# MEMORANDUM

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**The Duration and Outcome of Unemployment Spells**
**- The role of Economic Incentives**

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# The Duration and Outcome of Unemployment Spells – The Role of Economic Incentives

By Knut Røed and Tao Zhang*


## Abstract

We investigate how transitions from unemployment are affected by economic incentives and spell duration. Based on unique Norwegian register data that exhibit the rarity of random-assignment-like variation in economic incentives, the causal parameters are identified without reliance on distributional assumptions or functional form restrictions. We find that the hazard rates are negatively affected by the replacement ratio, but that the size of these effects varies considerably among individuals. There is strong negative duration dependence in the employment hazard and positive duration dependence in the 'discouragement' hazard. The employment hazard rises substantially in the months just prior to benefit exhaustion.


*Keywords*: Competing risks, unemployment duration, random assignment

*JEL Classification*: C41, J 64

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## 1 Introduction

The aims of this paper are first, to uncover the extent to which the level and the duration unemployment benefits affect individual transition probabilities out of unemployment, and second, to identify the shape of structural (genuine) duration dependence governing these transitions. In order to fulfil these aims, we have to overcome one of most fundamental problems in virtually all microeconometric applications: the isolation of causal effects from selection mechanisms related to unobserved heterogeneity. In our case, there are two types of unobserved factors that may corrupt our attempts to identify causality. The first is unobserved characteristics that are related to the economic incentive variables for which causal effects are to be identified. In our case, this kind of relationship arises because eligibility to, as well as the level of unemployment benefits typically depend on past labour market behaviour, which again may have been affected by unobserved personal characteristic that also affect the hazard rates in question directly. The second source of distortion is unobserved heterogeneity that at the moment of inflow to the unemployment pool is unrelated to the explanatory variables of interest, but nevertheless produce a sorting effect as the spells proceed. This sorting effect is well known to produce a transition rate pattern over spell duration that is far from causal, and also to inject a duration-specific dependence between unobserved heterogeneity and explanatory variables.

Although there is by now a vast and advanced unemployment duration literature addressing benefit compensation- as well as spell duration effects[1] (see e.g.

[1] Important contributions to this literature include Lancaster (1979), Moffitt (1985), Narendranathan et al (1985), Katz and Meyer (1990), Meyer (1990) and Card and Levine (2000).

Devine and Kiefer, 1991, or Pedersen and Westergård-Nielsen, 1998, for recent surveys), the issue of causality remains basically unsettled. The reason for this is that virtually all the proposed sources of identification are encumbered with disturbing question marks. The most promising attempts to identify causal effects associated with unemployment benefits are probably those built on the difference-in-difference methodology, in which identification is based on policy reforms that affect some, but not all unemployed (Meyer, 1989; Hunt, 1995; Winter-Ebmer, 1998; Carling et al, 2001). But even these papers have had to rely on the sometimes questionable (and untestable) assumption that labour market opportunities do not develop differently for the 'treatment' and the 'control' groups. The issue of identifying spell duration effects has been subject to huge progress during the past few years, particularly on the theoretical front (see van den Berg, 2001, for a recent survey), but most identification results still hinge on functional form assumptions such as mixed proportionality (MPH). Moreover, applications typically rest on additional and much more restrictive functional form assumptions, which are imposed for practical- or computational reasons.

In this paper we identify the causal effects of interest in a purely data-based fashion, with a minimum of parametric assumptions. For this purpose, we take advantage of a unique Norwegian database (the Frisch Database), which describes the main labour market activity for the Norwegian adult population by the end of each month during the period from 1992 to 1997. In order to identify the causal effect of unemployment benefits, we have searched through the benefit system and its recent history in order to disclose administrative procedures and/or events that may contain elements of 'random-assignment-like' variation in unemployment benefits. And what we have found is that the bureaucracy indeed produces differences in benefit out-comes that are arbitrary (many would say unfair) from the viewpoint of the individuals. For example, for reasons of verifiability, benefits are calculated on the basis of labour income recorded in the previous *calendar year*, implying that a given income entails higher benefits the more it is concentrated within the last calendar year. Rules like this have a purely administrative motivation with no behavioural justification, and in some cases they yield peculiar results. And although this type of variation is of minor importance for most people, the sheer size of the data we use ensures that it is sufficient for investigating not only average disincentive effects, but also the extent to which these effects interact with business cycles, spell duration, age and individual economic resources. The pattern of structural (individual level) duration dependence is identified non-parametrically without reliance on any arbitrary parametric assumptions. Our main basis for identification is the presence of multiple inflow cohorts, which at any duration above zero entails a substantial variation in lagged hazard rates, conditional on the current search environment. The intuition behind this source of identification is as follows: The lagged variation in hazard rates ensures that otherwise similar persons have been subject to different hazard rates earlier in their spell. And persons who according to observed characteristics (including business- and seasonal cycles) have had a high probability of making a particular transition without doing so, will on average have 'poorer' unobserved characteristics regarding that particular transition than persons who according to the observed information in any case have had a low probability of making that transition.

The present paper builds on previous work described in Røed and Zhang (2003), in which we analysed the effects of unemployment compensation on unemployment duration for a relatively homogenous group of unemployed (with previous incomes ranging from around 25,000 to 33,000 Euro) for which the variation in benefits (conditioned on previous income) was fully explained by a random-assignment-like process. We found that the average elasticity of the hazard rate with respect to unemployment compensation within this group was around –0.7, and that the behavioural responses towards unemployment compensation were relatively stable over the business cycle and over the spell duration. In the present paper, we instead seek to isolate the random-assignment type variation for unemployed workers in all income classes by a particular form of decomposition of the replacement ratio (unemployment benefits divided by the expected wage). This decomposition has the interesting property that the resulting random-assignment type component in the replacement ratio, which we use to identify causal effects, is correctly measured even when expected wages are incorrectly measured. Furthermore, we extend the single risk approach adopted in Røed and Zhang (2003) into a competing risks framework in which there are four possible exits out of insured unemployment spells; i) an ordinary job; ii) long term sickness or temporary/permanent disability (discouragement); iii) loss of benefits (due to exhaustion or sanctions); and iv) participation in labour market programs. The next section gives a brief outline of the theoretical background. Section 3 presents the data with a particular focus on the sources of independent (random) variation in the replacement ratio and the decomposition method used to isolate this variation. Section 4 presents the econometric model, including our treatment of unobserved heterogeneity. Section 5 presents the results and section 6 summarises the main conclusion.

## 2 The Theoretical Background

Dynamic search theory (Mortensen, 1977; 1990; van den Berg, 1990a) suggests that a higher level of unemployment benefits, conditioned on the expected wage, normally entails reduced job search effort, increased job selectivity (reservation wage), and hence a reduced probability of making a transition from unemployment to employment. Moreover, there is positive structural duration dependence in the job transition hazard, since search effort increases- and the reservation wage decreases as the moment of benefit exhaustion comes closer.

