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Temporary Layoffs and the Duration of Unemployment

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12 July, 2001

Temporary Layoffs and the Duration of Unemployment

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Abstract

We use Norwegian micro-data to identify the driving forces behind unemployment spells following temporary- and permanent dismissals. The duration of unemployment spells for *permanently* dismissed workers is primarily explained by individual resources and economic incentives, while spell-duration for *temporary* dismissed workers is explained by firm incentives. Higher benefits reduce the employment hazard for permanently- but increase it for temporary dismissed workers. Structural duration dependence is non-parametrically identified and sharply estimated with the aid of lagged hazard rate variation and repeated spells. The employment hazards for both spell-types exhibit positive duration dependence during the first months and negative duration dependence thereafter.

Keywords: unemployment duration, unobserved heterogeneity, competing risks

JEL classification: C41, J64

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1 Introduction

It has long been realised that the duration profile of unemployment spells depends on the extent to which recall is expected to occur (Katz, 1986; Jensen and Westergård-Nielsen, 1990; Corack, 1996; Jensen and Nielsen, 1999; Rosholm and Svarer, 2001). While individual economic incentives embedded in the unemployment benefit system play a crucial role in the search for new jobs, firm incentives play a key role in the timing of recalls. In most countries, the distinction between these two types of unemployment spells is not crystal clear however (Haltiwanger, 1984), and the recent empirical job search literature tends to treat all unemployed persons as subject to the risks of both a recall and a transition to a new job. This literature finds that the two transitions are indeed governed by very different causal mechanisms (Katz, 1986; Jensen and Westergård-Nielsen, 1990; Jensen and Nielsen, 1999), but that they nevertheless are interrelated because the intensity of search for new jobs depends negatively on the recall probability (Katz, 1986; Rosholm and Svarer, 2001). In reality, a number of unemployed persons face a zero recall probability (e.g. because the firm they worked in has closed down), while others face a recall probability close to unity (e.g. when the temporary dismissal is caused by industrial conflict or by regular seasonal cycles). Hence, the competing risks analytical framework may be misleading for a large fraction of unemployed workers.

In the present paper, we build on micro event history data from Norway in which temporary- and permanent dismissals are distinguished at the start of the unemployment spells and in which both individual- and firm incentive variables are recorded. We use these data to identify the driving forces behind each spell-type, in particular the role of economic incentives and the spell duration pattern. The data comprises unemployment spells caused by dismissals in Norway during the period from 1989 to 1998, and consists of 6.6 million monthly observations. We focus in particular on the effects associated with a series of reforms in the regulations regarding firms' pay liability during periods of temporary dismissals. These reforms constitute 'natural experiments' that are useful in order to identify the role played by firm incentives with respect to the duration of recall unemployment spells.

There is a large theoretical- as well as empirical literature considering individual search behaviour. Surveys are provided by Danziger et al (1981), Devine and Kiefer (1991), Atkinson and Micklewright (1991), and more recently by Holmlund (1998) and Pedersen and Westergård-Nielsen (1998). Two themes dominate this literature. The first is the extent to which the escape rate from unemployment rises or falls over spell duration, and in particular how a limited duration of unemployment benefits affects this pattern. With regard to the general pattern of duration dependence, there are large discrepancies in the literature. These discrepancies result from fundamental identification problems regarding the roles of unobserved heterogeneity and true duration dependence respectively, and by the failure to distinguish properly between temporary- and permanent dismissals. A more or less unanimous finding, however, is that the hazard rate rises as the moment of benefit exhaustion approaches (Meyer, 1990; Lindeboom and Theeuwes, 1993; Hunt, 1995; Carling et al, 1996; Bratberg and Vaage, 2000). The second major theme in the job search literature is the extent to which the escape rate from unemployment is affected by changes in the level of unemployment benefits. The evidence on this point is mixed. While US and UK studies typically provide benefit elasticity estimates (the elasticity of unemployment duration with respect to the level of the unemployment benefit) ranging from 0.2 to 0.9 (Moffitt, 1985; Narendranathan et al, 1985; Katz and Meyer, 1990; Meyer, 1990; Card and Levine, 1998), European elasticity estimates differ widely, from virtually zero (Hujer and Schneider, 1989; Groot, 1990; van den Berg, 1990; Steiner, 1990; Hernæs and Strøm, 1996) to above unity (Abbring et al., 1998; Carling et al (1999).

The literature is more silent regarding the driving forces behind the duration pattern of recall unemployment spells, although there is strong evidence suggesting that firms' economic incentives (the degree of experience rating) is of quantitative importance for the use of temporary dismissals as such (Topel, 1983; Card and Levine, 1994; Anderson and Meyer, 1994). According to the implicit contract literature (Feldstein, 1976; Baily, 1977), firms have to offer employment contracts that over time provide workers with a market-determined level of expected utility. Hence, in a general equilibrium context, variation in wages must compensate for variation in expected unemployment exposure. Some supporting evidence for the compensating wage differential hypothesis is provided by Topel (1984) and, more recently, by Moretti (2000). It has important implications for the duration pattern of recall unemployment spells. First, it implies that even fully Social Security financed temporary dismissals are costly from the firms' point of view, to the extent that workers are worse off during temporary dismissals. Second, workers with high unemployment benefits are more likely to be selected for a temporary dismissal than workers with low unemployment benefits (Baily, 1977), and the dismissal period is unlikely to stretch beyond the time of benefit exhaustion. At the same time, temporary dismissals generated in an implicit contract framework are most likely to occur in firms with relatively frequent use of temporary dismissals characterised by a high probabilities of recall. Hence, one may very well *observe* that temporary dismissed workers with the highest unemployment benefits (ceteris paribus) are those that have the highest recall hazard rate. To the extent that ordinary spells and recall spells cannot be distinguished properly, this may confound the underlying causal relationship between unemployment benefits on the one hand and search effort and the reservation wage on the other.

