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Does Unemployment Compensation Affect Unemployment Duration?

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Abstract

We use a flexible hazard rate model with unrestricted spell duration and calendar time effects to analyse a dataset including all Norwegian unemployment spells during the 1990's. The dataset provides a unique access to conditionally independent variation in unemployment compensation. We find that a marginal increase in compensation reduces the escape rate from unemployment significantly, irrespective of business cycle conditions and spell duration. The escape rate rises sharply in the months just prior to benefit exhaustion. While men are more responsive than women with respect to marginal changes in compensation, women are most responsive with respect to benefit exhaustion.

Keywords: Unemployment spells, business cycles, unemployment compensation, nonparametric duration analysis.

JEL Classification: C41, J64.

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1 Introduction

The question of how economic incentives embedded in the unemployment compensation system affect transitions out of unemployment has received considerable attention in the literature. Surveys are provided by Danziger et al (1981), Devine and Kiefer (1991), Atkinson and Micklewright (1991) and Layard et al (1991), and more recently by Holmlund (1998) and Pedersen and Westergård-Nielsen (1998). Lancaster (1979, p. 956) concluded more than 20 years ago that an elasticity of unemployment duration with respect to the benefit level in the order of 0.6 'could now be regarded as established beyond reasonable doubt'. Later American and UK studies have more or less confirmed this conclusion, with point estimates ranging from 0.2 to 0.9 (Moffitt, 1985; Narendranathan et al, 1985; Katz and Meyer, 1990; Meyer, 1990; Card and Levine, 1998). In continental Europe, the evidence is more mixed, and the typical result is that significant incentive effects associated with the compensation level cannot be robustly identified at all (Hujer and Schneider, 1989; Groot, 1990; van den Berg, 1990a; Steiner, 1990; Hernæs and Strøm, 1996). Some studies indicate substantial responses, though, such as Abbring et al. (1998) and Carling et al (1999), the latter with a reported compensation elasticity as high as 1.6. The European evidence is more unanimous in its evaluation of effects associated with benefit exhaustion; the exit rate does seem to increase just prior to when benefits run out (Wurzel, 1990; Lindeboom and Theeuwes, 1993; Hunt, 1995; Carling et al, 1996; Thoursie, 1998; Winter-Ebmer, 1998; Bratberg and Vaage, 2000).

The overriding problem in the whole literature is a lack of *independent* variation in benefit payments or replacement ratios. Variation in benefit entitlements is typically correlated to previous income, which again is likely to be correlated with unobserved characteristics that affect unemployment duration in their own right. One approach that has been used by some researchers to solve this problem is to take advantage of structural reforms (e.g. changes in the compensation level) that affect some, but not all unemployed persons, and apply the difference-in-difference methodology (Meyer, 1989; Hunt, 1995; Winter-Ebmer, 1998; Carling et al, 1999). This approach is not without pitfalls however, since it rests on the non-verifiable assumption that labour market opportunities do not develop differently for the 'treatment' and the 'control' groups.

In the present paper, we take advantage of some rather subtle particularities in the Norwegian unemployment benefit system in order to obtain what we consider to be truly independent variation in unemployment compensation, conditional on previous income, spell duration and calendar time. We estimate the relationship between unemployment compensation and unemployment duration, and investigate how it varies over business cycles and spell durations. Our main findings are that a marginal reduction in unemployment benefits typically yields an increase in the escape rate from unemployment, that this effect is much stronger for men than for women, and that it operates under all business cycle conditions and at all spell durations. Moreover, shortening of the period for which benefits can be maintained is likely to have a large effect on the escape rate, even when there are generous renewal practices and exemption rules. The next section gives a brief account of the relevant economic theory. Section 3 describes the data, with a particular emphasis on the unemployment compensation variables. Section 4 outlines the econometric model and section 5 presents the results. Section 6 summarises the conclusions.

2 Theory

Dynamic search theory (Mortensen, 1977; 1990; van den Berg, 1990b) typically pictures the unemployed worker as determining a level of search intensity and/or a degree of job selectivity (i.e. a reservation wage) in order to maximise the present value of expected utility. Unemployment benefits affect unemployment duration because they affect the utility gain associated with a transition from unemployment to employment. An important result arising from this literature is that for a given wage offer distribution, a higher level of unemployment benefits reduces the transition rate for a newly unemployed benefit claimant, because the value of continued search rises. Moreover, the transition rate from unemployment to employment increases as the job seeker approaches the date at which unemployment benefits expire, because the value of continued search falls. The latter conclusion may be modified, or even reversed, if long term unemployment cause discouragement or if spell duration is used by employers as a screening device (Blanchard and Diamond, 1994).

The exact way in which a given level of unemployment income affects the exit rate depends on the wage offer distribution faced by the job seeker. In the simplest of reservation wage models, the job seeker determines the lowest acceptable wage such that the direct utility gain associated with obtaining a job paying the reservation wage exactly matches the value of continued search. The latter is equal to the discounted value of the expected wage (conditional on being above the reservation wage) multiplied by the arrival rate of acceptable wage offers. The transition rate then depends on the ratio between the utility gain associated with receiving the reservation wage rather than being unemployed and the utility gain associated with receiving the expected wage rather than the reservation wage. In a model of optimal search intensity, search effort is determined such that the marginal cost of search exactly matches the marginal increase in the job offer probability multiplied by the discounted utility gain associated with obtaining a job. The transition rate depends in this case on the ratio between the optimal marginal search cost and the utility gain associated with getting a job. If the job seeker is risk neutral and the value of leisure (net of search costs) is proportional to the wage rate (or the expected wage rate), both the reservation wage- and the optimal search models predict that the exit rate is homogenous of degree zero in the wage and the benefit level, i.e. that it is only the *replacement ratio* (the benefit level relative to the predetermined or expected wage) that affects the transition from unemployment to employment (see e.g. Mortensen, 1990, pp 68-69).