A number of economic mechanisms has been identified in the literature that may complicate the relationship between marginal changes in the benefit level and the job search behaviour. First, the arrival rate of job offers not only depends on individual search effort, but also on the tightness of the labour market. This implies that the relative influence of the supply constraint (the reservation wage) and the demand constraint (the number of job offers) may differ over the business cycle, such that disincentive effects associated with the benefit level are stronger in good times than in bad times (Moffitt, 1985; Arulampalam and Stewart, 1995). Second, since the discounted value of future potential benefits declines over spell duration, it is possible that marginal changes in the benefit level has a larger behavioural effect the shorter is the unemployment spell. In addition, long term unemployed may have reduced their reservation sufficiently to make a marginal change in benefits virtually irrelevant (Narendranathan and Stewart, 1993). On the other hand, the presence of liquidity constraints may imply that economic incentives in general become more important as the unemployment spell is prolonged. Third, persons facing tight wage distributions are likely to exhibit larger benefit responses than persons facing wide wage distributions, since the effect of a rise in the reservation wage will have a larger impact on the job rejection rate the tighter is the wage distribution (Narendranathan et al, 1985). Since young workers typically face tighter wage distributions than older workers, this may imply that marginal disincentive effects are relatively stronger for younger workers. Fourth, disincentive effects may depend on marital status and the economic resources of the household. A sound family economy (with no immediate liquidity constraints) may entail a low sensitivity towards marginal changes in the benefit level. But even the other extreme (economic hardship) may imply low sensitivity towards marginal changes, since the benefit level in any case may be considered utterly insufficient.

The issue of duration dependence is also complicated by factors that are not directly related to benefit exhaustion. First, there may be discouragement effects, implying that the effective level of job search declines as the unemployment spell is prolonged (Layard et al, 1991; Vendrik, 1993; Røed et al, 1999). Second, the level of transferable skills may depreciate during longer unemployment spells (Pissarides, 1992), implying that the expected wage also declines and that the replacement ratio increases. Third, since the average (unobserved) 'quality' of the workers declines as a function of spell duration, employers may use the length of the unemployment spell as a tool for statistical discrimination (Blanchard and Diamond, 1994). All these mechanisms tend to produce negative structural duration dependence in the job hazard rate. At the same time, they probably contribute to positive duration dependence in the probability of leaving the labour force due to discouragement through periods of sickness/disability.

The extent to which unemployment benefits affect the escape rate to non-participation states such as sickness and disability depends of course on the exact way in which benefits are calculated in these alternative states. In Norway, sickness benefits for unemployed persons are exactly equal to their unemployment benefits; hence unemployed persons have apparently no pecuniary incentives to record themselves as 'sick' rather than 'unemployed'. However, disability rehabilitation programs may often entail higher benefits, and these programs are typically preceded by a period of sickness. It is therefore possible that some unemployed workers with particularly poor employment prospects and/or low unemployment benefits may consider a path of sickness, disability and subsequent rehabilitation (or permanent disability) more economically promising than continued job search. Labour market programs also typically involve continuation of the existing benefit level, although some forms of relief work entail higher benefits.

The relationship between economic incentives and search behaviour is further complicated by the existence of work tests. Refusal to take part in consultations at the employment office, refusal to accept offers of regular (but perhaps poorly paid) jobs or to participate in labour market programs, may imply that benefits are terminated. These threats are of course more important the higher are the benefits that can be forfeited. But the Public Employment Service may have orders to exert a stronger pressure on some unemployed – for example youths and long term unemployed – than on others.

We do not attempt to combine all these mechanisms into a coherent theoretical model, as such a model quickly would become intractable. Instead, we seek to construct a flexible transition rate model that is able to test the various hypotheses in the form of reliable *causal* reduced form parameters. To the extent that the qualitative and quantitative importance of the different causal mechanisms can be uncovered, this can subsequently contribute to a further development of the theoretical literature.

# 3 The Data and the Sources of Conditionally Independent Variation in the Replacement Ratio

The Norwegian unemployment insurance system is compulsory, and the benefit is calculated as 62.4 per cent of labour earnings the previous calendar year (or the average of the last three years if this average is higher than last years income), up to a ceiling of roughly 33,000 Euro. Apparently, this implies that there is no variation in benefits, conditional on previous wages. However, there are two features of the Norwegian benefit system that in fact do entail some degree of conditionally independent variation. The first source of variation applies only to persons with less than two years continues work-experience just prior to the unemployment spell: Since benefit entitlements are calculated on the basis of income earned in the previous *calendar year*, a given income in a given period just prior to the unemployment spell gives a higher benefit the more it is concentrated within the last calendar year. This is of course a purely administrative procedure with no behavioural justification, and it produces a variation in benefits which is similar to the way the tax level depends on the extent to which a given income is spread out on different tax years. The second source of variation is provided by indexation rules and applies to all unemployed: If benefits are granted during May-December, the base income (from the year before) is indexed according to the adjustment factor applying to the pension system before benefits are calculated. The same indexation is not implemented for spells starting in January-April. Moreover, indexation is not implemented for ongoing spells. These procedures imply that benefits are slightly higher for workers who become unemployed after 1 May than for workers becoming unemployed before that date and that, *relative to any sensible measure of expected wages*, the benefit level for ongoing spells is reduced in connection with the yearly wage settlements.

From a theoretical point of view, it is indeed the benefit level relative to the expected wage (or more general; the benefit level, conditioned on the expected wage) that affects the transition rate to a job; see e.g. Mortensen (1990). The expected wage is intrinsically unobserved, and depends of course on observed as well as unobserved individual characteristics. In order to purify our source of independent variation, we decompose the unobserved replacement ratio, i.e. the benefit level divided by the expected income, into one factor that is dependent- and one that is independent of individual characteristics, *conditional on work experience*. Let the replacement ratio for an individual $i$ at time $t$ be denoted $r_{it}$. Let $r_i^*$ denote the replacement ratio that individual $i$ would have obtained in a stationary environment (without wage growth) had he been continuously employed in the past calendar year, and let $a_{it}$ be the adjustment factor related to insufficient work experience in the past calendar year and to general wage- and benefit growth, such that $r_{it} = r_i^* a_{it}$. We then have that $r_i^* = 0.624 \dfrac{\min(y_{i,-1}, \bar{y})}{y_i^*}$, where $y_{i,-1}$ is income in the previous calendar year (the year before the start of the unemployment spell), $\bar{y}$ is the threshold income in the benefit system, and $y_i^*$ is the expected income. Let $e_i$ be the fraction of the last calendar year in which person $i$ was employed. Let $b_t$ be the adjustment factor used to index benefits granted after the 1 of May and let $g_t$ be the growth rate in aggregate wages (on a yearly basis), also taking place from the 1 of May. Assume that each person's expected wage grows in line with the aggregate wage rate (conditional on spell duration). We then have that the adjustment factor $a_{it}$ in the first month of the unemployment spell is determined as $a_{it} = e_i$ if the spell started in January-April and $a_{it} = \dfrac{e_i(1+b_t)}{1+g_t}$ if the spell started in May-December. In the subsequent duration months it is only changes in expected wages (related to new tariff agreements) that can change the replacement ratio, such that $a_{it} = a_{it-1}$ for all calendar months except May and $a_{it} = \dfrac{a_{it-1}}{1+g_t}$ in May.

