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The Roles of Health and Economic Incentives in Transitions from Unemployment out of the Labour Force*

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Abstract
This study uses 8 years of panel data to estimate a duration model for transitions from insured unemployment to sickness benefits (SB). There is positive duration dependence in the SB hazard rate, suggesting that the risk of developing illnesses increases with time unemployed. The hazard depends negatively on municipality unemployment, which indicates that the stigma of being unemployed depends on the social norm for working. Economic incentives to choose transition to SB arise from the combination of time limited UI and the fact that SB-rights do not depend on remaining UI. We find evidence of the incentive effect as the SB hazard increases by 54% when UI is about to end. Data on subsequent SB spells support the hypothesis of strategic behaviour of some SB entrants.

1 Introduction

In Western countries there is currently a public debate over policy measures to counter the fiscal challenges incurred by decreasing ratios of working to non-working population. This paper may be seen as a contribution to the knowledge base for this debate. We present an analysis of Norwegian unemployed people's transitions out of the labour force with focus on the two factors reduced health and economic incentives. In Norway, sickness benefits (SB) are in many cases the first stage in the transition from insured unemployment out of the labour force, and we estimate a model for the transition from insured unemployment to receiving SB.

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Becoming unemployed may imply reduced happiness and general well-being (Winkelmann and Winkelmann 1998 and Clark 2003), lower self-esteem, and may cause mental and physical illnesses, see surveys by Warr 1987, Warr, Jackson, and Banks 1988, Feather 1990, Goldsmith, Veum, and Darity 1996, Dooley, Fielding, and Levi 1996, and Kasl and Jones 2000.

The literature describes how the risk of developing mental and physical sickness may evolve as the period of unemployment extends, and cross sectional studies tend to find zero or a positive correlation between elapsed duration and well-being and health, see Clark and Oswald 1994 and the survey by Dooley, Fielding, and Levi 1996. However, results based on cross-section data are subject to selection problems. In particular, those who suffer most from being unemployed are likely to escape first, as confirmed in studies on panel data for Germany and the UK (Clark, Georgellis, and Sanfey 2001; Clark 2003). This paper uses the incidence of being granted SB as an outcome measure of health and estimates the structural duration dependence in the hazard rate for transition from unemployment to SB. A related study is Røed and Zhang 2005 who find positive duration dependence in the sickness/disability hazard.

The stigma of being unemployed is likely to depend on a person's own ability to cope with being unemployed, and on the social pressure associated with being unemployed. Ability to cope may be part innate "employment commitment" (Warr, Jackson, and Banks 1988), and may improve with past unemployment experience (Clark, Georgellis, and Sanfey 2001). Clark and Oswald 1994 find that the disutility from unemployment is lower in high-unemployment regions, and Clark 2003 replicates this result with panel data. Social interaction provides one potential explanation: It is more stressful to be unemployed in areas where there is a strong social norm for supporting oneself, and this can have a direct impact on well-being and health. Using data on voting behaviour Stutzer and Lalive 2004 find that the difference in well-being between unemployed and employed people increases with the strength of the social norm for working. Like Lindbeck, Nyberg, and Weibull 1999 we assume the existence of a social norm for working, and use the share of the reference group who do not adhere to the norm as a proxy for the strength of the norm. The reference group is defined as the municipality of residence and the municipality unemployment rate and share of working age population outside the labour force proxy the norm. The estimation controls for common time effects and for unobserved municipality-specific effects, such that variation within municipalities over time identifies the social interaction effect.

In principle a person is only granted SB if he has verifiable health problems which prevent labour force participation. However, the combination of time limited UI and the fact that SB-rights do not depend on remaining UI, creates economic incentives to choose transition to SB. Larsson 2002 finds significant effects of the same incentive in Sweden, and Røed and Zhang 2005 find a peak in the transition rate from unemployment to pooled SB/disability benefits in the months before UI exhaustion. Unlike these studies we exploit a policy reform that changed the maximum duration of UI to separate this *incentive effect* from the duration effect and estimate its size.

Although the data do include information on type of diagnosis this information may not be very valuable, because it is difficult and perhaps not even relevant for patients and physicians to pin down the primary diagnosis. For example, it has been established that psychological stress can have affect physical health (Berkman and Kawachi 2000) but the literature is inconclusive on whether this arises from behavioural changes in response to the stress factor. In this study all diagnoses are pooled into one exit state.

For the empirical analysis we specify a discrete-time proportional hazards duration model with non-parametric unobserved heterogeneity and dependent competing risks as described in Gaure, Røed, and Zhang 2005. Labour market training programmes may modify the impact of unemployment on health and we estimate the impact of training on the hazard rate, and model selection into training. The specification of duration dependence is flexible and unobserved heterogeneity is represented by a discrete distribution. The ‘correct’ number of mass point is determined by successively increasing the number of mass points until the likelihood (or an associated criterion) no longer improves. Seasonal and business cycle effects and the use of a data on several cohorts ensures that the model captures unobserved heterogeneity.

2 Effects of Unemployment Duration on Health

The first studies of the impact of unemployment on health were conducted in high-unemployment communities in the 1930s. These studies led to “stage theories”, which hold that the long term unemployed go through different mental stages as unemployment proceeds: From an initial shock to optimism about finding work, to a period of distress and pessimism, and eventually come to accept their situation as non-employed in a fatalistic sense (Jahoda 1988; Feather 1990). Later research focused on why it is harmful to be out of work, and hence on the beneficial aspects of working life. Jahoda 1988 argues that being out of work implies the absence of various beneficial aspects of employment:job loss entails the loss of certain non-pecuniary benefits, such as time structure, social network, purpose and goals beyond private ones, status, identity and activity. Warr 1987 provides an alternative framework as he identifies a number of characteristics of a person's environment that affect mental well-being, which in turn suggest that unemployment is harmful. Apart from the points in Jahoda's formulation, Warr 1987 states that the state of being employed is an environment that provides a sense of control, secure income and a framework to utilize one's skills, variety, and yet predictability. Winkelmann and Winkelmann 1998 confirm the relevance of this approach as they demonstrate that the non-pecuniary benefits from work contribute much more to life-satisfaction than earned income.

Other researchers have explored what happens when a person is deprived of the benefits of working, i.e. what drives the stage theory. One theory is that unemployment may affect "locus of control", which measures an individual's perception of his ability to control his own life, see e.g. Goldsmith, Veum, and Darity 1996. The locus of control is a continuous scale where the extremes are the internalizer, who is confident in his personal efficacy and ability to decide his own future, and the externalizer, who may be described as a fatalist. It is clear that the internalizer is better equipped for handling continued stress. Warr, Jackson, and Banks 1988 distinguish between "constructive adaptation" and "resigned adaptation". The former label refers to the behaviour of unemployed who find meaningful activities that substitute work, whereas the latter characterizes those who stop searching for work and reduce their aspirations in order to protect themselves from stress and disappointments. Although a person's type is thought to be established during childhood and early youth, any individual can move along the locus of control, e.g. when exposed to stress over a continued period of time. Since unemployment is partly beyond the control of the individual, unemployment may be such a stressor. The notion that an unemployed may lose his belief in his ability to change things is also known as learned helplessness.