Dynamic search theory can of course not be taken to imply the existence of a unique reduced form transition rate response parameter with respect to the compensation level or the replacement ratio of the type alluded to by Lancaster (1979). The transition rate is a product of two constraints; a supply constraint (i.e. the reservation wage) and a demand constraint (the job offer arrival rate). The relative importance of these constraints - and hence the response parameters associated with economic incentives - is likely to depend on spell duration, business cycle conditions and on individual characteristics (see e.g. Arulampalam and Stewart, 1995). In addition to that, the exhaustion of unemployment benefits implies that the incentive effects associated with a given level of benefits changes continuously over the spell duration. Moreover, incentive mechanisms may be checked by work tests, whereby benefits are withdrawn if (suitable) job offers are rejected. Hence, variation in elasticity estimates across countries and across different time periods is exactly what should be expected on the basis of economic theory, even if the estimates represent true causal effects.

3 Data and the Sources of Variation in Unemployment Benefits

The Norwegian unemployment insurance system is compulsory. The only condition for eligibility is a previous yearly earned income above a fairly low threshold (the threshold was raised from around 4,000 to 6,500 Euro in 1997). There is thus virtually no self-selection into the group of eligible workers. The unemployment benefit is calculated as 62.4 per cent of the labour earnings in the previous calendar year (or the average of the last three years), up to a ceiling of roughly 33,000 Euro (implying that the benefit level never exceeds 0.624×33,000 Euro). Benefits can be maintained for up to 156 weeks. Until 1997, there was a formal limitation of 80 weeks, followed by a 13-week cut-off period, after which a new 80-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. Elderly workers have been entitled to indefinite benefits throughout the whole 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 comprises all workers below 60 years of age who became unemployed in Norway during the 1990's, who had a full time job prior to the unemployment spell and who were eligible for unemployment benefits to start with. Each spell is traced until it ends with a transition or until the end of the observation period. Spells are censored when benefits are terminated and when the job seeker participates in labour market programs. Together, this leaves us with approximately 100,000 individuals, 103,000 spells and 937,000 monthly unemployment observations. The registers contain information about a number of individual characteristics, such as age, gender, marital status, children, county of residence, immigration status, educational attainment and previous income. Most of the variables are time varying. Some descriptive statistics are provided in Table 1.

Table 1
Descriptive Statistics

	Men	Women
# individuals	58,625	41,874
# spells	60,226	42,879
# monthly observations	499,648	437,015
Averages and fractions over monthly observations:		
Age (years)	38.04	37.50
Previous work experience (years)	14.28	11.15
Unemployment benefits (Euro)	13,818	11,549
Expected income (Euro)	26,812	22,031
Replacement ratio	0.53	0.51
Fraction with only compulsory education	0.19	0.20
Fraction with lower secondary education	0.23	0.30
Fraction with upper secondary education	0.38	0.35
Fraction with lower university degree	0.13	0.12
Fraction with higher university degree	0.02	0.02
Fraction immigrants (Non OECD countries)	0.06	0.03
Fraction married	0.39	0.53
Fraction with children (below 18 years)	0.42	0.51

We now turn to the two important incentive variables; unemployment benefits and expected in-work income. Valid inference about the *causal* effect of unemployment benefits requires variation in benefits conditional on expected income that is certain to be independent of unobserved individual characteristics. This kind of variation can rarely be obtained, as replacement ratios typically vary among individuals precisely because their past incomes have varied, and such variation is unlikely to be independent of unobserved characteristics. In the present analysis, however, we take advantage of a two particular features of the Norwegian benefit system that in fact do entail an element 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 gives a higher benefit the more it is concentrated within the last calendar year. Consider for example two persons who had both worked exactly one year before they became unemployed, and earned exactly the same income. One of them became unemployed in January, the other in July. The former of these persons earned all his income in the previous calendar year, while the latter only earned half of his income in this period. As a result, the former receives twice as much unemployment benefits as the latter. 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.

Assume (for the moment) that the expected yearly income in a new job is equal to (wage-growth adjusted) income earned in the last calendar year, adjusted for the number of months each person worked in that year (for example, if last years income was generated by only six months work, the expected income is assumed to be twice the previous yearly income). Then, for a considerable group of unemployed workers, the variation in replacement ratios is *completely driven* by our two sources of independent variation (conditional on entry month, spell duration, calendar time and work experience). These are workers who had worked for at least one year before they became unemployed, who received benefits equal to 62.4 per cent of their income in the calendar year prior to the start of the unemployment spell (or slightly higher if they became unemployed after April), and who's annual income did not exceed the ceiling in the benefit system. In the econometric analysis, we estimate all incentive effects separately for this group of persons. We also estimate the same incentive effects based on more 'standard' type of variation in replacement ratios (and apply control variables to 'remove' spurious correlation). Two sources of variation may be of particular interest. The first type of variation arises simply because of imperfect wage data: For a number of workers we are not able to identify precisely their previous work experience, but we observe that their previous income is below what can be expected from a full time job in the future. This can happen because they have had periods of part time- or occasional work, or simply because the registers are incomplete. For this group, we impose an expected income equal to the lowest agreed income in the public sector wage scales (for the relevant age group). This is of course somewhat arbitrary, but all the persons are at least subject to the same arbitrariness. The second type of variation arises because the replacement ratio declines monotonously with expected income when previous incomes are above the ceiling in the unemployment benefit system. This variation is slightly modified, however, as we have adjusted expected wages based on extremely high previous incomes downwards to a maximum of approximately 60,000 Euro (in 1999).

