confidence intervals
Note: The graphs are normalized on a situation with more than six months left of the UI period.

In order to summarize the impacts of the 1997 UI-reform, we perform a simulation exercise; i.e., we use the estimated model to simulate the outcomes (in terms of unemployment duration, destination state, and – if the destination state is employment – earnings and employment duration) of all insured unemployment spells under the old and the new regime, ceteris paribus. We restrict attention to the outcomes of the first unemployment spells only, since 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). The pre and post reform simulations are different only with respect to the value of the appropriate regime variables, except that the calendar time effects in the treatment hazard are scaled such that they are equal to their estimated pre and post reform averages, respectively (implying that the treatment hazard is approximately 30 percent higher in the pre than in the post reform regime, ceteris paribus). 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.[6] In total, we make 120 simulations under each regime, and calculate 95 percent confidence intervals for the statistics of interest. The results are provided in Table 5. They show that the reform (with longer maximum UI duration and weaker activity requirements) caused mean unemployment duration to increase by 1.8 months (around 27 percent). This is roughly in accordance with the findings reported by Røed and Westlie (2007). However, in contrast to Røed and Westlie (2007), we find that the rise in unemployment duration was accompanied by an increase in the proportion of unemployment spells ending with a transition to employment (by 2.7 percentage points). In line with the findings reported in Table 4, we also find that realized first-job-earnings increased by around 4.3 percent as a result of the reform.

Table 5
Simulated impacts of the 1997 UI reform

	I	II	III
	Based on the pre-1997 system. All unemployed are entitled to 2*80 weeks of UI-insurance	Based on the after-1997 system. All unemployed are entitled to 156 weeks of UI-insurance	Difference (II-I) [95% CI in brackets]
Outcomes of the first unemployment spell			
Percent of unemployment spells ending in			
Employment	69.53	72.22	2.69 [2.01, 3.35]
Education	14.63	12.81	-1.82 [-2.20, -1.34]
Other benefit	15.15	13.81	-1.34 [-1.82, -0.69]
Censored due to end of observation period	0.68	1.16	0.47 [0.35, 0.60]
Mean duration of unemployment spells	6.61	8.41	1.80 [1.65,1.97]
Outcomes of the first employment spell			
Mean monthly earnings first employment spell	30,813	32,132	1,319 [1086, 1510]
Fraction of employment spells terminated within first year after employment transition	27.09	27.22	0.13 [-0.75, 0.56]

[6] We draw 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 $b_s = \hat{b} + Lz_s$.

4.3 The impacts of ALMP

The estimated impacts of ALMP participation on events during the job search period are presented in Table 6. 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. For a typical participant, the employment effect becomes positive after 5-6 months 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 6
Effects of ALMP participation on hazard rates during the participation period and afterwards

	Employment		Education		Other benefit		ALMP	
	Est.	S.E.	Est.	S.E.	Est.	S.E.	Est.	S.E.
On-program effect reference	-0.363	0.014	-0.285	0.021	-0.753	0.024	-	
+ deviation from 4 month ongoing program duration (ln(duration)-ln(4))	0.805	0.010	0.801	0.015	0.257	0.015	-	
+ male	0.117	0.015	-0.077	0.021	0.028	0.024	-	
+ deviation from mean education (years)	0.045	0.004	-0.014	0.006	0.039	0.008	-	
+ deviation from mean cyclical conditions	0.323	0.057	0.771	0.089	0.131	0.090	-	
Post-program effect reference	0.196	0.017	0.065	0.030	0.015	0.027	0.519	0.018
+ deviation from 4 month completed program duration (ln(duration)-ln(4))	0.174	0.012	0.228	0.023	0.022	0.018	0.076	0.011
+ male	0.014	0.020	-0.065	0.036	0.074	0.028	0.182	0.018
+ deviation from mean education (years)	0.027	0.006	0.013	0.011	-0.020	0.010	0.096	0.005
+ deviation from mean cyclical conditions	0.153	0.077	0.171	0.145	0.186	0.113	0.115	0.082

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

Participation in ALMP also potentially affects the quality of a subsequent job; see Table 7. 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 termination hazard of around five percent, ceteris paribus.