3 Institutions and the Incentive Effect

SB may be granted someone who has been employed for at least two consecutive weeks or receive UI, and has some illness or injury that makes him temporarily unable to work (social problems not included). It is the physician (general practitioner) who examines the patient and issues a recommendation for SB which is usually followed by the local social security office (Trygdekontor). A person who is unemployed and does not receive UI is not eligible for SB. SB are paid from the first day of sickness for a maximum of 12 months regardless of the length of employment and unemployment spells before that. UI is paid for a certain maximum number of weeks. When a person is granted SB the remaining number of weeks with UI is held fixed during sickness such that the person can resume the “old” UI spell. Furthermore, SB make up the same amount as UI. When UI expires the unemployed can obtain alternative, less generous, income support by engaging in some form of activity, such as education or labour market training programmes. Hence, those who lose UI face a pecuniary loss and non-pecuniary losses in terms of reduced leisure time and possibly increased stress and uncertainty, creating strong incentives to apply for SB when the end of UI approaches.

It may now seem easy to detect persons who choose SB for economic reasons, but it is not evident that the incentive effect can be interpreted as a result of economic incentives alone. As an example, a sick person may delay his application for SB until UI are exhausted. There are two theoretical reasons for doing so. First, individuals with limited health problems and ample remaining UI have no immediate pecuniary incentives to transfer to SB. Second, there are non-pecuniary costs to becoming registered as sick. The application process is time consuming, and applying for SB may imply a change in perception of oneself from a temporarily unemployed worker to someone with a health problem, such that the applicant focuses all attention on his role as a disabled person and abandons his ambitions of returning to employment. Westin 1990 followed a group of displaced workers who eventually applied for disability benefits, and describes such a development. The change of role from unemployed to disabled is probably only strengthened by the fact that SB recipients are part of another bureaucratic system than the unemployed. Given these “soft” costs to applying for SB people might postpone applications until they run out of UI, implying that what might seem like incentive effects in the data could be delayed registrations of true sickness.

However, since UI recipients are obliged to be available for the labour market there is reason to believe that SB applications cannot be postponed. If it is deemed that a UI recipient does not fulfill his obligations, the unemployment office can withhold his UI. If health limitations seem to prevent availability for work the unemployment office will send the unemployed to a physician for examination, and the physician may then write a certificate which is needed to apply for SB. Given these sanctions and control by a physician, there is a limit to how long you can defer an application for SB. In addition, unemployment involves some “work” in the form of job search and mandatory participation in training programmes, and uncertainty about future status. Hence it is unlikely that delays in SB applications is widespread among those who really are unable to work.

We have argued that delayed applications for SB is not a problem – But might some UI recipients who are not sick choose to apply for SB early in their spell, long before UI expire? In his case study, Westin 1990 describes that physicians were reluctant to comply with the demands for sickness certificates from patients who were not about to lose UI and seemed fit to work. Otherwise we have no empirical evidence of the interplay between doctors and SB applicants, but we bluntly assume that transitions to SB which cannot be justified using a purely medical criterion are not likely to be a major issue until UI are about to be exhausted. But when UI do expire, physicians do cooperate. Based on interviews with physicians and individuals who had been granted SB while unemployed, Westin 1990 concludes that when UI are about to expire physicians tend to “shift side” and become an ally of the patient by adding diagnoses or by making existing diagnoses appear more severe than they were when last employed.

3.1 Labour Market Training and Partial Employment

There are 14388 transitions to labour market programmes (here crudely abbreviated ‘training’) in the sample. Training may provide some of the beneficial environmental features mentioned in the models by Jahoda 1988 and Warr 1987 that are not available during ordinary unemployment, such as social contact, time structure and activity. On the other hand, participation may imply a negative impact on well-being if the activities seem pointless to the participants or through physical and mental strain. Furthermore, training reduces leisure time and thus reduces the value of being out of work, making non-participation more attractive. Finally, training can trigger reporting of latent health problems which prevent full participation in the training programme but did not prevent satisfactory job search activity. Given these possible causal effect of training on SB hazards, we model the *treatment effects* of ongoing and completed spells on the risk of SB transitions. The estimation of treatment effects is complicated by the fact that participants differ systematically from non-participants in terms of observed and unobserved characteristics. The selection problem is further reinforced by the use of training as a way of testing whether job seekers are really willing to work, and as a way of keeping potential labour force leavers in the labour force. To account for selection on observed and unobserved attributes we explicitly model the transitions into training and allow for the unobserved elements that govern this hazard to be correlated with those of the SB hazard. Although selection effects and treatment effects differ between individuals and between types of training we ignore heterogeneity in treatment effects.

There are 51974 transitions to periods where a person is registered as a "partially employed" (PE) job seeker at the unemployment office. The PE label spans anything between a day's work and working every day during a month (the unemployed reports for every day of the month whether he was in paid work). Since the person is searching for permanent employment and is still "in the system" we treat PE as part of the unemployment spell. However, since the PE spells convey valuable information on unobserved heterogeneity, and because PE months earlier in the unemployment spell may modify the impact of elapsed unemployment duration on health, we model PE spells in the same way as training spells.

4 Data

We use data obtained from Statistics Norway with detailed information on all individuals in Norway 1989 to 2002. The data include information from social security registers on all employment spells (excluding self-employment) in both the public and private sector with start and stop dates. Data obtained from unemployment offices and social security registers tell us if an individual received any kind of transfers during a given month, the transfer type, and other information such as type of medical diagnosis, where applicable. Data on unemployment spells are recorded by unemployment offices, with information on benefits rights, amount paid, remaining weeks with UI, etc. The information is recorded at the end of each month, so spells that are shorter than one month and are not active on the 'recording' day are not registered (the estimation procedure takes this into account). Since some information is not available for all years the sample consists of all unemployment spells that started between November 1993 and September 2001. We restrict unemployment spells to start after at least three months employment and with no registration as either unemployed, recipient of SB or rehabilitation benefits, and only if the individual was eligible for UI at the first contact with the unemployment office. We do not allow spells to start with a PE period, apart from a solitary first month since PE registration typically arises when the person becomes unemployed in the middle of a month. The spell start criterion precludes those who quit, seeing that they are not entitled to UI in the first eights weeks after the first contact with the unemployment office. We also exclude temporarily unemployed on recall (this status is recorded explicitly) and those who enter unemployment from compulsory (draft) military service (the latter often wait for education to begin without being at risk of exit during their unemployment).