Table 2 displays some properties of the distribution of the replacement ratios for various groups of unemployed, according to the source of the within-group variation. Group A are the unemployed persons for which we claim that the within-group variation is *entirely* generated by *independent* forces. This is a relatively homogenous group, with expected yearly incomes between approximately 25,000 and 33,000 Euro (in 1999). Group B consists of workers with low previous wages, whose expected wages are adjusted upwards in order to be considered "reasonable". Group C consists of persons with expected incomes above the ceiling in the benefit system, whose replacement ratios therefore are negatively correlated to previous income.

Table 2
Distribution of Replacement Ratios According to the Source of Variation

	Men			Women		
	Group A Independent	Group B Low wage	Group C High wage	Group A Independent	Group B Low wage	Group C High wage
# observations	209,091	112,511	178,046	155,096	238,045	43,874
Mean	0.6088	0.4517	0.4858	0.6059	0.4509	0.5076
Std. Dev.	0.0280	0.1022	0.0923	0.0331	0.0990	0.0941
Maximum	0.6374	0.6374	0.6373	0.6374	0.6371	0.6362
Third Quintile	0.6292	0.5471	0.5623	0.6285	0.5400	0.5772
Median	0.6125	0.4528	0.5019	0.6111	0.4539	0.5352
First Quintile	0.5985	0.3439	0.4202	0.5971	0.3495	0.4624
Minimum	0.2104	0.2747	0.1168	0.2269	0.2721	0.1097
Range	0.4270	0.3627	0.5204	0.4105	0.3650	0.5265

It is clear from the description of indexing rules above that the independent replacement ratio variation within group A displays an element of seasonality, both with respect to the time of entry and with respect to ongoing spells, and is also correlated with spell duration. The scope for identification of disincentive effects (without restrictions on either calendar time-, seasonal entry- or spell duration effects) lies in the fact that the relationship between the replacement ratio on the one hand and entry time, calendar time- and spell duration on the other is not the same for all.

4 The Econometric Model

Since we observe unemployment status by the end of each month only, we develop the model in terms of discrete monthly hazard rates. Let i be the subscript over individuals, let t be calendar time and let d be process time (spell duration). Let r_{it} be the log of the replacement ratio and let G_i be a vector of dummy variables indicating to which of the three groups A,B,C each person belongs, according to the source of within-group variation in replacement ratios (see section 3). Let c_t be an indicator of the business cycle condition (which is explained below) and let S_{t-d} be a set of 12 seasonal dummy variables indicating the calendar month of *entry* into unemployment. Let E_{it} be a set of dummy variables indicating in which of five educational groups a person belongs. Let x_{it} be a vector of other (mostly dummy-coded) control variables (age, work experience, family situation, region, citizenship etc.), and let v_i be a variable that captures unobserved individual heterogeneity. The monthly exit probabilities to be estimated are then specified separately for men and women as

$$
\begin{aligned}
h(t, d, x_{it}, v_i) &= 1 - \exp\left(-\exp(z_{itd} + v_i)\right), \\
z_{itd} &= \sum_{g=A,B,C} \left[g_{g1} + g_{g2} c_t + g_{g3} \log(d)\right] r_{it} G_i' + \sum_{g=B,C} \left[d_{g1} + d_{g2} c_t + d_{g3} \log(d)\right] G_i' \qquad (1) \\
&\quad + \left[b_1 + b_2 c_t\right] E_{it}' + y\, x_{it}' + l_d + s_t + hS_{t-d} + t\, c_{t-d} + j\, c_t \log(d),
\end{aligned}
$$

where $\exp(z_{itd} + v_i)$ is interpreted as the integral taken over an underlying continues time hazard rate for the time interval corresponding to spell duration month number d (Prentice and Gloeckler, 1978, Meyer, 1990), hence the parameters can be interpreted in terms of the underlying hazard rate. The model allows large flexibility in the effects associated with unemployment compensation $(g_{gh}, g = A, B, C, h = 1,2,3)$ in the form of interaction terms with the business cycle and spell duration. The reason for representing economic incentives in the form of a single replacement ratio rather than in the form of the benefit level and the expected wage is that it makes it possible to purify the group A independent variation in unemployment benefits. An important point is that *even to the extent that we measure the expected wage incorrectly*, this can only produce ‘innocent’ noise in the replacement ratio, since all variation in the replacement ratio is generated by sources that are independent of individual characteristics and individual behaviour. Hence we effectively rule out any chance of spurious correlation between the incentive variable and the transition rate. The formulation in (1) does not necessarily imply that the hazard rate is homogenous of degree zero in benefits and expected in-work income (see section 2). If the true model deviates from homogeneity, we may nevertheless write it in terms of a (log) replacement ratio and a (log) expected income, in which case the coefficients attached to the (log) replacement ratio (and the appropriate interaction terms) equals the elasticities of the hazard rate with respect to unemployment income, while the coefficients attached to (log) expected income equals the sum of the elasticities with respect to unemployment income and the expected wage. In our case, we have a source of variation in the replacement ratio that is accurately measured and certain to be independent of individual characteristics, while our measure of the expected income is inaccurately measured and highly dependent of unobserved characteristics. Hence, one interpretation of the formulation in (1) is that the true expected wage is instrumented by (or more accurately, replaced by) some of its determinants, such as educational attainment, work experience and business cycle conditions. A corollary is that we cannot identify the extent to which these latter variables affect the hazard rate directly and to which extent they affect it via the expected wage.