The aim of the present paper is to take advantage of our ability to distinguish the two spell-types and our extraordinarily rich micro dataset in order to provide new insights into the causal mechanisms that determine the outcome of ordinary- and recall unemployment spells. The paper contains at least two contributions to the literature: First, we provide new evidence indicating that ordinary- and recall unemployment spells are governed by completely different causal mechanisms, and we show in particular that the length of the period by which firms can pass on their temporary dismissal costs to the public purse *strongly* effects the expected duration of recall spells. Second, we identify non-parametrically the pattern of structural duration dependence in the employment hazard for permanently- and temporary dismissed workers with a degree of robustness and sharpness that, according to our knowledge, goes well beyond any previously published results. The next section gives a brief description of the Social Security system in Norway, and the reforms used to identify the importance of firm incentives. Section 3 presents the data. Section 4 describes the econometric model and section 5 presents the results. Section 6 concludes.

2 Temporary Layoffs and the Social Security System in Norway

Norway is one of the few industrialised countries in which there is a clear distinction between ordinary- and recall unemployment in the sense that recall unemployed are explicitly exempted from the general job search requirements applying for other benefit claimants[1]. In return for this partly firm-specific reserve pool of labour, the firms are obliged to bear some of the costs. The regulations governing the employers' contribution to their own temporary dismissed workers have changed a number of times during the 1990's.

[1] The same applies under certain conditions to temporary dismissed workers in United States (OECD, 2000). In other countries (Belgium, France and Germany) there exist more limited work-sharing programs in which hours-reductions within a firm can be (partly) financed by unemployment benefits (Abraham and Houseman, 1994). The practice of not being available for placement into other jobs occurs in other countries as well and probably at a much larger scale than suggested by formal regulations. In some countries (Austria, Germany and Denmark) there is evidence of collusion between employers and employees in the form of layoffs with implicit recall agreements (Fischer and Pichelmann, 1991; Mavromaras and Rudolph, 1998; Jensen and Nielsen, 1999).

Until the autumn of 1990, employers paid nothing; the temporary dismissed workers received compensation from the Social Security system according to the general unemployment benefit rules, without being available for alternative work. From October 1990 and onwards, the costs have been shared between the employers and the Social Security system depending on the duration of the dismissal period. The employers pay the ordinary salary during the first three days of full-time dismissal. Then, there is a limited period in which the Social Security system pays unemployment benefits (the Social Security period). After that, the firm again has to pay the full wage, or fire the worker permanently (paying heed to advance notice regulations). As summarised by Table 1, following a rather bold tightening of the regulations in 1990 the system gradually slid back towards its pre-reform features through successive extensions of the Social Security period.

Table 1
Temporary Dismissals and Social Security contributions in Norway During the 1990's

Regime label	Period	Firms' pay liability during temporary full time dismissals (ordinary wage)	Social security period (62.4 per cent of ordinary wage up to a ceiling)
R1	Before Oct 1990	No pay liability.	80 weeks.
R2	Oct 1990-May 1991	First three days of each spell plus any dismissals exceeding the Social Security period.	12 weeks out of the last 52 weeks.
R3	Jun 1991-May 1992		20 weeks out of the last 52 weeks.
R4	Jun 1992-Dec 1993		26 weeks out of the last 52 weeks
R5	From January 1994		52 weeks out of the last 78 weeks.

While the Social Security period for temporary dismissed workers was tightened in October 1990, the benefit period for ordinary unemployed workers was extended from 80 weeks to 2x80 weeks, with a 26-week quarantine between the two 80-week periods. In June 1991, the quarantine was reduced to 13 weeks, and in June 1992 the quarantine was virtually abolished through the introduction of liberal exemption rules. In January 1997 a single 156-week rule was introduced.

3 The Data

The data we use describe the status of registered unemployment spells by the end of each calendar month during the period from 1989 to 1998 for persons with a previous job in a typical recall-sector of the economy, i.e. manufacturing, construction, transport or tourism[2]. These sectors are responsible for around 60 per cent of recall-unemployment and 40 per cent of ordinary unemployment in Norway. The spells are divided into recall spells and ordinary spells. Each spell is tracked until a transition occurs or until it is censored by the end of the observation period. We assume in this paper that transitions out of the unemployment register are associated with obtaining a job[3]. Unfortunately, we do not observe whether the workers return to their original employer or find a new job. We know, however, that recalls are rare for permanently dismissed workers. In a limited period (1995-1996) for which we have access to (poorly dated) employment records, we found that around 6 per cent of the terminated ordinary unemployment spells in our dataset could be identified as a recall. On the other hand, around 10 per cent of the recall spells were terminated by a new job. Hence, the distinction between the two spell types is not perfect, but probably sharper than in other countries.