Benefits can be maintained for up to 156 weeks in Norway. But until January 1997, there was a formal limitation of 80 weeks, followed by a 13-week cut-off period, after which a new 80+13-week period could start at a somewhat reduced benefit level. In practice, an exemption rule implied that benefits were rarely withdrawn during the cut-off period. Persons without benefits, e.g. because entitlements are exhausted, are entitled to means-tested social security support.

The data we use in the present analysis is obtained by merging a number of administrative registers. In principal, it gives an account of the main labour market activity for the whole Norwegian adult population by the end of each month during the period from 1992 to 1997. However, for the purpose of conducting the analysis in the present paper, we have restricted the survey population to new unemployment spells that satisfy a number of conditions. First, in order to be sure that the previous income is not affected by previous spells of unemployment, we condition on at least 24 months of absence from the unemployment register prior to a new spell. Second, we require at least two months of paid work prior to the unemployment spell in order to make sure that the monthly income (and the benefit level) is identified. Third, we restrict attention to persons that were involuntary unemployed (i.e. they did not quit their previous job voluntarily) and hence were entitled to benefits from the start of the unemployment spell. Fourth, we concentrate on persons with benefits calculated on the basis of last years income (implying that we exclude persons for which the average of the last three years income is higher than last years income). And finally, we limit the population to persons aged 20-59 years.

We track the unemployment benefit spells month by month until they are terminated with a job, with a withdrawal from the labour force in the form of sickness or disability, with loss of benefits, with participation in a labour market program, or censored. Censoring occurs when persons become 60 years of age, when spell duration exceeds three years, and at the end of the observation period.

We use a number of control variables to minimise the problem of unobserved heterogeneity. These controls include standard *demographic variables*, such as gender, age, county of residence, family situation, and nationality, as well as *human capital variables* such as educational attainment and years of work-experience. We also include controls for the month of entry into unemployment (12 seasonal dummies) and the extent of work-experience in the year just prior to the unemployment spell (12 dummies). The reason for this is that the presumed independent part of the replacement ratio, $a_{it}$, is strongly affected by the calendar month of entry as well as the extent of work in the past 12 months, and these variables may again be related to unobserved heterogeneity (e.g. in the form of a seasonal pattern in the 'quality' of inflow cohorts); hence the independence assumption is only credible conditioned on these variables. In order to improve upon the characterisation of individuals' human capital, we take advantage of income records (based on pension point accumulation) for the years back to 1967. The basic idea is that the ranking of individual abilities, conditioned on education and work-experience, is revealed through the actual income path (Røed and Nordberg, 2002). We use the following procedure to proxy the level of human capital embedded in individual ability: We first divide the whole Norwegian population into 120 relatively homogenous groups with respect to gender, educational attainment and work experience, and retrieve for each person the maximum yearly income earned after the education was completed (adjusted for aggregate wage growth). We then compute a set of dummy variables indicating the decile in the *within-group* maximum earnings distribution to which each person belongs. In order to avoid arbitrary functional form relationships, we use these dummies directly in the econometric models explaining labour market transitions.

Some descriptive statistics of the data-set is provided in Table 1. There are 44,816 spells that satisfy our data selection criteria. Only 169 of them are repeat spells. The low number of repeat spells results directly from our requirement of absence from the unemployment register for at least two years in order to be counted as a 'new' unemployed.

Table 1
Selected Descriptive Statistics

| | |
|---|---|
| Period* | 1992 (1993) -1997 |
| Number of individuals | 44,647 |
| Number of spells | 44,816 |
| Number of monthly observations | 281,834 |
| | |
| Per cent of spells ending in | |
| Transition to a job | 40.40 |
| Transition to a sickness or disability | 9.31 |
| Termination of benefits | 14.99 |
| Transition to a labour market program | 9.06 |
| Censored | 26.24 |
| | |
| Mean replacement ratio taken over observations (Standard Deviation) | |
| $r_{it}$ | 0.51 (0.15) |
| $r_i^*$ | 0.59 (0.08) |
| $a_{it}$ | 0.86 (0.22) |
| Per cent with less than 12 months work experience in the past calendar year | 32.59 |
| | |
| Other selected means and fractions (per cent) taken over observations | |
| Men (per cent) | 43.34 |
| Married (per cent) | 42.35 |
| Family wealth>0 (per cent) | 17.31 |
| Dependent children (per cent) | 38.50 |
| | |
| Educational attainment (per cent) | |
| Only compulsory education | 21.00 |
| Lower secondary education | 26.61 |
| Upper secondary education | 35.56 |
| Lower university degree | 14.17 |
| Higher university degree | 2.66 |
| Work experience (years) | 9.95 |
| Immigrants from Non OECD countries (per cent) | 5.20 |

* Since we use lagged information on work-experience the past 12 months as explanatory variables, only data from 1993 is used in the actual estimation.

# 4 The Econometric Model

We estimate a competing risks transition rate model with four competing destination states. Time has two dimensions in our analysis; calendar time $t$, and process time (spell duration) $d$. Let $k=1,...,4$ be the four alternative destination states of employment, sickness/disability, benefit termination and program participation, respectively. Let $i$ be the subscript over individuals. The four hazard rates are then defined as

$$q_{ik}(t,\, d) = \lim_{\Delta d \to 0} \frac{P(d \leq D \leq d + \Delta d\,, K = k \,|\, D \geq d, i, t)}{\Delta d}. \tag{1}$$

As we observe labour market status by the end of each month only, we set up the econometric model in terms of discrete (grouped) hazard rates (Prentice and Gloeckler, 1978; Meyer, 1990; Narendranathan and Stewart, 1993). Let $\bar{t}_i$ be the calendar time at which individual $i$ entered the state of unemployment. The grouped composite hazard, i.e. the probability of exiting to one of the four states during duration month $d$, given that no exit occurred before that, is given as

$$h_{id} = 1 - \exp\left( -\sum_k \int_{d-1}^{d} q_{ik}(\bar{t}_i + u, u)du \right). \tag{2}$$

We assume for simplicity that the hazard rates are constant within each calendar month. Let $x_{it}$ be a vector of observed control variables and let $v_{ik}$ be scalar measures of unobserved heterogeneity affecting the hazard rate to state $k$. Let $s_{kt}$ measure the calendar time effects, and let $l_{kd}$ measure the spell duration effects. Imposing exponential link functions between individual characteristics and the hazard rates, we have that

$$\int_{d-1}^{d} q_{ik}(\bar{t}_i + u, u)du = \exp(g_{ik} \log(r_{it}) + x_{it}\,' b_k + s_{kt} + l_{kd} + v_{ik}). \tag{3}$$