The spell duration effects (l_d) and the calendar time effects (s_t) for group A are estimated without parametric restrictions at all, with the aid of dummy variables (49 spell duration dummies and 104 calendar month dummies)[1]. The transition rate pattern arising from unobserved heterogeneity is disentangled from true duration dependence with the aid of lagged variation in exit rates. The basic idea is that the conditional expectation of unobserved heterogeneity (i.e. conditioned on observed characteristics and spell duration) depends on exit rates experienced earlier in the spell, while true duration dependence does not (van den Berg and van Ours, 1994; 1996).

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[1] Members of group B and C get their corresponding calendar time- and spell duration effects modified by the terms in the second summation bracket in line 2 of equation (1). The reason why we let duration- and calendar time effects vary across groups in this way is to make sure that any differences that might exist between the three groups are not 'thrown into' the interaction terms with the replacement ratio (the first summation bracket).

The higher the exit rates have been in the past, the more selection has taken place at any given spell duration, and the lower is the expected value of the unobserved covariate v_i. In the present context, the existence of multiple cohorts ensures that persons with exactly the same spell durations have been subject to different macroeconomic conditions earlier in the spell, and hence have been exposed to different selection forces. This variation in lagged explanatory variables is substantial and makes it possible to identify the underlying structural duration dependence without any arbitrary parametric assumptions at all. Identification is obtained even without the assumption of a Mixed Proportional Hazard model (Brinck, 2000). An additional source of identification arises from the existence of around 2,600 repeated spells, by assuming that the unobserved covariate v_i is fixed for each individual rather than for each spell.

In order to avoid unnecessary parametric restrictions, we assume that the unobserved variables v_i is 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 spells experienced by individual i during the whole estimation period. Let $y_{ib}=1$ if spell number b of individual i ends in a transition (non-censored), and zero otherwise, let d_{ib} be the duration of that spell. Let W be the number of mass points in the distribution of unobservables and let p_w be the probability that the unobserved covariate obtains the value v_w . The likelihood function in terms of observations of (d_{ib}, y_{ib}, t, x_{it}) is then given as

$$L=\prod_{i=1}^{N}\sum_{w=1}^{W}p_w\prod_{b=1}^{B_i}\left(\left(h(t,d_{ib},x_{it},v_w)\right)^{y_{ib}}\prod_{s=1}^{d_{ib}-y_{ib}}\left(1-h(t-s,d_{ib}-s,x_{it-s},w_w)\right)\right),\quad \sum p_w=1,$$
$$t=1991.2,\ldots,1999.9,\quad d=1,2,\ldots,48. \tag{2}$$

The likelihood (2) is maximised with respect the model parameters in (1) and with respect to the parameters entering the discrete distribution of unobserved heterogeneity (W,p_w,v_w)[2].

One of the variables used in the model cannot be observed directly, namely the state of the business cycle c_t. A commonly used proxy for this variable is the national or the local rate of unemployment. However, the unemployment rate is not only determined by the current state of the business cycle, but also by previous states of the business cycle, as well as by the current composition of the unemployment pool. An alternative would be to use the aggregate outflow rate from the unemployment pool, but even this variable is to a large extent governed by composition effects. Hence, rather than using any of these observed aggregates, we estimate the business cycle indicator in a separate econometric model. The model used for this purpose is a Hazard rate model which are proportional in calendar time for both men and women. The resulting calendar time dummy estimates are collected into a time varying scalar variable, and a smoothing filter (X11ARIMA) is then applied in order to remove seasonal effects and other high frequency movements. The resulting variable is denoted c_t, and it is used as our business cycle indicator. This indicator has previously been shown to lead other labour market tightness statistics and to give a reliable picture of the business cycle developments in Norway during the 1990's (Røed 2001).

[2] The computational task of maximising this likelihood is formidable, particularly because extensive search is required in order to ensure that the maximum found is really global. We have used a program written for us in fortran by Simen Gaure at the Computing Resource Centre at the University of Oslo. The program is based on the concept of implicit dummy variables, effectively reducing any set of dummy variables (e.g. our 104 calendar time dummies) to a single variable.

5 Results

The likelihood functions for the male- and female models obtained their maxima with five- and four mass-points in the distribution of unobserved heterogeneity, respectively. These maxima were robustly identified on the basis of a large number of estimations with different starting values. Moreover, small changes in the number of mass-points hardly affected the results of interest at all (including the spell duration baseline hazard rate), hence the selection of Information Criterion (see Baker and Melino, 2000) is of minor importance in the present case. Our interpretation of this finding is that the information content in the data with respect to the distribution of unobserved heterogeneity is sufficient to ensure a robust and purely data based identification of structural duration dependence. The main estimation results are presented in Table 3 and in Figures 1-3 (complete results are available on request). It may be noted from the Likelihood Ratio test statistics reported at the bottom of the table, that the Mixed Proportional Hazard rate model (which is a special case of our more general model) is rejected by the data.