The dataset consists of roughly 6.6 million monthly unemployment observations divided into 800,000 spells distributed among 500,000 individuals. The unemployment data is merged with other administrative registers providing information about individual characteristics such as gender, age, educational attainment, work-experience and individual economic incentives. Economic theory identifies two important incentive factors, the level of unemployment benefits and the distribution of feasible wages. Our data makes it possible to calculate the level of benefits accurately, while individuals' prospective wage

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[2] A large fraction of the remaining recall unemployment spells is within seafood industries, which are subject to separate rules and hence not affected by the reforms considered in this paper.

[3] Transitions into- and out of the two unemployment states are defined in the following way: i) A transition into either of the two unemployment states occurs in month *t* if the person was recorded as unemployed by the end of that month and had not been recorded as unemployed during the previous three months; ii) A transition from unemployment to a job is recorded in month *t* if the person was unemployed by the end of month *t-1*, not unemployed by the end of month *t*, and did not return to register at all within the next three months; iii) A transition from recall unemployment to ordinary unemployment is recorded in month *t* if the person was temporary dismissed by the end of month *t-1*, not temporary dismissed by the end of month *t*, and was ordinary unemployed by the end of month *t* or in any of the next three months.

distribution is of course unobserved. We use two proxies for the individuals' wage prospects. The first is the maximum of the last three years observed incomes. This variable picks up human capital factors as well as firm-specific factors, and may also include transitory components. The second is a predicted income derived from the results of a separate selection-type wage model, based on complete income records for full time workers in the relevant sectors in 1995. This is a pure human capital based measure based on observed qualifications only (education, work-experience), and as a consequence, it leaves behind a large element of residual (unexplained) variation.

Table 2 provides some descriptive statistics. Recall unemployment accounts on average for 32.2 per cent of the spells, but only 13.3 per cent of total unemployment exposure. According to the results from a multivariate logit model (not shown), the probability that a given unemployment spell started up as a recall- rather than an ordinary spell, declined from around 40 to 30 per cent as the result of the 1990 Social Security reform (R2). The later Social Security period extensions (R3-R5) did no reverse this development, hence it seems reasonable to interpret the decline in the relative number of recall spells as a response to the three days pay liability at the start of each spell.

Table 2.
Descriptive statistics.

	Ordinary unemployed	Recall unemployed
# monthly observations	5,715,526	880,305
# spells	548,720	266,653
# individuals	336,608	162,377
Transitions (per cent of spells)		
Out of unemployment	87.73	81.39
Ordinary unemployment	-	17.31
Censored	12.27	1.29
Observation averages		
Benefit claimants (per cent)	72.4	90.4
Claimants' replacement ratio, (benefits relative to predicted income, based on human capital model)	0.40	0.47
Women (per cent)	27.0	13.7
Married (per cent)	36.6	46.3
Age (years)	37.3	38.7
Work experience (years)	10.6	13.1
Educational attainment (years)	12.2	12.2

4 Econometric Approach

A common approach to the analysis of recall unemployment spells is to assume that *all* unemployed are subject to the competing risks of a recall and a new job (Katz, 1986; Jensen and Westergård-Nielsen, 1990; Mavromaras and Rudolph, 1998; Jensen and Nielsen, 1999; Rosholm and Svarer, 2001). In our data, there is one group for which the recall probability is known to be substantial (the recall unemployed) and one group for which it is very small (the ordinary unemployed). Therefore, we divide the unemployed into two separate risk sets. Since we do not observe the nature of each job transition, we treat employment as a single destination state. Temporary dismissed workers are also at risk of being permanently dismissed, and hence make a transition to ordinary unemployment. Therefore, for the temporary dismissed workers we estimate a competing risks model for transitions to employment and ordinary unemployment, while for permanently dismissed workers, we estimate a single risk model for transitions to employment[4]. Let $j=r,o$ be the two *origin* states of recall unemployment (r) and ordinary unemployment (o), respectively and let $k=e,o$ be the two possible *destination* states of employment (e) and ordinary unemployment. Let $q_{ijk}(t,d)$ be the hazard rate by which an individual i transits from state j to state $k\,(\neq j)$ at calendar time t and process time (duration) d, and assume (for reasons of tractability and interpretability) that the hazard rates are proportional in factors depending on calendar time, spell duration, individual observed- and unobserved characteristics and (for temporary dismissed workers) the degree of Social Security exhaustion. Since our observations are collected by the end of each calendar month only, we formulate our model in terms of grouped (discrete) hazard rates. Let $\bar{t}_i$ be the time at which a spell was first recorded. Assume for simplicity that the hazard rates are constant within each obser-

[4] In order to avoid inessential complications, we exclude from the latter group persons entering ordinary unemployment from recall unemployment.

vation month. The process-time-specific integrated hazard rates, $j_{ijk}(\overline{t_i}, d)$, are then defined and parameterised in the following way

$$j_{ioe}(\overline{t_i}, d) = \int_{d-1}^{d} q_{ioe}(\overline{t_i}+u, u)du = \exp\left(CT'y_{oe} + SD'\,l_{oe} + e_{it}^{'}g_{oe} + x_{it}^{'}b_{oe} + v_{ioe}\right), \tag{1}$$

$$j_{ire}(\overline{t_i}, d) = \int_{d-1}^{d} q_{ire}(\overline{t_i}+u, u)du = \exp\left(CT'y_{re} + SD'\,l_{re} + SS'd_e + e_{it}^{'}g_{re} + x_{it}^{'}b_{re} + v_{ire}\right), \tag{2}$$