We use the municipality of residence as the reference group for the unemployed workers. Norway has 434 municipalities (a couple of municipalities merged during the data period). The municipality is the administrative unit with respect to welfare, but it is the patient's own physician who makes the decision to grant SB. By using a dummy variable for each of the municipalities (less one) we can control for unobserved differences in administrative practices and remove the possibility of endogeneity in variables measured at the municipality-level. We remove all spells in municipalities with less than two transitions to SB, resulting in the loss of 65 municipalities and unemployment spells. Of course, the estimation strategy involves a trade-off between a representative sample and being able to control for unobserved effects, because the excluded municipalities are relatively small. The mean working age population in the excluded municipalities is 3143, compared to 7525 for the included ones. Note that we use dummy variables for local unemployment office districts in the hazard rate for labour market training, since the around 200 unemployment offices manage the training programmes. For education and disability we use dummies for county.

The sample is restricted to include those aged 20 to 59 at the beginning of the spell with censoring at age 60, and we end up with 89708 spells of which 6356 end with transition to SB, see Table 1. Right censoring occurs when the spell has not ended within 36 moths at risk of making a transition, has not ended within the data window, or when the individual turns 60, dies, or leaves the country. 21% of the spells are right censored.

5 Econometric Model and Identification Strategy

5.1 Econometric Model

We estimate a proportional hazards duration model with a special transition structure: When unemployed you are at risk of transition to training, PE, employment, SB, education and disability (pooled rehabilitation benefits and disability pension). Label these states $s = 1,..,6$. The unemployment spell ends with exit to employment, SB, education or disability. The spell continues during training and PE spells, but a participant is not at risk of a transition to training during training, and not at risk of transition to PE during PE. If training or PE is completed with return to ordinary unemployment you are again at risk of all transitions. Apart from the number of transitions and special attention to PE months, this structure is adopted from Røed and Raaum 2003 and Røed and Zhang 2005. The “treatment effects” of training on the hazard rates are estimated with dummy variables for ongoing training and months since latest training spell. We use dummies for ongoing and accumulated PE months to control for heterogeneity. Anticipation effects cannot be estimated; we assume that unemployed do not react on information about future training spells.

We only observe individuals at the end of each month, and therefore we need to make assumptions on the within-month behaviour of duration densities or hazard rates. We assume that hazard rates are constant within each calendar month. Let x_{ti} be the vector of characteristics that influence the transition to state s which we observe for spell i in the t’th calendar month, the time interval $(t-1,t]$. Note that due to months when spells are not at risk of ending with exit to state s (e.g. months when the individuals participate in training and therefore are not at risk of exit to training, or have their UI temporarily suspended and therefore is not eligible for SB) the duration variable becomes state specific, and we write d_{tsi} for elapsed duration at risk of transition to *s* at the beginning of month *t*. The integrated monthly hazard*s* are formulated as proportional hazards,

$$\lambda_s\left(d_{tsi}, r_{ti}, x_{ti}, \alpha_{ts}, v_{si}\right) = \exp\left(x_{ti}\beta_s + \gamma_s\left(d_{tsi}\right) + \delta_s\left(r_{ti}\right) + \alpha_{tsi} + v_{si}\right) \quad (1)$$

where v_{si} is spell- and risk-specific unobserved heterogeneity, and $\delta_s\left(r_{ti}\right)$ is a function of r_{ti} which is weeks remaining until benefits expire as of the beginning of month *t*. α_{tsi} is a function which measures the effect of ongoing and completed training and PE spells. $\gamma_s\left(d_{tsi}\right)$ is the log baseline hazard of exit to *s* for the interval $(t-1, t]$. The log baseline hazards for SB, work, PE and training are estimated with coefficients on dummy variables for each of the duration months 2 to 18, and for the grouped durations (19-21), (22-24), (25-30) and (31-36), whereas the specification for disability is alittle less flexible and the education hazard is not allowed to depend on elapsed duration (transitions to education are restricted to January and August). The probability of exit during month *t* conditional on being unemployed at the beginning of the month, is $h_{ti} = 1 - \exp\left(-\sum_{s\in K_{ti}} \lambda_s\left(d_{tsi}, r_{ti}, x_{ti}, \alpha_{tsi}, v_{si}\right)\right)$, where K_{ti} is the risk set for spell *i* at the beginning of interval *t*.

Due to the discrete nature of the data we do not observe spells which do not encompass any observation point (the end of a month). These short spells are probably a selected sub-sample of all unemployment spells, and the likelihood function must take this into account. We follow the approach of Gaure, Røed, and Zhang 2005.

Conditional on being observed, the likelihood contribution of a spell that ends with exit to *s* during month *t* is the joint probability that the spell ends with exit to *s* during *t* and that there was no other transition in that month *t* before transition to *s*. This can be shown to be equal to

$$\frac{\lambda_s\left(d_{tsi}, r_{ti}, x_{ti}, \alpha_{tsi}, v_{si}\right)}{\sum_{s\in K_{ti}} \lambda_s\left(d_{tsi}, r_{ti}, x_{ti}, \alpha_{tsi}, v_{si}\right)} h_{ti} \prod_{j\in B_i,\, j<t} \left(1 - h_{ji}\right) \quad (2)$$

where B_i is the set of calendar months when spell *i* was at risk of completion. The conditional likelihood of a right censored spell is $\prod_{j\in B_i}\left(1 - h_{ji}\right)$. Define the transition indicator y_{tsi} for spell i ending with transition to state s at the end of month t. The conditional likelihood function for spell i conditional on unobserved heterogeneity $v_i = (v_{1i},..,v_{6i})$ and on being observed is

$$L_i(v_i, z=1) = \prod_{t\in B_i}\prod_{s\in K_{ti}}\left[\{1-h_{ti}\}^{1-\sum_{s\in K_{ti}} y_{tsi}}\left\{h_{ti}\frac{\lambda_s(d_{tsi}, r_{ti}, x_{ti}, \alpha_{tsi}, v_{si})}{\sum_{s\in K_{ti}}\lambda_s(d_{tsi}, r_{ti}, x_{ti}, \alpha_{tsi}, v_{si})}\right\}^{y_{tsi}}\right] \tag{3}$$

where z is an indicator variable equal to one if a spell is observed, 0 otherwise.