Table 7
Effects of ALMP participation on the quality of a realized job

	Employment termination		Log monthly earnings	
	Estimate	S.E.	Estimate	S.E.
Effect of program reference	0.051	0.019	0.040	0.005
+ completed program duration (ln(duration)-ln(4))	-0.138	0.012	0.070	0.003
+ male	-0.074	0.019	-0.019	0.005
+ deviation from mean education (years)	-0.016	0.006	0.006	0.001
+ deviation from mean cyclical conditions	-0.025	0.068	0.030	0.022

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

In order to evaluate the overall impact of ALMP, we perform a simulation exercise similar to the one described in the previous subsection, only this time we manipulate the impacts of ALMP. More specifically, we compare simulations based on the estimated model with simulations based on the same estimated model, with the important exception that ALMP is assumed irrelevant (the impacts on all final destination hazards are set to zero). The latter simulations represent the no-treatment-world, with the important qualification that it does not remove the effects that a given program structure may have on search behavior other than through *actual* participation. Note, however, that the group of participants is identified even in the no-treatment world, based on exactly the same sorting process as in the treatment world (the only difference is that treatment is completely irrelevant in the non-treatment world). This implies that we can compare the group of treated individuals with and without actual treatment. It also implies that we can characterize the sorting process into treatment.

The results are provided in Table 8. The first two columns summarize the outcomes for non-participants and participants *in the absence of any treatment effects*. The results indicate that there is a negative selection into ALMP. Non-participants' likelihood of ending up in employment is on average 8.4 percentage points higher than those of the participants. Their subsequent earnings are around 11 percent higher. 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 unemployment duration. The causal impacts of ALMP are assessed by comparing the outcomes for the group of participants in the treatment and non-treatment worlds, see Column 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, or 2.5 percent. 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.

Table 8
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	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	1,056,245	700,667	689,739	-10,928 [-20,851, 371]

Table 8
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]*
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	9.85	19.33	20.47	1.14 [0.99, 1.23]
Mean number of months in 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		0	45,130	45,130
C. Total mean operating cost of ALMP per participant		0	21,086	21,086
D. ALMP net revenue per participant (A+B-C)				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. 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).

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. 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. 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 earnings). However, some ALMP's clearly involve work of direct economic value. Around 60 percent of the program activities are employment programs 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.* (2007). 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. Job 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 8 are based on the assumption that subsidized 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 payments received by the participant – amounts to 3,620 NOK per month.[7] 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 towards 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.

4.4 The role of human capital, economic incentives and family situation

The job seeker's human capital is of great importance, both with respect to the duration and outcome of the unemployment spell and with respect to job quality. Higher education and more work experience (conditional on age) imply shorter unemployment and higher probability of getting a job, and also higher earnings and more secure employment given that a job is found. Comparing, for example, a PhD education with a 12 year secondary education, the job

[7] 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.

hazard is approximately 40 percent higher, the earnings level is 42 percent higher, and the employment termination hazard is 47 percent lower, ceteris paribus.

It is difficult to evaluate the impacts of economic incentives embedded in the level of UI payments, since the benefit variation in our data is non-random. The estimated UI benefit elasticity in the employment hazard is -0.039 (0.001) (standard error in parenthesis). In the earnings equation, the elasticity is estimated to the negligible level of -0.004 (0.0005). However, the former of these estimates is much smaller than previously found on similar data with exploitation of random-assignment-like variation in UI benefits (Røed and Zhang, 2003; 2005); hence we view our results at this point with suspicion.

We also investigate the impact of the realized earnings level – i.e. the draw from the individual log-normal earnings distribution – on the employment termination process. The elasticity of the employment termination hazard with respect to the earnings level is estimated to 0.254 (0.016), i.e., a 10 percent increase in monthly earnings implies a 2.5 percent increase in the job termination hazard. This positive relationship is most likely driven by involuntary job terminations, and indicates that higher earnings (conditional on human capital variables) to some extent compensate for insecure jobs.

We have found that family situation – in terms of the number and age of children – has a substantial impact on the behavior of women, but virtually no impact on the behavior of men. For women without children, we find that the job hazard rate is almost identical to that of otherwise identical men. However, upon obtaining a job, men’s earnings are approximately 17 percent higher than women’s earnings, ceteris paribus. Having responsibility for small children reduces the women’s job hazards and earnings substantially. For example, a having a single child aged 4-6 reduces the female job hazard by 41 percent and the earnings level by 12 percent.

4.5 Unobserved heterogeneity

The correlation structure of the six random individual effects is described in Table 9. 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.[8] There seems to be a positive unobserved selection into ALMP in the sense that the treatment propensity correlates positively with employment propensity. Unsurprisingly, the employment propensity also correlates positively with earnings and negatively with employment termination propensity.

Table 9
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

Unobserved heterogeneity also explains a substantial fraction of earnings dispersion across individuals. From Table 2, we have that the overall standard deviation of log earnings is 0.602. By including 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.