$$j_{iro}(\overline{t_i}, d) = \int_{d-1}^{d} q_{iro}(\overline{t_i}+u, u)du = \exp\left(CT'y_{ro} + SD'\,l_{ro} + SS'd_o + e_{it}^{'}g_{ro} + x_{it}^{'}b_{ro} + v_{iro}\right), \tag{3}$$

where *CT* and *SD* are vectors of calendar time- and spell duration dummies respectively, and *SS* is a vector of dummy variables indicating exhaustion of the social security period (relevant for recall unemployed only). The vectors $(y_{oe}, y_{re}, y_{ro}, l_{oe}, l_{re}, l_{ro}, d_e, d_o)$ represent the calendar time, spell duration and Social Security exhaustion baseline hazard rates, respectively (one element in each of these vectors is normalised to zero). We do not include separate dummies for the *individuals'* exhaustion of unemployment benefits, since the modest variation in these rules during the 1990's are insufficient for separating these effects from other types of spell duration effects (all the claimants in our dataset were eligible for around 18 months of benefits). Any causal effects of benefit exhaustion will therefore be embedded in the spell duration baselines. The vector e_{it} contains individual economic incentives as reflected in unemployment benefits and the predicted wage. The vector x_{it} contains a number of control variables such as age, gender, educational attainment, region, industry, previous income etc. We also include dummy variables reflecting the Social Security regime prevailing at the time of entry into unemployment, in order to capture regime-specific selection mechanisms. The variables $v_i = (v_{ioe}, v_{ire}, v_{iro})$ are unobserved individual fixed characteristics affecting each of the three transitions, respectively.

Apart from the assumption of proportionality, the specification (1)-(3) is highly flexible; there are no parametric restrictions at all on the effects of calendar time, spell duration and Social Security exhaustion. Most of the variables (such as age, work experience and educational attainment) are dummy-coded in order to avoid arbitrary functional form restrictions. In total, the model contains more than 600 parameters to be estimated.

In order to derive the likelihood functions, let y_{ik} be binary indicator variables denoting transitions to the two alternative destination states, i.e. $y_{ik}=1$ if individual i transited to state k and zero otherwise. In the singel risk model for the ordinary unemployed, the contribution to the likelihood function of a spell lasting d_i months, conditional on v_i, is given as

$$L_{iv}\,|_{j=o} = \left[1-\exp\left(-j_{ioe}(\bar{t}_i, d_i)\right)\right]^{y_{ie}} \prod_{s=1}^{d_i-y_{ie}} \left[\exp\left(-j_{ioe}(\bar{t}_i, s)\right)\right]. \tag{4}$$

In the competing risks model for the recall unemployed, the likelihood contribution is given as (see Røed and Zhang, 2000)

$$L_{iv}\,|_{j=r} = \prod_k \left[\left(1-\exp\left(-\sum_k j_{irk}(\bar{t}_i, d_i)\right)\right)\frac{j_{irk}(\bar{t}_i, d_i)}{\sum_k j_{irk}(\bar{t}_i, d_i)}\right]^{y_{ik}} \prod_{s=1}^{d_i-\sum_k y_{ik}} \left[\exp\left(-\sum_k j_{irk}(\bar{t}_i, s_i)\right)\right] \tag{5}$$

Note that a partitioning of the likelihood (5) into separate terms that depend on parameters entering cause-specific hazard rates only, as suggested by Narendranathan and Stewart (1993), is *not* possible, even in the absence of unobserved heterogeneity. The reason is that the grouping of the hazard rates entail a dependency of the competing risks *within* each time interval. The existence of unobserved heterogeneity induces another source of interdependency between the marginal hazard rates because v_{ire} and v_{iro} may be correlated. We assume that the unobserved variables v_i is discretely distributed (Lindsay, 1983), with a number of mass-points chosen according to the Hannan-Quinn Information Criterion (see, Baker and Melino, 2000, for some Monte Carlo evidence favouring this type of criterion). These mass-points (or combination of mass-points) and their associated probabilities are estimated together with the other parameters of the model[5]. Regardless of the particular assumptions about unobserved heterogeneity, our data provides a unique foundation for disentangling the effects of structural duration dependence and Social Security exhaustion on the one hand and unobserved heterogeneity on the other. The main reason is that there is a large variation in *lagged explanatory variables*, conditioned on individuals' current explanatory variables. Brinck (2000) proves that variation in covariates over time, combined with variation in covariates across individuals, is sufficient for non-parametric identification of structural duration dependence and unobserved heterogeneity without the assumption of proportional hazards. Intuitively, identification rests on the idea that the conditional expectation of unobserved heterogeneity at any point in time depends on transition rates experienced earlier in the spell, while structural duration dependence (and Social Security exhaustion effects) does not (van den Berg and van Ours, 1994; 1996). In the present case, variation in lagged exit rates conditional on current exit rates is primarily driven by variation in calendar time itself, i.e. business- and seasonal cycles. Persons with the same spell duration have been exposed to different business- and seasonal cycle conditions earlier in the spell; hence they have been subject to different selection mechanisms. For example, a temporary dismissed person who is still unemployed after d months of unemployment, will clearly have a lower expected value of v_{irk} the higher the probabilities of exiting to state k has been earlier in the spell ($k=e,o$). Identification is also aided by the existence of repeated unemployment spells, since v_i is assumed constant at the individual level across different spells. As proved by Elbers and Ridder (1982), the Mixed Proportional Hazards model that we use in the present paper is identified even without variation in lagged covariates and without multiple spells; variation in

[5] The computational task of estimating these models with millions of observations and several hundred parameters is formidable and far beyond the scope any existing statistical software package. We have received invaluable programming- and estimation assistance from Simen Gaure at the Computing Resource Centre at the University of Oslo.

current covariates is sufficient. However, in that case the identification rests heavily on the proportionality assumption, for which we have no theoretical justification. And even though practical considerations has led us to impose Mixed Proportional Hazards, as well as other functional form restrictions, we do not want our qualitative results to be completely driven by these assumptions. We consider the existence of non-parametric identification without proportionality assumptions to be important because it implies that qualitative results are robust towards the actual form of the restrictions imposed.