The unconditional likelihood is obtained by integrating over the conditional distribution of unobserved heterogeneity, $f(v_i \mid z=1)$. Using Bayes' Theorem we see that

$$f(v_i \mid z=1) = \frac{P(z=1 \mid v_i)}{P(z=1)} f(v_i) \tag{4}$$

Since we do not know anything about behaviour before spells are observed we need to make assumptions about this in order to proceed. We assume that the flow of entry is constant during each month, and that the effect of unemployment duration on the hazard rate is the same in the unobserved entry month as in the first observed month. Similarly, the effect of calendar time is assumed to be the same in the month before the first observation in the data. Then we obtain for a spell which was first observed in period t

$$\begin{aligned} P(z=1 \mid v_i) &= \int_0^1 \exp\left(-(1-s)\sum_{s\in K_{ti}}\lambda_s(d_{tsi}, r_{ti}, x_{ti}, \alpha_{tsi}, v_{si})\right)ds \\ &= \frac{1-\exp\left(-\sum_{s\in K_{ti}}\lambda_s(d_{tsi}, r_{ti}, x_{ti}, \alpha_{tsi}, v_{si})\right)}{\sum_{s\in K_{ti}}\lambda_s(d_{tsi}, r_{ti}, x_{ti}, \alpha_{tsi}, v_{si})} \end{aligned} \tag{5}$$

and

$$P(z=1) = \int P(z=1 \mid v_i) f(v_i) dv_i \tag{6}$$

It now remains to integrate out the unobserved heterogeneity components v_{si}. We use a discrete distribution for unobserved heterogeneity. If there are M support points $v_m = (v_{1m},..,v_{6m})$ with weights p_m, $\sum_{m=1}^{M} p_m = 1$, $0 < p_m \le 1$, the likelihood contribution becomes $L_i = \sum_{m=1}^{M} p_m L_i(v_m \mid z=1) f(v_m \mid z=1)$. By substituting in expressions (3), (4), (5) and (6), and replacing the integral in (6) with its discrete counterpart, the complete likelihood function emerges.

Gaure, Røed, and Zhang 2005 conduct simulation studies of the properties of discrete-time proportional hazards duration models with nonparametric specification of both unobserved heterogeneity and duration dependence (NPMLE) and two dependent competing risks. They find that the ability of the models to identify the true parameters depends crucially on allowing the data to decide the correct or "optimal" number of points of support in the heterogeneity distribution. The optimal number M is found by sequentially estimating the model for increasing M, starting from M=1, until the likelihood or some related criterion no longer increases. In some cases improvement in the log likelihood itself is a too weak criterion for adding another mass point and this may result in upward biased duration dependence. Hence it can be appropriate to apply a stopping criterion which requires estimation to stop if the likelihood does not increase enough to "justify" adding the extra parameters. Gaure, Røed, and Zhang 2005 conclude that the AIC is an appropriate criterion for deciding the optimal M, whereas the Bayesian Information Criterion (BIC) and Hannan-Quinn Information Criterion (HQIC) often result in too few mass points.[1][2] In this paper the AIC is applied. We note the conclusion by Gaure, Røed, and Zhang 2005 that the position and size of the mass point of the heterogeneity distribution cannot be interpreted in terms of different types of spells and their relative frequency, because different combinations of mass point locations and probabilities create observationally equivalent unobserved heterogeneity distributions in terms of its mean and variance. These two moments were found to be estimated consistently in the simulations, though.

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[1] Baker and Melino 2000 faced a similar problem in their simulation studies of single risk models and concluded that the BIC or HQIC yielded better fits with the data than the unadjusted likelihood, but did not consider the AIC. These criteria penalize the likelihood for extra parameters, the BIC being somewhat more restrictive than the HQIC, and HQIC being more restrictive than the AIC: $\text{BIC} = L - c\ln\left(N\right)/2$, $\text{HQIC} = L - c\ln\left(\ln\left(N\right)\right)$ and $\text{AIC} = L - c$ where *L* is the log likelihood value, $c = (S+1)*M - 1$ is the number of "free" parameters in the distribution of unobserved heterogeneity, *S* is the number of transitions, and *N* is the number of monthly observations. Note that BIC and HQIC depend on the number of observations, whereas the AIC does not.

[2] Estimation was feasible using a supercomputer at the University of Oslo and by using a very efficient estimation procedure programmed by Simen Gaure. The maximization algorithm is described in Gaure, Røed and Zhang 2005.

5.2 Identifying Duration Dependence and Unobserved Heterogeneity

Zhang 2003 and Gaure, Røed, and Zhang 2005 show in simulation studies that for mixed proportional hazard models with time varying covariates (TVC) identification of both duration dependence and unobserved heterogeneity is obtained with large samples.[3] TVCs ensure identification because individuals who enter unemployment at different dates are exposed to different time effects (such as business cycle and seasonal patterns) during their spell, and this provides indirect information on the unobserved heterogeneity: If a spell "survives" through a time period when observed variables suggest a high probability of exit to a particular state, then we expect that this spell has unobserved elements which give a low probability of making this transition. Likewise, calendar time provides important variation in the form of lagged explanatory variables, e.g. the month at entry or the state of the business cycle at the date of entry to unemployment. These variables do not vary within spells, but capture some of the otherwise unobserved differences between spells. The idea is that those who become unemployed when few others do have relatively poor latent "labour market quality" and e.g. lower expected hazard rates for transition to work. Røed and Raaum 2003 and Røed and Zhang 2005 exploit this source of identification for modeling transitions out of unemployment, and we adopt this strategy and use 98 dummy variables for the current calendar month, and 11 season-month dummies for the month of entry to unemployment. For the entry month we also use an indicator of labour market tightness, which captures (smoothed) labour market conditions, see Gaure and Røed 2003 for details. Figure 1 shows the distribution of entry dates into unemployment for the sample. The graph shows both variation over the business cycle and seasonal variation, with peaks in January and July. In Figure 2 we display the rate of transition to SB from unemployment (number of exits divided by number at risk in the current month). This also exhibits substantial seasonality, being relatively low in December, January and July. Figure 3 shows the indicator of labour market tightness.

The data offer the opportunity to use repeated spells for identification of unobserved heterogeneity. However, this requires that the individual effects are constant across

[3] Gaure, Røed, and Zhang 2005Gaure, Røed, and Zhang 2005 also found that violations of the proportionality assumption generally causes severe bias in all parameter estimates. However, we continue to base the analysis on the PH assumption, in the tradition of the literature.

spells, ruling out state dependence and important events in between spells. Since we intend to identify the effect of unemployment duration on health it is not reasonable to assume that unobserved attributes are constant across spells. As pointed out by Røed and Raaum 2003, using repeated spells also implies a special selection problem, since the probability of being registered with a second spell depends negatively on how far into the data window the first spell began, and negatively on the length of the first spell. Therefore all spells are treated as independent.

It remains to explain how we separate the duration and incentive effects. The primary source of identification is a change in UI regulations 1997 which drives a wedge between elapsed duration and remaining UI to identify the duration and incentive effects. Before the reform (most) unemployed were entitled to 80 weeks UI, followed by 13 weeks without benefits and then another 80 week period was allowed if the persons had fulfilled his obligations in job search. Since 1997 the rule was a straight 156 weeks on benefits, 78 for those with previous earnings below a certain threshold. Figure 4 shows the empirical hazard rates for exit to SB for those with a maximum of 80 and 156 weeks (18.5 and 36 months), respectively. We note the marked increase in hazards when UI run out, suggesting that something is going on around the time of UI exhaustion that is not due to health and elapsed duration. Since the reform was applied uniformly to all unemployment entrants there is no contemporaneous control group. Therefore the usefulness of the reform as identifying variation relies entirely on the absence of unobserved differences between spells that started before and after the reform, and requires that the incentive effect – the proportional change in the hazard rates when UI are about to expire – does not depend on elapsed duration.