Taken at face value, the parameter $g_{ik}$ is the elasticity of individual $i$'s hazard rate to transition $k$ with respect the replacement ratio. But if the benefit level and the expected income in a job affect the hazard rate with coefficients that are not equal in absolute terms, $g_{ik}$ may in fact be given a more general interpretation as the elasticity with respect to the level of unemployment benefits. To see this, assume that the level of expected income affects the hazard rate with the elasticity $j_{ik}$, such that the term $g_{ik}\log(r_{it})$ in (3) is replaced by $g_{ik}\log(\text{benefits}) - j_{ik}\log(\text{expected income})$. This expression can then be reorganised into $g_{ik}\log(r_{it}) - (j_{ik} - g_{ik})\log(\text{expected income})$, hence the formulation in (3) appears to require the restriction that $j_{ik} = g_{ik}$. However, in our case, we may interpret a number of our control variables (such as previous income, educational attainment, position in skill-specific wage income distribution, and work experience) as instruments for the intrinsically unobserved level of expected income. What remains to be explained is how we can calculate the replacement ratio itself without observations on expected income. Now, according to the decomposition discussed in the previous section, we have that the replacement ratio can be factorised into the two terms $r_i^*$ and $a_{it}$, reflecting individual factors and 'random assignment' factors respectively. The individual factor, $r_i^*$, cannot be calculated without making an assumption about the level of the expected wage, and for simplicity we assume that the expected income is equal to the last years income[2], i.e. $y_i^* = y_{i,-1}$. The random assignment factor, however, is *independent of the expected wage*. Hence, we may use the factorisation of the replacement ratio to obtain two sets of parameter estimates for the same coefficients $(g_{ik}(\log r_{it}) = g_{ik}\log(r_i^*) + g_{ik}\log(a_{it}))$, one that is known to be consistent, and one that may be inconsistent due to correlation with unobservables or to systematic errors in the prediction of expected wages.

[2] This implies that if the true expected wage declines, and hence the replacement ratio rises, as a function of spell duration (see e.g. Gregory and Jukes, 2001, for some evidence indicating that this may be the case), this effect will in our case show up in the estimated spell duration baseline.

In this paper, we wish to identify the 'average' disincentive effects, as well as the degree to which these effects vary with demographic factors and economic circumstances. Hence, we assume that each elasticity $g_{ik}$ depends on a vector of observed covariates $z_{it}$, such that $g_{ik} = a' z_{it}$. In line with the our theoretical considerations in Section 2, the variables included in $z_{it}$ are spell duration, business cycle condition[3], age, gender, wealth[4], marital status and income of the spouse. While the roles of the replacement ratio and the respective interaction terms are relatively straightforward in the employment hazard, these variables have a more vague role to play in the other hazard rates. But, in the absence of better predictors for the economic incentives associated with the various non-employment states, we apply the same incentive variables in all transitions.

We now turn to the estimation of the model, based on observations of explanatory variables and transitions. In order to avoid unnecessary parametric restrictions, we assume that the unobserved variables $v_{ik}$ are discretely distributed (Lindsay, 1983), with the number of mass-points chosen by adding points until it is no longer possible to increase the likelihood function (Heckman and Singer, 1984). Let $B_i$ be the number of unemployment spells experienced by individual $i$ during the observation period. *Let* $y_{ibk}$ be binary indicator variables denoting transitions to the four alternative destinations states, i.e. $y_{ibk}=1$ if individual $i$ transited to state $k$ in spell $b$, and zero otherwise. The contribution to the likelihood function from a spell starting at time $\bar{t}_{ib}$

[3] We use a monthly business cycle indicator provided by Røed and Zhang (2003). This indicator reflects the aggregate monthly flow from unemployment to employment, corrected for selection effects due to observed heterogeneity and spell duration.

[4] Only taxable wealth is measured in the registers, implying that most persons are recorded with zero wealth. For this reason we represent wealth by a dummy variable indicating that family wealth is strictly positive.

and lasting $d_{ib}$ months, conditional on a particular vector of unobserved heterogeneity $v_l = (v_{l1}, v_{l2}, v_{l3}, v_{l4})$, may then be written:

$$L_{ib} \mid v_l = \prod_k \left[ \left(1 - \exp\left(-\sum\nolimits_k j_k(\bar{t}_{ib}, d_{ib}, x_{it}, v_{lk})\right)\right) \frac{j_k(\bar{t}_{ib}, d_{ib}, x_{it}, v_{lk})}{\sum_k j_k(\bar{t}_{ib}, d_{ib}, x_{it}, v_{lk})} \right]^{y_{ibk}} \times \prod_{s=1}^{d_{ib} - \sum_k y_{ibk}} \left[ \exp\left(-\sum\nolimits_k j_k(\bar{t}_{ib}, d_{ib}, x_{it}, v_{lk})\right) \right] \tag{4}$$

where $j_k(\bar{t}_{ib}, d_{ib}, x_{it}, v_{lk}) = \exp\left(a_k' z_{it} \log(r_i^*) + a_k' z_{it} \log(a_{it}) + x_{it}' b_k + s_{kt} + l_{kd} + v_{lk}\right)$.

We assume that the four unobserved variables are discretely distributed with $W$ points of support, and estimate these mass points together with their associated probabilities. In terms of observed variables, the likelihood is then given as

$$L = \prod_{i=1}^{N} \sum_{l=1}^{W} p_l \prod_{b=1}^{B_i} L_{ib} \mid v_l, \quad \sum_{l=1}^{W} p_l = 1 \tag{5}$$

where $p_l$ is the probability of a particular combination of unobserved variables.

The data at hand provides a unique opportunity for disentangling the effects of structural duration dependence and unobserved heterogeneity, without relying on particular distributional assumptions. One (minor) reason for this is that there are a few repeat spells in the data (Honoré, 1993). However, a much more important and reliable source of identification is that there is large variation in lagged explanatory variables, conditioned on individuals' current explanatory variables. Intuitively, this source of identification rests on the idea that the conditional expectation of unobserved heterogeneity depends on hazard rates experienced earlier in the spell, while structural duration dependence does not (van den Berg and van Ours, 1994; 1996). In the present case, variation in lagged hazard rates is primarily driven by variation in calendar time itself, i.e. business- and seasonal cycles. Persons with the same spell duration have been exposed to different business cycle conditions earlier in the spell; hence they have been subject to different selection mechanisms. For example, a person who is still unemployed after $d$ months of unemployment will clearly have a lower expected value of $v_k$ the higher the probabilities of exiting to state $k$ has been earlier in the spell. Brinch (2000) provides a formal proof for the idea that variation in covariates over time, combined with variation in covariates across individuals, is sufficient for the identification of structural duration dependence in the presence of unobserved heterogeneity, without parametric assumptions on either of these components and even without the assumption of proportional hazards.