5 Results

The Hannan-Quinn Information Criterion yielded six unobserved mass-points in the single risk model, and 16 mass-points in the competing risks model. When the number of mass-points was increased beyond these numbers, there were no discernible changes in the parameter estimates associated with observed factors, not even in the estimated spell duration baseline. The same happened when the number of mass-points was slightly reduced. This confirms that the exact number of mass-points in the mixing distribution is of minor importance for the identification of duration dependence when the purely databased foundation for non-parametric identification is sufficiently sound.

Figure 1 presents the estimates of structural duration dependence for ordinary unemployed and for recall unemployed (for the latter group when the remaining Social Security period is at least 7 months). For permanently dismissed workers, there is weak positive duration dependence in the employment hazard during the first three months of unemployment (the hazard rises with around eight per cent), after which there is a fairly long period with negative duration dependence (after one year of unemployment, the hazard is around 25 per cent below its initial level). Then, there is a rise in the hazard rate of approximately 14 per cent after around 18 months of unemployment, which corresponds to the time at which the first unemployment benefit period is exhausted (see section 2).

For temporary dismissed workers, there is positive duration dependence in the employ-ment hazard (the hazard rises with around 30 per cent) during the first 6 months of the spell, after which there is a monotonous negative duration dependence. As expected, there is no rise around the time of benefit exhaustion, since (the few remaining) temporary dismissed workers receive their ordinary wage at this stage of the spell. The hazard for transitions to ordinary unemployment displays a very strong (and fairly monotonous) positive duration dependence. Hence, the longer a recall spell has lasted, the more likely it is that it ends in ordinary unemployment rather than in the intended recall.

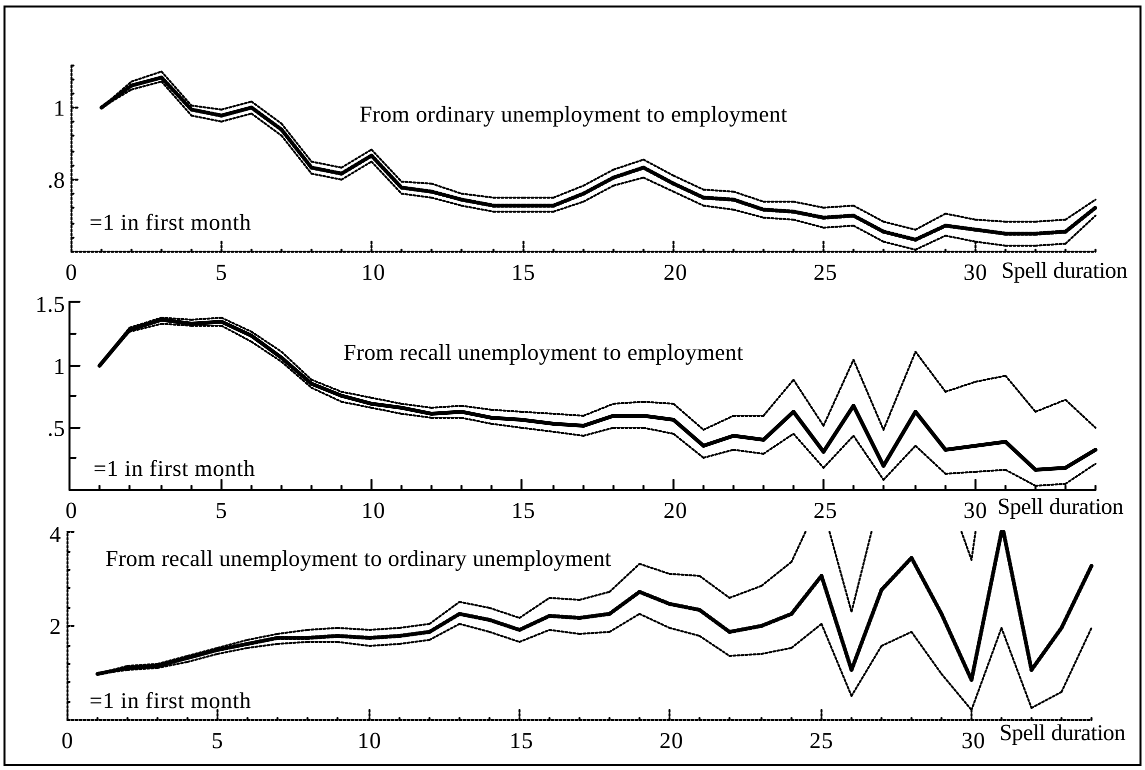

Figure 1. Estimated spell duration baseline hazard rates ($\exp(\hat{l}_{oe})$, $\exp(\hat{l}_{re})$ and $\exp(\hat{l}_{ro})$ with 95 per cent point-wise confidence intevals).