Another source of random gap between elapsed and remaining UI comes from variation in maximum UI duration around a certain threshold amount of previous earnings. The ceiling on the number of weeks you can receive UI differs according to previous earnings (since 1997). The previous earnings measure is defined by calendar time – it is the larger of earnings in the previous calendar year and the average earnings in the previous three calendar years. This introduces randomness in UI rights. Røed and Zhang 2005 describe this source of random variation in benefits in greater detail.

The third source of difference between elapsed duration and remaining comes from the PE months. When PE the unemployed only consumes part of the UI, and therefore the total UI period is extended. We account for unobserved characteristics of persons with PE months through 21 dummy variables for number for accumulated months registered as PE during the spell.

Figure 5 illustrates the variation in remaining UI at spell start. Apart from the sources described above, variation also comes from the fact that many sample entrants have experienced some unemployment previously and therefore have already "consumed" some of their UI rights before entering this unemployment spell.

6 Results

We focus on the hazard rate for transitions to SB, but the results for transitions to other states can be obtained upon request. Based on the AIC the estimation resulted in the optimal number of mass points in the heterogeneity distribution, M, being 10. Selected parameter estimates for the SB hazard rate are reported in Table 2, and the distribution of unobserved heterogeneity in Table 3. The entire set of parameter estimates can be obtained upon request.

6.1 Duration Dependence and Incentive Effects

The estimated baseline hazard function for SB transitions is shown in Figure 6. Apart from parameter uncertainty it appears that the hazard increases over the entire range and we conclude that the risk of being registered as sick increases throughout the unemployment spell. This is consistent with the hypothesis that unemployment gradually breaks down self-confidence and increases the risk of developing mental and physical illnesses. It does not support the "stabilization" hypothesis that people learn to handle their situation during the spell, although we may fail to pick heterogeneity in duration dependence. The increase in the hazard is much less than reported by Røed and Zhang 2005 – their hazard increases by a factor of around 2.5 to 3 over the first 12 months - although comparability is limited seeing that their exit state is pooled SB and various forms of disability benefits. In the present model the estimated baseline hazard for transition to disability reaches a maximum of 90% over the baseline at entry after 15-18 months duration, which is somewhat more than the increase in the SB hazard, but still far from the estimate in Røed and Zhang 2005.

6.2 Incentive Effect

The incentive effect of expiring UI is estimated using six dummy variables for the intervals 0-4, 5-8, 9-13, 14-26, 27-39 and 40-52 weeks before UI expires[4]. The incentive effect is highly non-linear, with significant effects confined to the last three months, and the hazard increases by 54% in the last month before UI expire. This pattern is probably driven by a combination of myopic behaviour of the unemployed, and by physicians lowering the threshold for SB when UI exhaustion approaches. The (imaginary) proportional impact on the baseline hazard rate for someone who risks losing UI after 18 months and again after 36 months is illustrated in Figure 7.

6.3 Social Interaction

We use the unemployment rate and the share of working age population outside the labour force in the municipality of residence, as proxies for social norms for working and for being available for work. These variables vary with calendar year, and different patterns of time variation in these rates identify the causal effects of social interaction. The effect of calendar time is absorbed by 98 dummies for current month, and unobserved municipality-specific effects are accounted for using dummy variables for 368 municipalities, which capture time-invariant factors that might create spurious correlation between unemployment rates and individual health outcomes. For example, positive correlation might arise from collective learning of how to cope with unemployment in high-unemployment communities (Warr, Jackson, and Banks 1988), and negative correlation could arise from past adverse labour market shocks which drive up the local unemployment rate and reduce average health. We find that the risk of exit to sickness benefits depends negatively on the municipality unemployment rate, an increase in the unemployment rate of one percentage point translates into an increase of 14% in the hazard rate. The result mirrors studies which have established that average psychological health is better among those unemployed who are surrounded by many unemployed, than among those who are unemployed in areas with little unemployment.

[4] The information on time with UI is given as the number of weeks used at the end of each month.

See e.g. Clark and Oswald 1994 and Clark 2003 for a study using panel data. The effect of a change in the share out of the labour force is positive but insignificantly different from zero. Again, the sign is consistent with social interaction effects as it is easier to violate the norm of labour force participation and leave the labour force if the norm is weak.

One possible limitation to the causal interpretation is that the attention that each unemployed person receives from his case worker at the unemployment office, may depend negatively on the local unemployment rate. If pressure from the case worker can induce stress and reduce well-being then this may explain some of the correlation between hazard rate to SB and local unemployment rate.

6.4 Effects of Labour Market Training

The effect of participating in labour market training programmes is estimated using dummy variables for the duration of ongoing training and for the number of months since the latest training spell. The SB hazard is elevated by some 35-80% during the first five months of training, and then the effect becomes smaller or at least insignificant (Figure 8). This might be a genuine health effect driven by increased stress from being forced into activity. However, it is plausible that part of the pattern is due to increased reporting of minor illness when training begins, illness which prevents full participation but is not detected without participation.

Unemployed persons who are about to lose UI are often assigned to training, implying that participants face zero UI when the program ends (you cannot lose UI during participation). Since we do not observe the latent length of any programme, the incentive effect is restricted to zero during training, and as a result the estimated effect of training is probably biased upward. In order to investigate the size of the bias we estimate the model allowing for an incentive effect for any month when UI are about to expire, regardless of whether the person is in training. This will remove the bias in the training effect but implies negative bias in the estimated incentive effect. The coefficient on the training indicator changes only by an order of 10-15%, and we conclude that this issue is of minor importance.

The post-participation effect is largely different from zero at all durations form 1 to 12 months after participation (Figure 9). For comparison, the effect on the job hazard is positive in the first months after training, and decreasing with time since participation.

6.5 Observed and Unobserved Heterogeneity

We have information on sickness/rehabilitation benefits in the period 12 to 4 months prior to spell start, and use three dummy variables for diagnoses for musculoskeletal, mental or other illness. The fact that you need to have worked recently to qualify for SB implies that we do not use information on distant past spells of registered sickness. The coefficients reflect the persistence of health problems over time, and persons diagnosed with psychological illness have a somewhat larger hazard rate than persons with diagnosis for musculoskeletal health problems.

The estimates show that the SB-hazard increases with work experience for men, whereas the coefficient is zero for women (experience is measured as the ratio of actual to potential work experience[5]). Other things equal, a 10% points higher ratio of actual to potential experience generates a 4.3% higher hazard rate. This may mirror a positive correlation between experience and latent commitment to work, in the sense that those who feel a stronger intrinsic desire to work suffer more from being unemployed, and therefore have a higher risk of becoming sick as a result of being unemployed. A complementary explanation is the positive “habituation” effect of past unemployment on the ability to cope with present unemployment as found by Clark, Georgellis, and Sanfey 2001. In Figure 11 we see that persons with less than maximum employment the 12 months before spell start (less the three months immediately before spell start, which the spell is conditional on) have a higher SB hazard, which could reflect a positive correlation between recent employment and health. The zero effect for women is probably due to experience being a poor proxy for employment commitment for women.