Table 3
Selected Maximum Likelihood Estimation Results

	Men		Women	
	Estimate	SE	Estimate	SE
I. Group-specific disincentive effects				
Group A				
Log replacement ratio	-0.9463	0.1642	-0.3492	0.1910
Log replacement ratio × Business cycle	0.6806	0.7217	-1.0799	0.8199
Log replacement ratio × Log spell duration	-0.0293	0.1183	-0.0022	0.1327
Group B				
Log replacement ratio	-0.4047	0.0549	-0.2510	0.0472
Log replacement ratio × Business cycle	0.6076	0.2432	0.7109	0.2145
Log replacement ratio × Log spell duration	-0.0585	0.0450	0.0234	0.0359
Group C				
Log replacement ratio	-0.3561	0.0523	-0.2263	0.0993
Log replacement ratio × Business cycle	0.3739	0.2661	0.8033	0.4913
Log replacement ratio × Log spell duration	-0.1393	0.0409	-0.0529	0.0776
II. Interaction between duration dependence and the business cycle	-0.1854	0.0353	-0.1697	0.0399

Table 3
Selected Maximum Likelihood Estimation Results

	Men		Women	
	Estimate	SE	Estimate	SE
III. Individual characteristics				
Number of years education				
≤9 (level 1)	-0.1470	0.0176	-0.3103	0.0231
≤9× Business cycle	0.1604	0.0939	0.0788	0.1153
10 (level 2)	-0.2189	0.0163	-0.2218	0.0189
10× Business cycle	0.2063	0.0871	0.2806	0.0964
11-12 (level 3)	ref.	ref.	ref.	ref.
13-16 (level 4)	0.1082	0.0176	0.4499	0.0214
(13-16)× Business cycle	-0.4091	0.0906	0.1082	0.1023
≥17 (level 5)	0.2026	0.0385	0.6501	0.0485
≥17× Business cycle	-0.3622	0.1827	-0.6806	0.2260
Married	0.2845	0.0152	-0.0237	0.0156
Children	-0.1380	0.0138	-0.3477	0.0171
Immigrant (outside OECD)	-0.6963	0.0475	-0.4910	0.0657
IV Business cycle conditions at time of entry	-1.5302	0.0523	-0.4184	0.1876
V. Model properties				
Total number of coefficients	219		217	
Number of mass-points unobserved heterogeneity	5		4	
Log likelihood	-132134.1039		-99415.8079	
VI Restriction tests				
LR test: No unobserved heterogeneity	$X^2(8)=69.1$ [0.0000]		$X^2(6)=112.4$ [0.0000]	
LR test: Proportional model	$X^2(15)=148.5$ [0.0000]		$X^2(15)=64.7$ [0.0000]	
LR test: Zero compensation effect group A	$X^2(3)=45.2$ [0.0000]		$X^2(3)=8.6$ [0.0351]	

Note: In addition to the variables reported, the following variables were included among the explanatory variables: Calendar time dummies, inflow month, region, work experience, average income during work career, group dummies (B,C) and group-specific interaction terms with log(spell duration) and log(business cycles). All variables (except the dummies) are measured as deviation from mean. The range for the business cycle variable is around 0.62.

Table 3, part I, reports benefit elasticity estimates for the mean covariate vectors. Since the interaction terms with spell duration and business cycles make it difficult to interpret the results directly, Table 4 offers the means and the ranges of the predicted elasticities taken over all monthly observations. According to the estimates associated with group A – i.e. the estimates based on the *purely independent variation in replacement ratios* - there is an average compensation elasticity around –0.95 for men and –0.35 for women[3]. This indicates slightly stronger male responses than suggested by previous findings for the United Kingdom (Narendranathan et al, 1985; Narendranathan and Stewart, 1993; Arulampalam and Stewart, 1995), much stronger responses than typically found in most of continental Europe (Hujer and Schneider, 1989; Groot, 1990; van den Berg, 1990; Steiner, 1990) and Norway (Hernæs and Strøm, 1996), but weaker responses than found in Sweden (Carling et al, 1999). It also indicates that female responses are significantly weaker than male responses. In the present analysis, it seems that spurious correlation between replacement ratios and unobserved characteristics does not tend to bias the benefit elasticity estimates away from zero. On the contrary, for men the group B and C coefficients (based on variation suspected to depend on unobservables) indicate weaker responses than for group A, while for women, there is a conspicuous concurrence in the three group-specific estimates.