Figure 2 presents our estimates regarding the effects of Social Security exhaustion for temporary dismissed workers. They leave little doubt that the firms' willingness to recall, as well as to dismiss permanently, their temporary dismissed workers, responds strongly to the degree with which the costs can be passed on to others. According to the point-estimates, the rehiring- and permanent dismissal rates increase by factors of 3.5 and

7.5, respectively, when the public subsidies are fully exhausted, compared to a situation with at least seven months left of these subsidies.

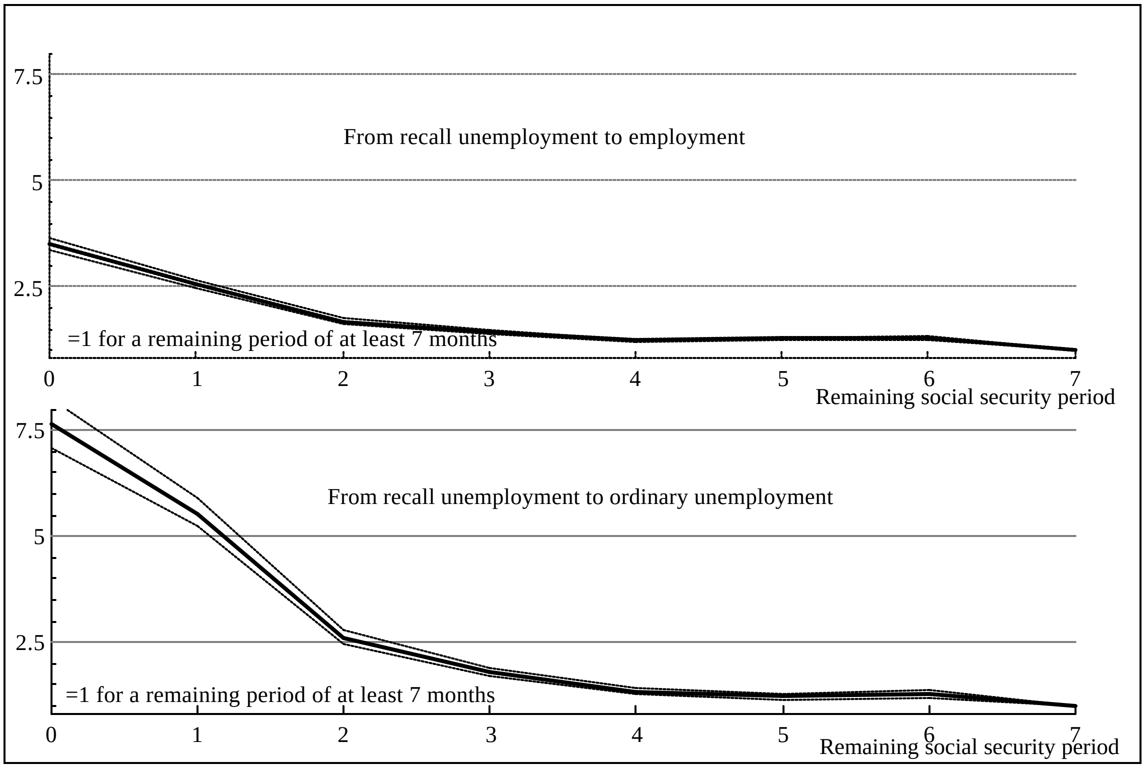

Figure 2. Estimated effects of ‘time until Social Security exhaustion’ on transitions from recall unemployment ($\exp(\hat{d}_e)$ and $\exp(\hat{d}_o)$ with 95 per cent point-wise confidence intevals).

In Figure 3, we have combined the estimated effects of Social Security exhaustion and spell duration in order to illustrates what the monthly transition probability pattern have looked like under the various Social Security regimes for a reference temporary dismissed person[6]. As expected, there are huge differences in the transition pattern between the various regimes. This is perhaps even more clearly demonstrated in terms of expected durations. The expected duration of the reference person’s recall unemployment spell (assuming constant hazard rates after three years of unemployment) was reduced from 7.8

[6] The reference person is a male benefit claimant of age 26-30 with secondary education living in the Oslo-area and working in the transport sector, facing ‘mean’ business cycle conditions and having the mean value of other covariates (including unobserved heterogeneity). According to the model, other persons may have different probability *levels*, but the same spell duration pattern.

months under the most generous regime (R1) to 3.3 months under the strictest regime (R2), and then increased again to around 5.0 months under the present regime (R5).