## 5 Estimation Results

The step-wise inclusion of mass-points ended up with five different types of unobserved covariate vectors. Through this process, the log-likelihood function was improved by around 100 units, from -128565.9 without unobserved heterogeneity to –128469.2 for the preferred model. From this point, we were not able to increase the likelihood any further, neither through local grid searches nor through new and independent estimations based on scrambled starting values. A total number of 680 parameters were estimated. The results that we present in this section are based on this model[5]. Given the large number of estimated parameters, we do not spell out the complete results (these are available on request). The plan of this section is as follows: We first present estimates regarding structural duration dependence and the effects associated with benefit exhaustion. We then turn to the effects associated with economic conditions in general and marginal changes in unemployment benefits in

[5] Estimation of this model was a huge computational task, and, to our knowledge, a competing risk model of this scale and flexibility has never before been estimated in practice. We could not have done this without the support of Simen Gaure at the Computing Resource Centre at the University of Oslo.

particular. Finally, we present some results regarding the impact of observed- and unobserved heterogeneity.

### 5.1 Duration Dependence and Benefit Exhaustion

Figure 1 presents the estimates of structural duration dependence in the four hazard rates. The expected time of temporary benefit exhaustion is marked as an interval (month 18-20) in the figure, since time aggregation prevents us from computing the exact timing of this potential event for each individual. The job hazard rate displays a clear pattern of negative duration dependence during the first year of unemployment. Thus, throughout most of the spell it seems that the positive duration dependence implied by limited benefit duration is more than compensated for by discouragement effects, declining human capital or statistical discrimination based on spell duration (conf. Section 2). This pattern is turned upside down in the months just prior to benefit exhaustion. The employment hazard rises by 50-100 per cent during the last three-four entitlement months, indicating substantial, but rather myopic responses to the prospect of benefit exhaustion. Our finding at this point is in line with previous results reported in Røed and Zhang (2003), and suggests that even the very mild limitations embedded in the Norwegian benefit system (with generous exemption rules) entail a relatively strong 'last-minute'-type behavioural response.

The interpretation of the generally declining employment hazard rate in terms of a discouragement effect is supported by the estimated duration pattern in the probability of becoming sick or disabled. Although this pattern is imprecisely estimated (as indicated by the large confidence intervals), there is strong evidence of positive duration dependence. The lower bound of the 95 per cent confidence interval suggests that the hazard rate is more than doubled during the first half year of unemployment. The spell duration pattern in the benefit termination- and employment program hazard rates mirrors administrative procedures. The benefit termination hazard rises monotonously during the first benefit period, with a sharp increase when the benefit period is exhausted. It then declines somewhat as a new benefit period begins. The labour market program hazard also displays positive duration dependence, as these programs are primarily aimed at long-term unemployed. Again, there is a relatively sharp increase in the hazard rate around the time of benefit exhaustion.

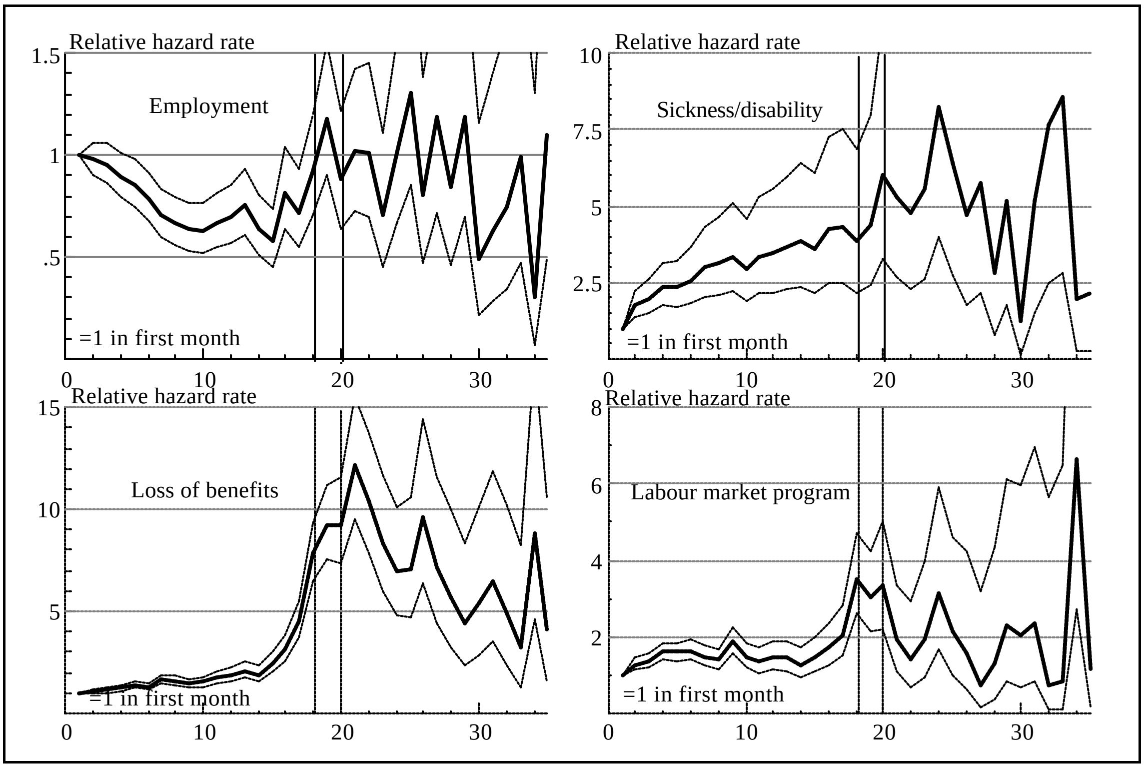


Figure 1. Estimated baseline hazard rates with 95 per cent (point-wise) confidence intervals (calculated for a person with average replacement ratio)

Note: Verticle lines indicate the expected time of temporary benefit exhaustion. We suppress parts of the upper confidence limits for expository reasons. Note also that the scales are not the same across the four panels.

## 5.2 The Benefit Elasticity and individual economic conditions

As explained in Section 4, the model generates two sets of estimators for the same sets of elasticity parameters, one based on the part of the replacement ratio that may be correlated to unobserved individual characteristics $(r_i^*)$ and one based on the random-assignment-like variation $(a_{it})$. We report the full set of parameter estimates for the estimates based on the random-assignment variation only, since these are the parameters considered to reflect *causality*. Part I of the table reports the elasticities estimated for the 'mean covariate vector', while part II reports the estimated interaction effects between the replacement ratio and other variables. Part III reports the *average predicted elasticities* (taken over all observations in the dataset), and in order to illustrate the potential bias generated by the dependence between observed replacement ratios and unobserved characteristics, we also report these summary statistics for the predicted elasticities based on the non-independent variation $(r_i^*)$. Part IV reports other parameter estimates reflecting effects of economic conditions.