---

[8] 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 $\tau_{kl} = \frac{c-d}{c+d}$. Note that we disregard the fraction $\sum_{s=1}^{Q} q_s^{\,2}$ of identical pairs drawn from the same location vector.

5. Alternative model specifications and robustness

We have estimated a number of alternative models, both to test particular hypothesis of economic interest and to assess the robustness of our results. In this section we briefly describe these alternative models and the corresponding estimation results. Complete results from all the estimations described in this section can be downloaded from our web site www.frisch.uio.no/docs/job_search.html.

5.1 Liquidity constraints

Existing empirical evidence indicates that the behavioral impacts of UI insurance depend on the prevalence of liquidity constraints (Chetty, 2008). For a large fraction of unemployment entrants in our dataset (those entering after 1994), we have register-based information on their own, as well as their family members', bank accounts at the end of the year prior to the year of entry into unemployment. We define liquidity as the sum of the family account balances divided by the square root of the number of family members. We then divide the analysis population in two equally sized groups; those with liquidity above the median and those with liquidity below the median. We estimate a version of the model described in Section 3 where all incentive and duration dependence parameters are allowed to vary between these two groups. The most important differences between the two groups are; first, that the transition rate to other benefits is significantly higher for job seekers with poor liquidity; second, that liquidity-constrained individuals exhibit stronger negative duration dependence in the hazard rate to employment; and third, that these individuals also lose a subsequent job much faster than non-constrained individuals. These findings may reflect that poor liquidity actually *results from* weak labor market attachment in the past, which is also associated with high exit probability through other benefits and rapid discouragement. We find no evidence that job seekers with poor liquidity responded particularly strongly to UI exhaustion or to the UI reform in 1997.

5.2 Non-modeled re-entries

As explained in Section 3, re-entries into unemployment (repeat spells) are endogenously modeled insofar as they follow from the termination of an observed employment spell. However, the data also include a number of non-modeled re-entries into unemployment, namely those occurring after transitions to education or sickness/disability. These re-entries are thus treated as determined outside the model (exogenous). Since the process of re-entry may be driven by the same unobserved characteristics that determine modeled outcomes, this represent a potential source of sorting bias. To assess the importance of this problem we also estimate the model without including non-modeled re-entry spells. It turns out, however, that while the inclusion of these spells does have some impact on the estimated effects of past unemployment (lagged duration dependence) it only has minor impacts on the parameters discussed in this paper.

5.3 Part-time work

UI claimants are obliged to report any part-time or occasional work that they perform while claiming benefits. If the earnings are X percent of previous full-time earnings, the benefit for the corresponding period is also reduced by X percent. These part-time work spells are recorded in the data, and in one version of the model we have included transitions to part-time work as an additional endogenous event (in exactly the same fashion as we include ALMP; see Section 3). The results from this exercise indicate that part-time and occasional work may serve as important stepping-stones towards more satisfactory employment. Otherwise, the results are very similar to those reported in Section 4. The reason why we left part-time work out of our preferred model is that we suspect measurement errors to be large. In particular, we do not systematically record part-time work for non-claimants. We also suspect that part-time work sometimes indicates that a satisfactory job is really found, but that it for some reason is difficult to start working full-time immediately (in which case our stepping-stone effect will be biased upwards).

5.4 Unobserved heterogeneity

While our preferred model is obtained on the basis of an AIC criterion (resulting in a joint discrete distribution of unobserved heterogeneity with 27 support points), we have of course also estimated the model with fewer support points, including only one (no unobserved heterogeneity). The results from the latter model are reported in full on our web page. Unsurprisingly, these results are significantly different from those reported in the paper, particularly with respect to the various duration dependence baselines. The "approach" towards the preferred model is smooth, however, and most of the estimated parameters are close to their "final" values after the inclusion of 10-15 points in the heterogeneity distribution. We also continued to include more points in the heterogeneity distribution after the AIC criterion was satisfied. However, apart from the heterogeneity parameters themselves (locations and probabilities), no parameters were visibly affected by this model extension. We terminated our likelihood improvement attempts after 35 support points.

6. Conclusion

A key parameter of UI systems 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 the maximum UI duration, 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.

Actual participation in labor market programs also involves some conflicting mechanisms. With respect to unemployment duration, there is initially an adverse 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 on average 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 initial 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 i 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.

References

Abbring, J. H. and Van den Berg, G. J. (2003) The Non-Parametric Identification of Treatment Effects in Duration Models. *Econometrica*, Vol. 71, No. 5 (September), 1491-1517.

Acemoglu, D. and Shimer, R. (1999) Efficient Unemployment Insurance. *The Journal of Political Economy*, Vol. 107, 893-928.