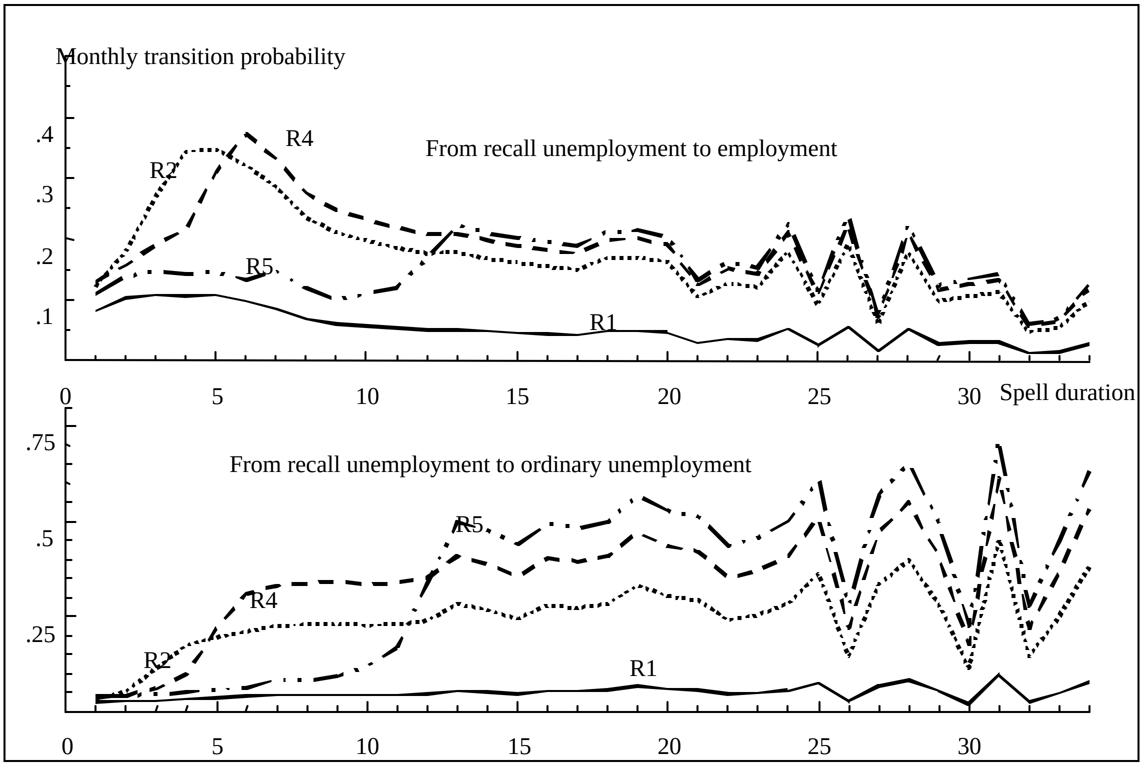

Figure 3. Predicted transition probabilities out of recall unemployment for 'the reference person' under various Social Security regimes.

Table 3 provides estimation results for other parameters of interest. Note that parameter attached to the log of a continuous variable has the interpretation of an elasticity (the elasticity of the hazard rate with respect to the variable in question), while the exponential function of a parameter attached to a dummy measures the hazard rate for a person with the dummy equal to one relative to a reference person. Individual economic incentives have the expected effects on ordinary unemployed workers. Non-claimants exit more than twice as fast as claimants do. The elasticity of the hazard rate with respect to unemployment benefits is around –0.25, while the elasticity with respect to the experience based predicted income is around 0.33. The human capital based predicted income, however, has a 'wrongly' signed coefficient. This is probably because the human capital based predicted income, conditional on the previous actual income, act as a proxy for unobserved ability (a low actual income combined with a high predicted income based on observed human capital variables may signal unfavourable unobserved characteristics). Also among recall unemployed, non-claimants exit faster than claimants do. However, the marginal effect of unemployment benefits on the hazard rate to employment is *positively* signed, with an elasticity of 0.2. This confirms the implicit contract hypothesis and suggests that frequently dismissing firms allocate unemployment to workers with high benefits. As expected, there is no effect of the size of unemployment benefits on the transition from recall- to ordinary unemployment (the benefit level is the same in these two states). The predicted income has a negative effect on the employment hazard, suggesting that firms are more reluctant to recall temporary dismissed workers the higher are the associated wage costs. To sum up the role of economic incentives, it seems that while ordinary unemployment spells are explained primarily by individual search behaviour, recall unemployment spells are largely explained by firm behaviour (or implicit contracts between workers and firms). This pattern is also confirmed by the coefficients attached to the industry-dummies, which indicate that industry affiliation has a much larger impact on recall spells than on ordinary spells.

Table 3.
Selected parameter estimates (standard errors in parentheses).

	Single Risk Model	Competing Risks Model	
	From ordinary unemployment to employment	From recall unemployment to employment	From recall unemployment to ordinary unemployment
Economic incentives			
Unemployment benefits (log)	-0.254** (0.012)	0.204** (0.026)	0.046 (0.055)
No benefits (dummy)	0.856** (0.004)	0.369** (0.008)	0.328** (0.019)
Experience based predicted income (log)	0.329** (0.011)	-0.151** (0.021)	-0.083* (0.043)
Human capital based predicted full time income (log)	-0.421** (0.039)	-1.512** (0.084)	-0.199 (0.168)
Industry			
Transport	ref.	ref.	ref.
Tourism (hotels and restaurants)	-0.006 (0.007)	0.099** (0.018)	0.104** (0.034)
Manufacturing/construction	0.005 (0.005)	0.309** (0.012)	0.003 (0.024)
Education and demographics			
Primary school	-0.191** (0.007)	-0.171** (0.018)	-0.218** (0.035)
Secondary school, specialisation within ‘recall sectors’	-0.224** (0.007)	-0.157** (0.016)	0.002 (0.031)

Table 3.
Selected parameter estimates (standard errors in parentheses).