Table 2
Effects of economic incentives
(Standard errors in parentheses)

| | Employ-ment | Sickness/ disability | Loss of benefits | Program particip. |
|---|---|---|---|---|
| I. Benefit elasticity based on independent variation ($a_{it}$), reference group (unmarried woman, no wealth, average age, average business cycle and average spell duration) | -0.42**<br>(0.05) | -0.22*<br>(0.11) | -0.40**<br>(0.07) | 0.39**<br>(0.11) |
| II. Interaction terms of replacement ratio $a_{it}$ | | | | |
| with business cycle | -0.10<br>(0.20) | -0.68<br>(0.50) | 0.69*<br>(0.35) | -0.63<br>(0.58) |
| with log duration | -0.14**<br>(0.02) | 0.05<br>(0.05) | -0.05<br>(0.04) | -0.11<br>(0.06) |
| with log age | -0.25**<br>(0.10) | -0.91**<br>(0.23) | -0.07<br>(0.16) | 0.52*<br>(0.23) |
| with dummy for male | -0.00<br>(0.04) | 0.33**<br>(0.13) | 0.18**<br>(0.07) | -0.19<br>(0.11) |
| with dummy for (family) wealth>0 | -0.11<br>(0.08) | -0.35*<br>(0.17) | -0.10<br>(0.12) | -0.33<br>(0.18) |
| with dummy for high income spouse | 0.21**<br>(0.08) | -0.29*<br>(0.13) | 0.06<br>(0.11) | -0.13<br>(0.16) |
| with dummy for low income spouse | 0.16*<br>(0.08) | -0.17<br>(0.15) | -0-02<br>(0.11) | 0.08<br>(0.17) |
| with dummy for no income spouse | 0.24<br>(0.15) | -0.54*<br>(0.23) | -0.06<br>(0.20) | 0.28<br>(0.37) |
| III. Average predicted benefit elasticity taken over all observations [standard deviation] | | | | |
| Based on independent variation $(a_{it})$ | -0.43<br>[0.08] | -0.26<br>[0.19] | -0.40<br>[0.06] | 0.40<br>[0.11] |
| Based on suspected non-independent variation $(r_i^*)$ | -0.00<br>[0.04] | 0.94<br>[0.11] | 0.31<br>[0.08] | 0.05<br>[0.09] |
| IV. Level effects of having | | | | |

Table 2
Effects of economic incentives
(Standard errors in parentheses)

| | Employ-ment | Sickness/ disability | Loss of benefits | Program particip. |
|---|---|---|---|---|
| family wealth>0 | 0.08** | -0.26** | -0.00 | -0.02 |
| | (0.03) | (0.06) | (0.04) | (0.05) |
| high income spouse | 0.17** | 0.17** | -0.05 | -0.14** |
| | (0.03) | (0.05) | (0.04) | (0.05) |
| low income spouse | 0.14** | 0.34** | -0.03 | -0.02 |
| | (0.03) | (0.06) | (0.06) | (0.05) |
| no income spouse | 0.09 | 0.37** | -0.16* | -0.10 |
| | (0.05) | (0.09) | (0.07) | (0.09) |
| children (for women) | -0.90** | -0.33** | -0.43** | -0.27** |
| | (0.03) | (0.06) | (0.05) | (0.06) |
| children (for men) | -0.28** | -0.08 | -0.02 | 0.11* |
| | (0.03) | (0.07) | (0.05) | (0.05) |

*(**) significant at the 5(1) per cent level in a two-sided test.

The *causal* benefit elasticities are negative for the transitions to employment, sickness/disability and loss of benefits. This is in line with prior expectations. The employment hazard elasticity is on average around –0.4, which indicates slightly weaker responses than previously reported by Røed and Zhang (2003). However, there is substantial heterogeneity in individual elasticities, suggesting that average elasticity estimates are likely to vary according to the composition of the population under study. For transitions to labour market programs, there is a positive benefit elasticity, probably reflecting that higher benefits make it more costly to reject program participation.

For the employment hazard, the absolute value of the benefit elasticity increases significantly with age, hence we apparently reject the theoretically founded prediction, discussed in Section 2, that disincentive effects are stronger for young persons due to the tight wage distributions they face (Narendranathan et al, 1985). We speculate that our finding at this point is related to the fact that the Public Employment Service exerts a relatively strong pressure on young unemployed persons to accept available jobs or program slots, hence they are left with less room for individual optimisation. The benefit elasticity also increases with spell duration. Again our results contradict theoretical predictions as well as previous empirical evidence (Narendranathan and Stewart, 1993). A possible explanation is that liquidity constraints accentuate the role of economic incentives, and that these constraints become more prevalent as the spells are prolonged. We do not find any significant changes in the elasticities over the business cycle (conditioned on spell duration), except that the elasticity of the loss-of-benefits-hazard with respect to the replacement ratio becomes smaller (in absolute terms) in good times than in bad times.

The disincentive effects in the employment hazard are stronger for single- than for married persons, but the income of the spouse does not have a significant impact. There is weak evidence in favour of a hump-shaped relationship, implying that the benefit elasticity is larger (in absolute terms) for persons with a low-income spouse than for persons with either a high-income spouse or a zero-income spouse. The employment hazard is generally higher for married than for unmarried persons, but lower if there are children in the family. The latter is particularly the case for females, for which the predicted employment hazard is more than halved as a result of responsibility for children. This result constitutes fairly strong evidence that opportunity costs do matter significantly for search behaviour and/or reservation wages. Economic wealth does not seem affect marginal disincentive effects associated with the replacement ratio. It apparently has a positive impact on the level of the employment hazard and a negative impact on the sickness-disability hazard. This is, however, likely to reflect correlation with unobserved characteristics rather than causality.

The predicted benefit elasticities based on the suspected non-independent variation in replacement ratios $(r_i^*)$, reported in part III of the table, reveal that neglect of unobserved heterogeneity may produce a substantial bias in response parameters. In our case, it seems that the elasticity of the job hazard rate with respect to the replacement ratio would have been seriously underestimated had we based our inference on the observed variation in replacement ratios and relied on control variables only to remove spurious correlation. This may perhaps explain why European evidence so often has failed to come up with significant disincentive effects at all (see e.g. Hujer and Schneider, 1989; Groot, 1990; van den Berg, 1990b; Steiner, 1990; Hernæs and Strøm, 1996).