The proportional effect of age is presented in Figure 10 panels a and b for men and women. The bulk of this pattern is probably due to the causal effects of age on health. However, selection may be part of the explanation, because employment stability

[5] Work experience is based on accumulated pension points obtained from tax files. Since the system began 1967, this underestimates the experience of those sample members who began their work life before that. Therefore we use the ratio of actual to potential experience.

increases with age, and those who become unemployed at an age when most people are continuously employed are expected to have relatively poor unobserved attributes in relation to labour market performance, and perhaps also poor health.

Note that the UI compensation ratio is not included since it can only be measured with reasonable accuracy for those who can be assumed to have worked full time all year for at least one of the three full calendar years before becoming unemployed.

The estimated correlations of unobserved heterogeneity across exit states should be interpreted with caution because the standard errors of the estimates are unknown. The estimated distribution of the $1-\exp\left(-\exp\left(v\right)\right)$ unobserved heterogeneity is shown in Table 3. It is interesting to note that those whose unobserved attributes suggest a high probability of transition to SB also have a higher probability of finding work; this correlation has been positive and "large" in all estimated models. This might arise if "strong" individuals are more likely to embark on the application process for SB. Latent employment commitment is another potential source of the correlation.

7 After Transition to SB

There are reasons to expect differences in the duration of SB spells between those who entered SB as UI was about to expire, and those who entered well before UI exhaustion. A relatively large share of the former are supposedly "strategic entrants" who are less sick and therefore remain in SB for a shorter period than the latter who are "legitimate" SB claimants. However, seeing that the strategic entrants applied for SB in order to prolong their time on benefits at the same amount as UI, we would expect that they do what they can to remain on SB for the maximum 12 months. A mandatory health check after 8 weeks on SB might prevent this: The physician carries out a thorough examination in order to provide the social security authorities with detailed information on diagnosis, treatment and prospects for recovery. Westin 1990 portrays a long process from SB to permanent disability benefits, with multiple treatments and conversations with physician(s) who are reluctant to let people drift out of the labour force for good. On the other hand, given that physicians avoid sending patients into uninsured unemployment, this follow-up may not form an important obstacle to maintaining SB for the maximum 12 months when UI are not an option. This is explored further by estimating a binary outcome model where the dependent variable equals 1 when the SB spell lasts 12 months, 0 if it is completed before that. We need to recognize that those who are unemployed long enough to face the UI exhaustion are not a random subset of all unemployed – this is due to differences in hazard rates out of unemployment which may be correlated with the duration of SB, and due to causal effects of unemployment on health. We therefore include a number of observed variables including the diagnosis type and elapsed unemployment duration at the month of transition to SB. A dummy variable for "strategic entrant" indicates whether the individual was granted SB within the last three months before UI expired. This includes both strategic entrants and other entrants to SB. The coefficient on this dummy variable is significantly positive at the 1% level. Even though the dummy for "strategic entrant" is measured with error because it includes some none-strategic entrants and leaves out some strategic entrants, the result does indicate that strategic entrants are on average "successful" in staying on SB and receiving UI-level transfers for the maximum allowed 12 months.

8 Conclusion

The proportion of the Norwegian working age population who were permanently disabled increased from 8.3% in 1995 to 10.4% 2004, and the number of SB-days per employed person increased from 8.1 in 1994 to 14 in 2004. Many explanations of this worrying development have been proposed, from tougher labour market conditions (a higher incidence of firm downsizing, more privatization of public enterprises and more competitive working environments) to lower thresholds for applying for welfare benefits. This paper contributes with new knowledge about one particular path out of the labour force, the one that goes from unemployment to sickness benefits. The results show that the risk of transition to sickness benefits increases as unemployment proceeds, and jumps up in the months before UI expire. The former can be interpreted as a health effect, whereas the latter is a result of economic incentives.

The detrimental effects of unemployment on health have been acknowledged in medical and social research at least since the 1930s. However, there has been little formal analysis of the causal effect of elapsed time unemployed on health. This paper uses duration analysis with a flexible specification of both duration dependence and unobserved heterogeneity, and we find that the hazard rate of transition to sickness benefits increases throughout the unemployment spell. Although being granted sickness benefits is a rather crude measure of severe health impairment, the result does tell us that the risk of developing serious health problems increases as a result of continued unemployment. The result does not support the hypothesis that people adapt to being unemployed.

The incentive effect arises because it is possible to prolong the total time on UI-level benefits by transition to sickness benefits. We take advantage of a reform which changed the maximum length of UI to identify the causal effect of UI exhaustion on hazard rates. The estimated increase in the hazard rate for transition to sickness benefits is 54% in the last month before UI expires. We also follow those who go to sickness benefits and find that, compared to those who entered SB from unemployment with more one month of UI remaining, those who make the transition when UI expired are more likely to stay on sickness benefits for the maximum of 12 months. This shows that some "strategic entrants" are "successful" in prolonging their stay on an income transfer at the level of UI. Of course, this behaviour requires the aid of a physician. We do not go further into the interplay between patient and physician, but the results do suggest that the system is more flexible than intended for unemployed persons who are about to lose their UI.

The estimates also reveal that men with more labour market experience, who supposedly have a stronger commitment to work, face a higher probability of transition to sickness benefits. Furthermore, the risk of transition to sickness benefits decreases with the municipality unemployment rate, after controlling for municipality specific factors and common time effects. We take this as an indication that the social norm for supporting oneself is stronger when more people work, and when the norm is stronger it is more stressful to be unemployed.

Participation in labour market training programmes might be seen as a way of preventing exit from the labour force. However, the results suggest that labor market training does not reduce the negative impact of unemployment on mental and physical health. In fact, the SB hazard rate is elevated substantially during training whereas the post-participation effect is not significantly different from zero. Part of the participation effect may be due to increased reporting of actual sickness during training programmes.

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Appendix

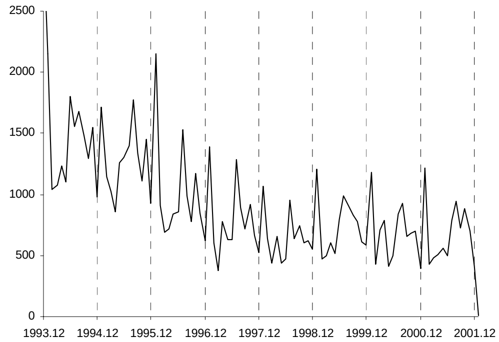

Figure 1. Entrants to the sample

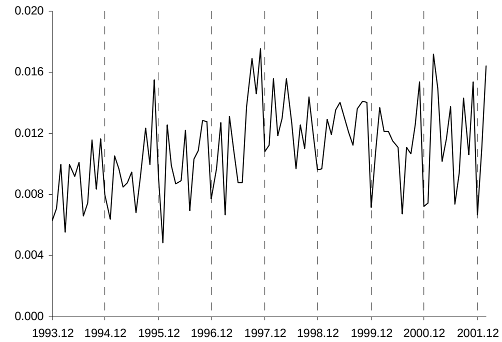

Figure 2. Transitions from unemployment ot SB. Note that since the first spells start 1993.12, all durations are not represented before 36 months later (1996.12) onwards.