Table 4
Predicted Replacement Ratio Elasticities

	Men			Women		
	mean	lowest	highest	mean	lowest	highest
Group A	-0.9545	-1.1022	-0.6215	-0.3423	-0.7416	-0.0698
Group B	-0.3816	-0.5836	-0.0544	-0.2485	-0.4787	0.0486
Group C	-0.3572	-0.6073	0.0521	-0.2238	-0.4556	0.1680
Group A, B and C	-0.6126	-1.1022	0.0521	-0.2793	-0.7416	0.1680

The disincentive effects seem to be either non-cyclical or even *counter-cyclical.* The latter implies that the benefit elasticity is larger (in absolute terms), the worse are the business cycle conditions. This is particularly evident for group B. Our results at this point are contrary to findings for the United Kingdom (Arulampalam

[3] Note that although non of the group A elasticity estimates for women are significantly different from zero, they are jointly significant (se the test statistic reported in the last line of Table 3). It may also be noted that when the two interaction terms are dropped, the elasticity estimate for women in group A is –0.38, with a standard error of 0.15.

and Stewart, 1995) and the United States (Moffitt, 1985). While these earlier findings suggested that economic incentives were virtually irrelevant during economic slumps (in which the demand constraint on labour dominated the supply constraint), our results indicate that economic incentives are important throughout the cycle. Our findings at this point are more naturally associated with a model of optimal search effort than with a model of reservation wage determination[4]. The potential role of reservation wages during a boom may be restrained by strict enforcement of job acceptance requirements, implying that benefit receivers are compelled into the vacant jobs.

The benefit compensation effect remains relatively stable over the unemployment spell for both men and women. Hence our results do not confirm existing evidence at this point (Nickell, 1979; Fallick, 1991; Narendranathan, 1993; Narendranathan and Stewart, 1993; Arulampalam and Stewart, 1995; Bover et al, 1998; Jenkins and García-Serano, 2000), which indicates a rapidly declining effect of unemployment income as a spell lengthens. On the contrary, for men belonging to group C, our estimates indicate a weak *increase* in incentive effects[5].

[4] There is some evidence, based on the estimation of structural search models, indicating that reservations wages are empirically unimportant, and that virtually all job offers are accepted (van den Berg, 1990a; Devine and Kiefer, 1991).

[5] As pointed out by Westergård-Nielsen (1998, p. 87), the few observations available at high durations in the previous studies suggest that these results are rather tentative. In the present analysis, we have more than 125,000 monthly observations at durations above two years.

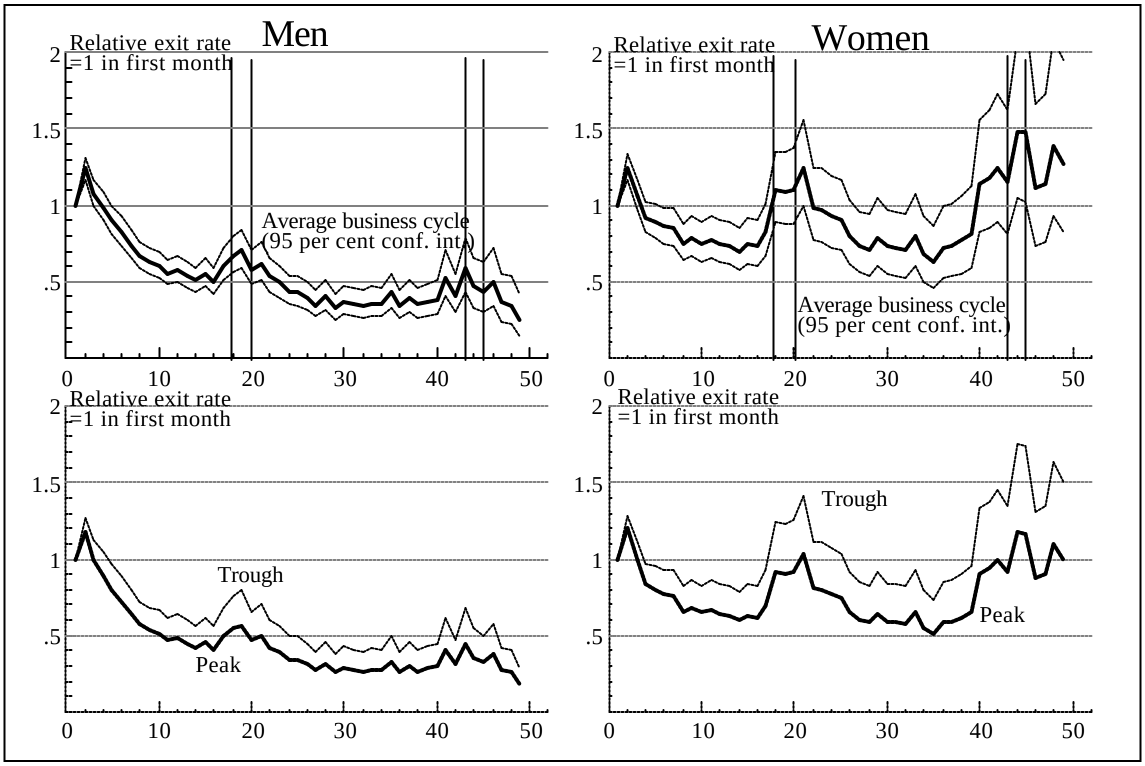

Figure 1. Estimated duration baseline hazard rates (group A).

Note: Vertical lines indicate temporary (after 18-20 months) and permanent (43-45 months) benefit exhaustion.