### 5.3 Observed and unobserved heterogeneity

There are substantial variations in individuals' hazard rates. Figure 2 presents estimation results regarding educational attainment and Figure 3 presents the results regarding individual ability (proxied by position in the national education- and experience specific maximum wage distribution, see Section 3). Both educational attainment and ability contribute to higher employment hazard rates. There are two possible explanations for that, one demand-side and one supply-side. The demand-side explanation is that the relatively compressed Norwegian wage distribution makes high-skilled- and high-ability workers attractive labour from the employers' point of view (Røed, 1998; Røed and Nordberg, 2002). The supply side explanation is that the ability- and skill variables operate as proxies for expected wages (see Section 4). There is no clear pattern in the way education and ability affects the transitions to sickness and disability. For ability, there is weak evidence in favour of a non-monotonic pattern. It may be noted that high ability implies a relatively high risk of being sanctioned. A likely explanation is that high-ability workers are more selective, given their relatively strong labour market performance in the past. High-ability workers also have a relatively high exit rate to labour market programs, indicating an element of positive selection to these programs (our ability measure will typically be unobserved in most studies).

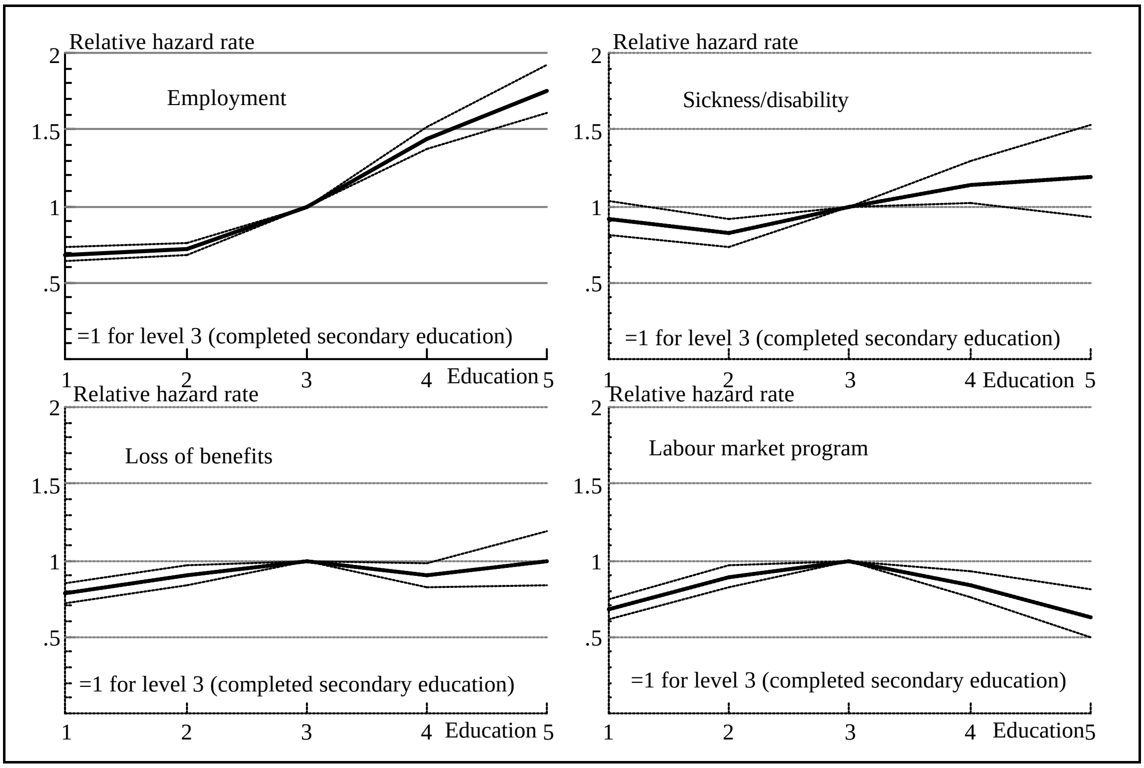


Figure 2. Estimated effects of educational attainment, with 95 per cent confidence intervals.

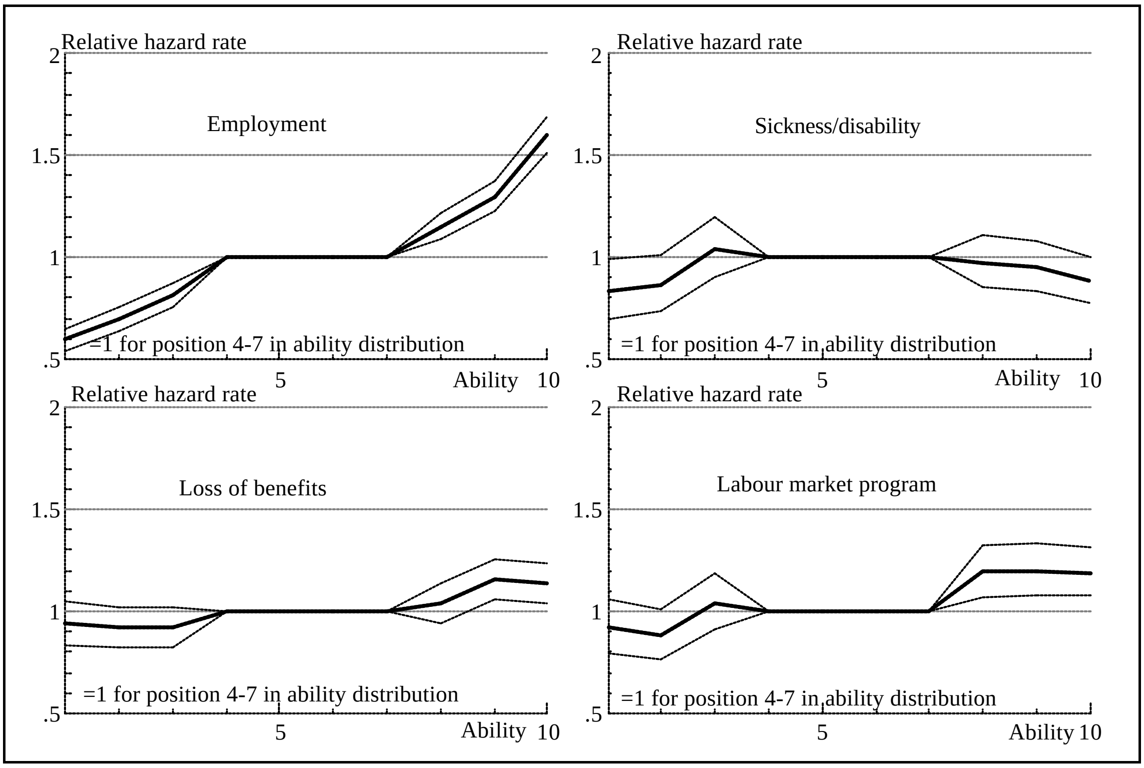


Figure 3. Estimated effects of ability, with 95 per cent confidence intervals.