Acemoglu, D. and Shimer, R. (2000) Productivity gains from unemployment insurance. *European Economic Review*, Vol. 44, 1195-1224.

Addison, J. T. and Blackburn, M. L. (2000) The Effects of Unemployment Insurance on Post-unemployment Earnings. *Labor Economics*, Vol. 7, 21-53.

Belzil, C. (2001) Unemployment Insurance and Subsequent Job Duration: Job Matching versus Unobserved Heterogeneity. *Journal of Applied Econometrics*, Vol. 16, 619-636.

Black, D. A., Smith, J. A, Berger, M. C. and Noel, B. J. (2003) Is the Threat of Reemployment Services More Effective than the Services Themselves? Evidence from Random Assignment in the UI System. *American Economic Review*, Vol. 93, 1313-1327.

Brinch, C. (2007) Non-Parametric Identification of the Mixed Hazards Model with Time Varying Covariates. *Econometric Theory*, Vol. 23, 349-354.

Chetty, R. (2008) Moral Hazard versus Liquidity and Optimal Unemployment Insurance. *Journal of Political Economy*, Vol. 116, No.2, 173-234.

Eberwein, C., Ham, J. C. and LaLonde, R. J. (1997). The Impact of Being Offered and Receiving Classroom Training on the Employment Histories of Disadvantaged Women: Evidence from Experimental Data. *Review of Economic Studies*, Vol. 64, 655-82.

Gaure, S. and Røed, K. (2007) How Tight is the Labour Market? Sources of Changes in the Aggregate Exit Rate from Unemployment across the Business Cycle. *Journal of Business Cycle Measurement and Analysis*, Vol. 3, No. 2, 155-174.

Gaure, S., Røed, K. and Zhang, T. (2007) Time and Causality: A Monte Carlo Assessment of the Timing-of-Events Approach. *Journal of Econometrics*, Vol. 141 (2007) 1159–1195.

Heckman, J. and Singer, B. (1984) A Method for Minimizing the Impact of Distributional Assumptions in Econometric Models for Duration Data. *Econometrica*, Vol. 52, 271-320.

Jespersen, S. T., Munch, J. R, and Skipper, L. (2008) Costs and Benefits of Danish Active Labour Market Programmes. *Labour Economics*, forthcoming.

Kluve, J. (2006) The Effectiveness of European Active Labor Market Policy. IZA Discussion Paper No. 2018.

Kluve, J., Card, D. and Fertig, M. (2007) *Active Labor Market Policies in Europe - Performance and Perspectives*. Springer-Verlag Berlin and Heidelberg GmbH & Co.

Lindsay, B. G. (1983) The Geometry of Mixture Likelihoods: A General Theory. *The Annals* *of Statistics*, Vol. 11, 86-94.

Marimon, R. and Zilibotti, F. (1999) Unemployment vs. Mismatch of Talents: Reconsidering Unemployment Benefits. *The Economic Journal*, Vol. 109, 266-291.

McCall, B. P. (1994) Identifying State Dependence in Duration Models. American Statistical Association 1994, Proceedings of the Business and Economics Section, 14-17.

Meyer, B. D. (1990) Unemployment Insurance and Unemployment Spells. *Econometrica*, Vol. 58, No. 4, 757-782.

Pavoni, N. (2007) On Optimal Unemployment Compensation. *Journal of Monetary Economics*, Vol. 54, No. 6, 1612-1630.

Prentice, R. L. and Gloeckler, L. A. (1978) Regression Analysis of Grouped Survival Data with Application to Breast Cancer Data. *Biometrics*, Vol. 34, 57-67.

Røed, K. and Raaum, O. (2006) Do Labour Market Programmes Speed up the Return to Work? *Oxford Bulletin of Economics and Statistics*, Vol. 68, No. 5, 541-568.

Røed, K. and Westlie, L. (2007) Unemployment Insurance in Welfare States: Soft Constraints and Mild Sanctions. IZA Discussion Paper No. 2877.

Røed, K. and Zhang, T. (2003) Does Unemployment Compensation Affect Unemployment Duration? *The Economic Journal*, Vol. 113, 190-206.

Røed, K. and Zhang, T. (2005) Unemployment Duration and Economic Incentives - A Quasi Random-Assignment Approach. *European Economic Review*, Vol. 49, 1799-1825.

Van Ours, J. C. and Vodopivec, M. (2008) Does Reducing Unemployment Insurance Generosity Reduce Job Match Quality? *Journal of Public Economics*, Vol. 92, No.3-4, 684-695.