The effects of spell duration and benefit exhaustion are illustrated in Figure 1. The two upper panels display the estimated spell duration baseline hazard at average business cycle conditions (with 95 per cent confidence intervals). The hazard rate rises with around 20 per cent from the first to the second month, probably reflecting that the search- and hiring process is time consuming. After the second month of job search, however, the hazard rate declines more or less monotonically during the next 15-16 months. The degree of negative duration dependence in this phase of the spell is much stronger for men than for women. There are substantial rises in the hazard during the months just prior temporary- and permanent benefit exhaustion. The point estimates indicate that the hazard rises with around 40 per cent for men and almost 60 per cent for women in the months just prior to the first (and temporary) benefit exhaustion. This is of course likely to reflect pure economic incentive effects. But an additional explanation is that persons with imminently expiring benefits have been given very high priority in the allocation of labour market services. Prior to temporary benefit exhaustion (after 18-20 months of unemployment) the job seekers have typically been summoned to consultations at the Public Employment Service in order to discuss employment opportunities, job search efforts, alternative possibilities of income support etc. Similar procedures have been adopted at the end of the second benefit period, i.e. after around 43-45 months of unemployment, although the exact timing vary somewhat according to the capacity and the priorities of the local Public Employment Service. The fact that even a very soft and 'negotiable' constraint on benefit duration entails large behavioural responses confirms previous results reported by Dolton and O'Neill (1996) and Gorter and Kalb (1996) indicating that just by giving attention to unemployed workers, employment offices stimulate them to find a job more quickly.

Table 3, part II, reports estimates suggesting that long term unemployed persons are less sensitive towards business cycles than the short term unemployed. As illustrated in the two lower panels of Figure 1, this implies that the degree of negative duration dependence is weaker during recessions than during recoveries. Our results at this point are clearly at odds with the ranking hypothesis[6] proposed by Blanchard and Diamond (1994), as well as with empirical evidence reported by Dynarski and Sheffrin (1990) and Butler and McDonald (1986). However, they are in line with findings reported by Imbens and Lynch (1993) and Rosholm (1996). Our results may be interpreted as support for the discouraged worker- or the scarring type hypotheses -

[6] The ranking argument goes as follows: When firms receive job applications, they hire the applicant with the shortest unemployment duration. During a recession, there are typically many applicants per vacancy; hence the ranking effect (and the associated negative duration dependence) becomes stronger.

i.e. that it is more demoralising and more stigmatising to be long-term unemployed in good than in bad times.

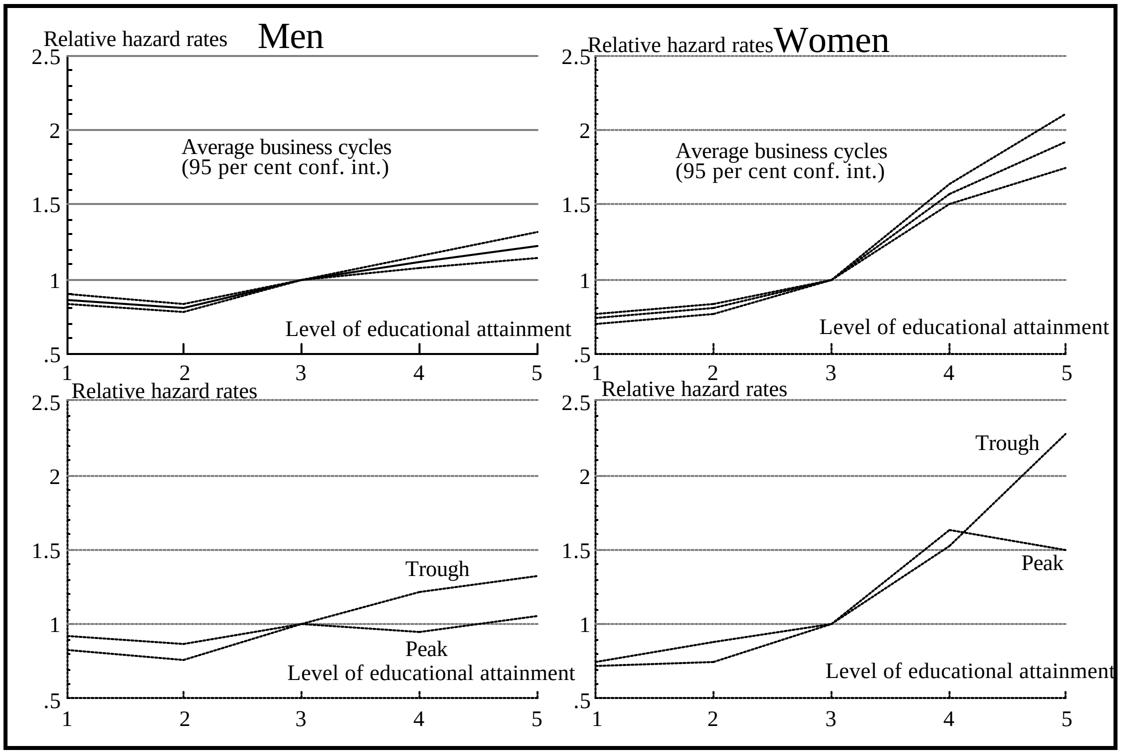

Figure 2. Estimated relative hazard rates (with 95 per cent confidence intervals) as functions of educational attainment
Note: The levels of educational attainment are as indicated in Table 3.