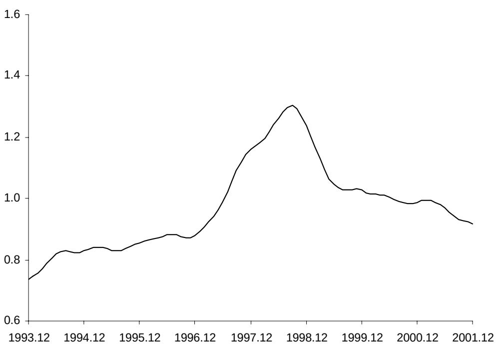

Figure 3. Labour market tightness indicator. Source: Gaure and Røed 2003.

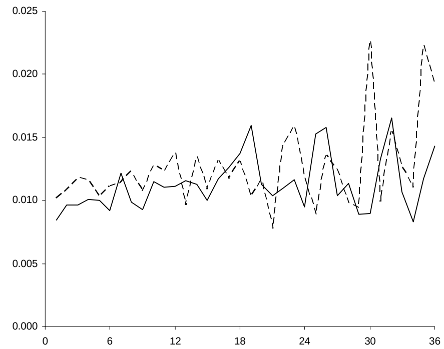

Figure 4. Empirical SB-hazard. Full line for entrants with 80 weeks (18.5 months) UI, broken line for entrants with 156 weeks (36 months) UI.

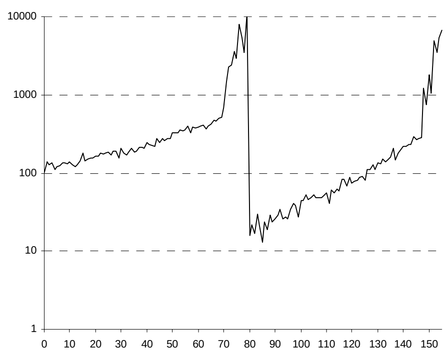

Figure 5. Remaining UI at spell start (weeks) . Vertical axis is number of entrants (Logarithmic scale).

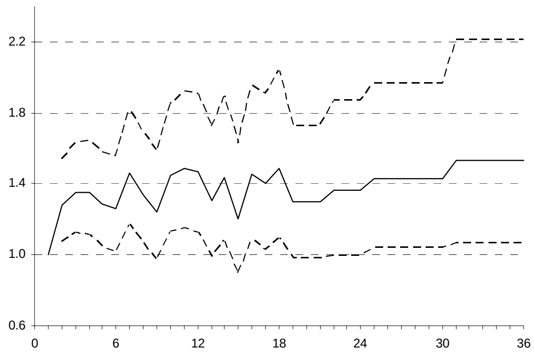

Figure 6. Baseline hazard, normalised to 1 at duration month 1, with 95% pointwise confidence intervals.

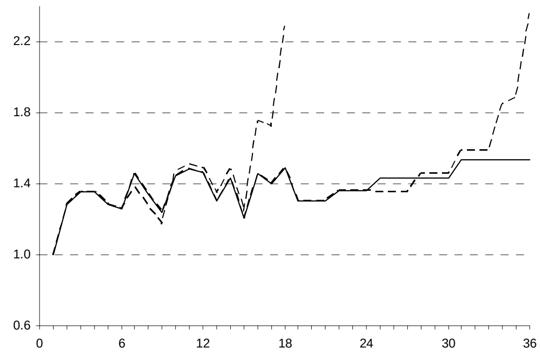

Figure 7. Baseline hazard, with incentive effect for UI expiring after 18 and 36 months.

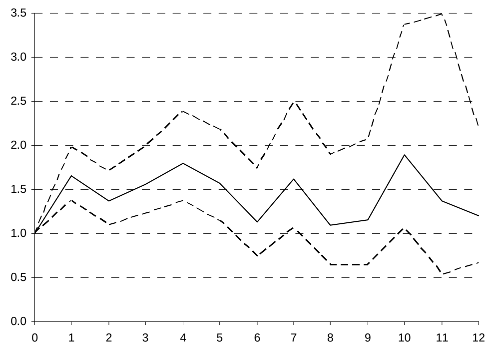

Figure 8. The proportional effect of ongoing training, relative to no training, with 95% pointwise confidence intervals. Duration of training in months on horizontal axis.

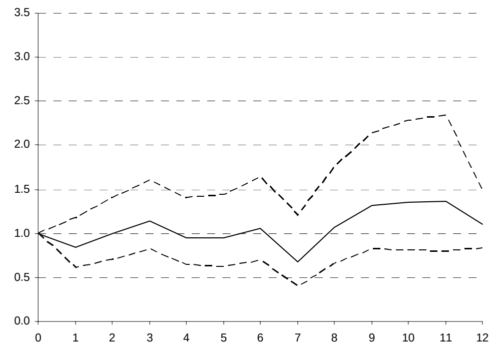

Figure 9. The proportional post-participation effect of training, normalized to 1 before and more than 12 months after participation, with 95% pointwise confidence intervals. Time since training in months on horizontal axis

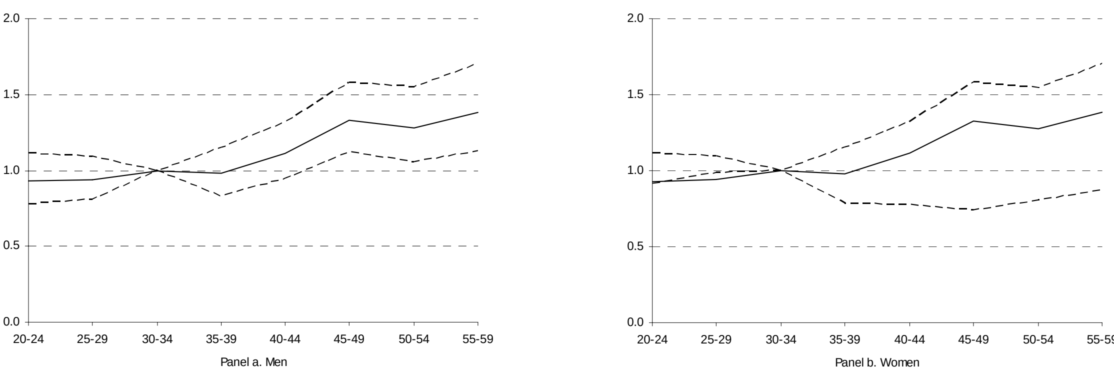

Figure 10. The proportional effect of age on hazard rates, with 95% pointwise confidence intervals. Normalised to 1 for age group 30-34.