	Single Risk Model	Competing Risks Model	
	From ordinary unemployment to employment	From recall unemployment to employment	From recall unemployment to ordinary unemployment
Secondary school, specialisation within other vocational topics	0.024** (0.006)	0.043** (0.014)	-0.059 (0.028)
Secondary school, general subjects	ref.	ref.	ref.
College engineer	0.067** (0.017)	0.125** (0.029)	0.079 (0.060)
College other	0.341** (0.010)	0.268** (0.031)	0.231** (0.060)
University	0.382** (0.014)	0.316** (0.035)	0.028 (0.069)
Age 16-18	0.467** (0.018)	0.642** (0.111)	-0.395 (0.263)
Age 19-21	0.352** (0.008)	0.414** (0.024)	-0.044 (0.049)
Age 22-25	0.169** (0.006)	0.173** (0.012)	-0.007 (0.024)
Age 26-30	ref.	ref.	ref.
Age 31-35	-0.139** (0.006)	-0.067** (0.011)	-0.032 (0.023)
Age 36-40	-0.232** (0.007)	-0.126** (0.014)	-0.020 (0.029)
Age 41-50	-0.331** (0.008)	-0.166** (0.015)	-0.032 (0.030)
Age 51-60	-0.538** (0.010)	-0.220** (0.016)	-0.157** (0.034)
Age > 60	-0.987** (0.013)	-0.316** (0.019)	-0.419** (0.042)
Women	-0.286** (0.006)	-0.170** (0.013)	-0.122** (0.027)
Married	0.105** (0.004)	0.079** (0.006)	-0.055** (0.014)
Immigrant from Non-OECD country	-0.380** (0.009)	-0.297** (0.024)	0.270** (0.043)
Work experience			
Less than a year	-0.017 (0.011)	0.057 (0.078)	0.388** (0.126)
1 year	0.025** (0.009)	0.042 (0.024)	0.122** (0.042)
2 years	0.045** (0.008)	0.004 (0.018)	0.043 (0.033)
3-5 years	ref.	ref.	ref.
6-10 years	-0.011 (0.006)	0.081** (0.012)	-0.097** (0.022)
11-14 years	0.037** (0.008)	0.211** (0.016)	-0.185** (0.032)
15-19 years	0.088** (0.009)	0.307** (0.019)	-0.186** (0.032)
20 years or more	0.109** (0.011)	0.395** (0.020)	-0.337** (0.040)
Having some part-time work while unemployed	0.570** (0.004)	0.426** (0.005)	0.058** (0.011)
Regime at entry			
R1	ref.	ref.	ref.
R2	-0.036** (0.008)	0.063** (0.023)	-0.272** (0.037)
R3	-0.003 (0.01)	0.219** (0.039)	-0.159** (0.063)
R4	0.021 (0.012)	0.241** (0.050)	0.185* (0.084)
R5	-0.02 (0.016)	0.313** (0.055)	0.460** (0.100)
Unobserved fixed effects			
Mean	-3.022	-2.884	-4.225
Standard deviation	0.503	0.595	1.205
Correlation in competing risks		-0.257	
Log-likelihood	-1534037.90	-613067.10	
Number of estimated parameters	205	421	

Notes: In addition to the above coefficients, there are calendar time dummies, spell duration dummies, Social Security dummies (the results for the two latter are presented graphically), controls for region, last years income and dummies for missing information about previous income, benefits and education.
* (**) Significant at the 5 (1) per cent level.

The effects of individual characteristics, such as education, age, gender and marital status are as expected. The general pattern is that the estimated effects are stronger for the ordinary- than for the recall unemployed. Educational attainment has a fairly large effect on the two employment hazard rates, but virtually no effect on the transition from recall- to ordinary unemployment. The employment hazards decline more or less monotonically as a function of age. Work experience (conditional on age) enhances the employment hazard. This effect is much stronger for the recall unemployed than for the ordinary unemployed, suggesting that there is a marked seniority element in firms' recall decisions. This is also consistent with the strong negative effect of work experience in the transition rate from recall- to ordinary unemployment.

Unobserved heterogeneity is responsible for a substantial degree of variation in the hazard rates, particularly in the transition from recall- to ordinary unemployment. An increase in the unobserved covariates of around one standard deviation raises the two employment hazard rates by around 60 per cent and the hazard rate from recall- to ordinary unemployment by 200 per cent. For recall unemployed, there is also a negative correlation (-0.26) between unobserved characteristics affecting each of the two hazard rates, suggesting that persons with a high re-employment probability has a relatively low probability of becoming permanently dismissed. There are some selection effects associated with the changes in Social Security regime. When the rules were tightened in October 1990 (R2), the temporary dismissed workers became a more positively selected group, with better reemployment prospects and lower risk of permanent dismissal than what was previously the case. The group of permanent dismissed workers became a slightly more negatively selected group, although these effects seemed to be very short-lived.

6 Concluding remarks

The duration pattern of *recall* unemployment spells is highly sensitive towards *firm* incentives. Limitation of the period by which firms can pass temporary dismissal costs on to others strongly effects the transition rates out of recall unemployment. A short-lasting reform in Norway, which reduced the Social Security period associated with temporary dismissals from one and a half year to three months, reduced the expected duration of recall unemployment spells (for a reference person) from around eight to three months. The duration pattern of *ordinary* unemployment spells is sensitive to *individual* economic incentives. The employment hazard rate is twice as high for non-claimants as for claimants, but marginal changes in unemployment benefits seem to have only modest effects (a 10 per cent reduction increases the employment hazard by 2.5 per cent).

It is clear that the pattern of ordinary- and recall unemployment spells is determined through very different causal mechanisms. While ordinary unemployed fits well into the standard search- and reservation wage framework, the apparent behaviour of recall unemployed is probably to a large extent driven by the firm or by implicit contracts between the firm and the worker.

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