Part III of Table 3 presents some other results of interest. The exit rate rises monotonically with education, and it does so much more for women than for men. However, this pattern virtually vanishes during peak times, particularly for men. Low-skilled individuals are much more sensitive towards business cycle conditions than high-skilled individuals. These points are also illustrated in Figure 2. The estimates indicate for example that a man with only compulsory education has an 8 per cent lower hazard rate than the reference man during a peak, and a 17 per cent lower hazard during a trough. Similar results have previously been reported for the Netherlands by Teulings (1993). The explanation is probably that when employment prospects are sufficiently meagre, educated persons are willing to accept jobs for which they are over-qualified. At the same time, employers take advantage of the excess labour supply to increase qualification standards for new hires. As a consequence, the competition for low-skill jobs becomes harder, and in this competition, the persons with lowest education yield. Marriage entails a higher predicted exit rate for men and a lower predicted exit rate for women. Responsibility for children reduces the hazard rate, particularly for women. Immigrants from non-OECD countries have exit rates 40-60 per cent lower than natives. The age effect is estimated with a number of dummy variables, and the results are presented in Figure 3. The exit rate declines monotonically with age for both men and women. For men, it declines particularly strongly from the age of 20 to the age of 40, while for women it declines most strongly after the age of 40.

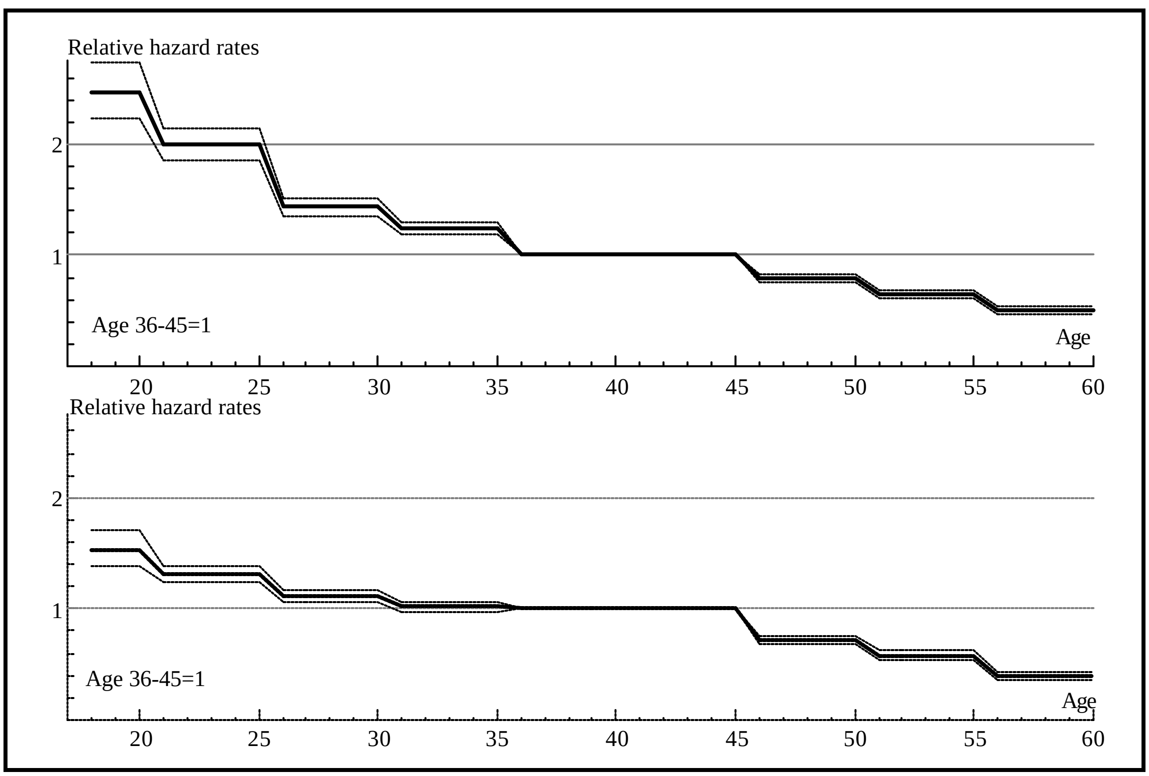

Figure 3. Estimated relative hazard rates (with 95 per cent confidence intervals) as functions of age

As indicated by Table 3, part IV, there seems to be substantial variation in the selection of persons into unemployment over business cycles (there are also large seasonal variations, but we have not reported those results in the table). This is particularly the case for men. A man becoming unemployed during a business cycle boom has an individual probability of escaping unemployment 60 per cent below that of a man becoming unemployed during a slump (given the same *current* business cycle conditions). For a woman, the difference is around 30 per cent.

6 Concluding Remarks

Our results suggest that *marginal changes* in the unemployment benefits affect the behaviour of men much more than the behaviour of women, while the threat of more *drastic cuts* (benefit termination) affects women much more than men. The average elasticity of the hazard rate with respect to unemployment benefits is around –0.95 for men and –0.35 for women, implying that a 10 per cent reduction in benefits may cut a 10 month duration by approximately one month for men and 1-2 weeks for women. Disincentive effects associated with the unemployment benefit system are at work throughout the business cycle and at all spell durations.

The threat of benefit termination has a substantial positive effect on the escape rate from unemployment in the months just prior to exhaustion. The effect is much stronger for women than for men. Even a temporary benefit termination with generous exemption rules seems to entail this type of response (a 40 per cent increase for men and a 60 per cent increase for women). Our estimates suggest that by imposing a temporary and renewable benefit constraint after six months of unemployment, a spell expected to last for 10 months under the current rules would be cut by around six weeks for women and by three weeks for men.

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