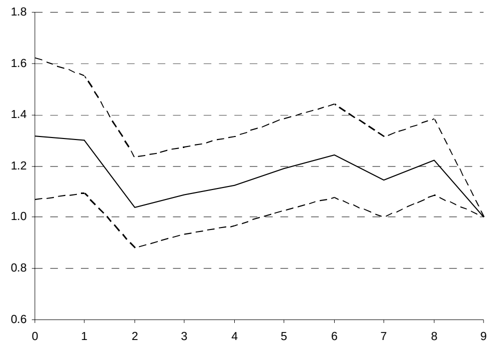

Figure 11. The proportional effect of months employed during the period 12-4 months before spell start, with 95% pointwise confidence intervals.

Table 1. Descriptive statistics

Number of observations		702778	
Number of spells		89708	
		Elapsed duration	
Transitions	Number	Mean	*Std. dev.*
Sickness benefits	6359	7.970	*7.277*
Labour market training programmes	14388	6.981	*6.491*
Partial employment	51974	4.575	*5.252*
Employment	61890	5.156	*5.679*
Education	2255	6.028	*6.233*
Disability	649	9.823	*8.886*
Right censored	18555	9.810	*9.385*

Distribution of variables at spell start	Mean	*Std. dev.*
Months employed in the period 12-4 months before spell start (maximum is 9)	7.622	*2.425*
Share who received sickness benefits at least one month in the period 12-4 months before spell start		
Diagnosis for psychological illness	0.014	
Diagnosis for musculoskeletal illness	0.047	
Other diagnoses	0.041	
Woman	0.449	
Age	34.788	*10.395*
Years of education	11.039	*2.301*
Years of work experience*	10.422	*8.273*
Share not OECD country of origin	0.065	
Number of Children	0.686	*0.975*
Marital status (shares)		
Single, never married	0.530	
Married	0.359	
Separated, divorced or widowed	0.111	
Municipality		
Unemployment rate, age 16-66	0.031	*0.012*
Share out of the labour force, age 20-59	0.120	*0.030*

* Work experience is based on pension points accumulated since 1967.

Table 2. Results of duration analysis

Log likelihood	-427133.3013	
Log likelihood adjusted with AIC	-427202.3013	
Number of parameters	2410	
Selected coefficient estimates for the SB hazard	Estimate	*s.e.*
Dummies for time remaining till UI are exhausted		
0-4 weeks	0.430	*0.091*
5-8 weeks	0.205	*0.111*
9-13 weeks	0.187	*0.096*
14-26 weeks	0.035	*0.064*
27-39 weeks	0.018	*0.057*
40-52 weeks	-0.054	*0.053*
more than 52 weeks	ref.	
Municipality		
Unemployment rate, age 16-66	-14.635	*3.980*
Share out of the labour force, age 20-59	3.402	*2.385*
Partially employed	-0.125	*0.050*
Dummies for received sickness benefits with this diagnosis at least one month in the period 12-4 months before spell start		
Diagnosis for psychological illness	0.791	*0.078*
Diagnosis for musculoskeletal illness	0.585	*0.050*
Other diagnoses	0.791	*0.078*
No sickness benefits	ref.	
Work experience, men*	0.432	*0.107*
Work experience, women*	-0.011	*0.079*
Education level dummies		
8 years or less	0.129	*0.051*
9-10 years	0.099	*0.033*
11-12 years	ref.	
13 years or more	-0.258	*0.051*
Woman	0.794	*0.123*
Labour market tightness, at spell start	0.255	*0.206*

Other explanatory variables included those presented in graphics in addition to the following: 22 dummies for accumulated months with PE code; 11 dummies for month of spell start; 98 dummies for current month; 368 dummies for municipality of residence; 4 Gender-specific dummies for marital status; 4 Gender-specific dummies for number of children; 18 dummies for county of residence; 14 Gender-specific age dummies for 5-year age intervals; Dummy for non-OECD country of origin.

* Work experience is based on pension points accumulated since 1967. Measured as percentage of maximum potential experience.

Table 3. Estimated distribution of unobserved heterogeneity

Number of mass points 10

Distribution of the 1-exp(-exp(.)) masspoint distribution

	Expectation	*Variance*
Sickness benefits	0.0285	*0.0056*
Labour market training	0.0104	*0.0002*
Partial employment	0.1377	*0.0097*
Employment	0.2119	*0.0810*
Education	0.0088	*0.0005*
Disability	0.0004	*0.0000*

Correlation coefficients

	Labour market training	Sickness benefits	Partial employment	Employment	Education
Sickness benefits	-0.23				
Partial employment	0.03	-0.18			
Employment	-0.22	1.00	-0.17		
Education	0.29	-0.14	0.03	-0.15	
Disability	-0.15	-0.45	0.10	-0.45	0.33

Note: The statistical properties of the distribution of unobserved heterogeneity are unknown.

Table 4. Logit model for the probability of remaining on sickness benefits for 12 months

Number of observations	6295	
Number of observations with dependent variable equal to 1	797	
Selected coefficient estimates	Estimate	*s.e.*
Dummies for time remaining till UI are exhausted		
0-4 weeks	0.573	*0.210*
5-8 weeks	0.239	*0.295*
9-12 weeks	-0.107	*0.260*
more than 12 weeks	ref.	
Diagnosis		
Psychological illness	0.110	*0.091*
Musculoskeletal illness	0.752	*0.112*
Other illness	ref.	
Elapsed unemployment duration in months	0.064	*0.016*
Elapsed unemployment duration in months, squared	-0.001	*0.001*
Accumulated months as partially employed	-0.038	*0.013*
Partially employed	-0.337	*0.116*
Ongoing labour market training	-0.824	*0.166*
Months since completed latest training spell	-0.047	*0.024*
Months employed during 12 months before unemployment spell	0.015	*0.018*
Dummies for received sickness benefits at least one month in the 12 months before spell start		
Diagnosis for psychological illness	0.268	*0.199*
Diagnosis for musculoskeletal illness	0.201	*0.138*
Other diagnoses	0.391	*0.143*
No sickness benefits	ref.	
Work experience, men*	-0.465	*0.277*
Work experience, women*	-0.275	*0.232*
Age	0.038	*0.034*
Age, squared	0.000	*0.000*
Education level dummies		
8 years or less	0.180	*0.133*
9-10 years	0.194	*0.101*
11-12 years	ref.	
13 years or more	-0.188	*0.163*
Woman	-0.392	*0.280*
Municipality		
Unemployment rate, age 16-66	-14.677	*4.601*
Share out of the labour force, age 20-59	1.081	*1.869*

All variables measured at entry to sickness benefits. Other explanatory variables included: Gender-specific dummies for marital status; 18 dummies for county of residence; Dummy for non-OECD country of origin.

* Work experience is based on pension points accumulated since 1967. Measured as percentage of maximum potential experience.