Demographic factors also have a substantial influence on the hazard rates. The coefficients attached to a male dummy are estimated to 0.18 (0.02) in the transition to employment and to –0.89 (0.06) in the transition to sickness/disability (there are no significant gender effects in the other two transitions). These estimates imply that men have a transition rate to employment that is around 20 per cent higher, and a transition rate to sickness/disability that is around 60 per cent lower, than otherwise equal women. The effect of age is illustrated in Figure 4. All the hazards decline as functions of age. The strong decline in the sickness/disability hazard may appear surprising. Our interpretation of this result is that the propensity to report sickness during unemployment spells is higher for younger- than for older persons, both because they are subject to a stronger pressure towards accepting jobs and because they have a higher probability of being selected for rehabilitation programs.

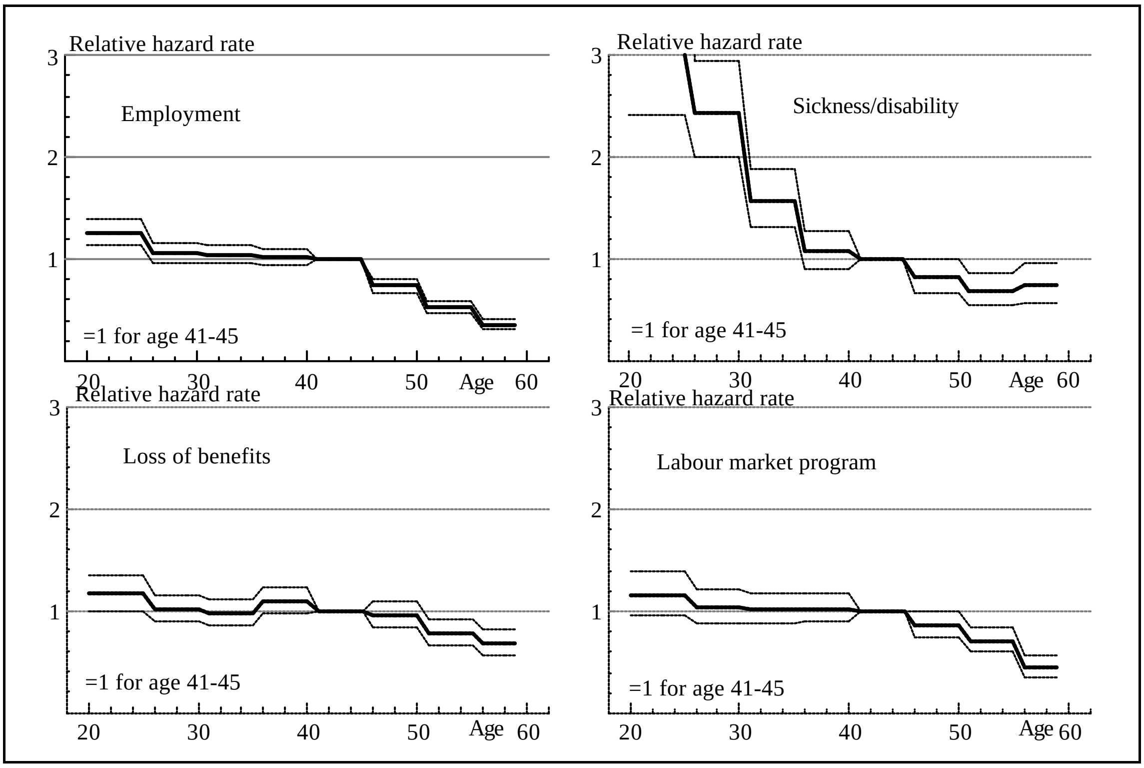


Figure 4. Estimated effects of age, with 95 per cent confidence intervals.

The estimated distribution of unobserved heterogeneity is described in Table 3. Although it is difficult to interpret the various 'types' directly[6], it may be of interest to have a look at the relationship between unobserved heterogeneity in the different transitions. It turns out that unobserved heterogeneity in transitions to employment, sickness/disability and loss of benefits display a mover-stayer-property, i.e. it is a strong positive correlation between unobservables regarding these transitions. There is a negative correlation between unobservables regarding employment transitions and program participation (the correlation coefficient between $\exp(v_{i1})$ and $\exp(v_{i4})$ is –0.18), indicating that there is negative selection on unobservables to labour market program in these data.

Table 3
The Estimated Distribution of Unobserved Heterogeneity
(standard errors in parentheses)

| | Probability (per cent) | Employment $v_{i1}$ | Sickness/ disability $v_{i2}$ | Loss of benefits $v_{i3}$ | Program participation $v_{i4}$ |
|---|---|---|---|---|---|
| Type 1 | 19.08 | -3.98 | -7.84 | -4.86 | -6.03 |
| | | (0.21) | (0.60) | (0.29) | (0.42) |
| Type 2 | 0.89 | 2.34 | 0.73 | - infinity | -6.49 |
| | | (2.22) | (2.28) | | (0.41) |
| Type 3 | 15.07 | -1.79 | -4.56 | -5.07 | -6.17 |
| | | (0.17) | (0.45) | (2.69) | (1.59) |
| Type 4 | 62.63 | -2.84 | -6.99 | -3.03 | -5.21 |
| | | (0.17) | (0.50) | (2.24) | (0.29) |
| Type 5 | 2.32 | 0.00 | -4.05 | -3.24 | -5.36 |
| | | (0.60) | (1.90) | (0.24) | (0.27) |

[6] Our experience is that different combinations of 'types' and probabilities sometimes produce equally 'good' likelihood functions, indicating that there is a fundamental lack of identification of the unobserved heterogeneity distribution (there are different local maximums with almost the same function value). However, the moments of these alternative distributions are typically almost identical (they have very similar Laplace transforms), and the parameters attached to observed variables (including spell duration) also tend to be the same.

## 6 Concluding Remarks

Based on random-assignment-like variation in unemployment benefit replacement ratios, we have found that the average elasticity of the employment hazard rate with respect to the replacement ratio was around –0.4 in Norway during the 1993-97 period. We have also found that the disincentive effects become stronger as the unemployment spell is prolonged, that they are stronger for older- than for younger people, and that they stronger for single than for married persons. The disincentive effects are stable over the business cycle. Economic conditions embedded in family wealth and spouse income do not affect the benefit elasticity.

We have identified the degree of structural duration dependence in the propensity to find a job, to become sick or disabled, to lose benefits, and to enter into labour market programs, without reliance on any parametric assumptions about either unobserved heterogeneity or the distribution of individual durations. We find that there is a substantial negative duration dependence in the employment hazard, apart from a significant rise in the months just prior to benefit exhaustion. There is positive duration dependence in the sickness/disability hazard. Together, these findings suggest that discouragement, depreciation of human capital and/or statistical discrimination against long-term unemployed are significant real-world phenomena. The propensity to lose benefits (sanctions) and enter into labour market programs also exhibit positive duration dependence, but these patterns are more governed by administrative procedure than by individual behaviour.

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