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Unemployment Insurance in Welfare States: Soft Constraints and Mild Sanctions

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Abstract

Based on a sequence of reforms in the Norwegian unemployment insurance (UI) system, we show that soft duration constraints and mild sanction practices significantly increase UI claimants' job hazard rates. Soft duration constraints appear to have almost the same behavioral impacts as hard constraints. In particular, the spikes at benefit exhaustion are of similar magnitude, irrespective of the kind of services and benefits that are offered after exhaustion, e.g., in terms of follow-on benefits and access to paid labor market programs. We find that extensive use of activity requirements backed up by mild sanctions effectively speed up the job search process and cut unemployment duration.

Keywords: Competing risks, unemployment insurance, timing-of-events, NPMLE, MMPH

JEL classification: C14, C15, C41, J64, J65, J68

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1 Introduction

Welfare states face a dilemma regarding the design of unemployment insurance (UI) systems. On the one hand, the relatively generous replacement ratios that are embedded in these systems call for tight benefit duration limits, strict work-tests, and austere sanction practices in order to offset moral hazard problems. On the other hand, the prevalence of multiple layers in the social security safety net, e.g., in the form of sickness insurance, rehabilitation benefits, housing subsidies, and means-tested welfare assistance, often render threats of complete benefit termination non-credible. Given a political obligation of poverty prevention, a more thrifty UI system may simply shift insurance costs over to other social security programs. European policy makers have responded to this dilemma by developing "soft" duration constraints and "mild" sanction practices within their UI systems. For example, rather than setting a definite maximum UI benefit period, duration limitations are often confined only to the most generous and unconditional form of income support. When this period expires, benefits may still be maintained, but at a somewhat lower level than before and/or conditional on participation in active labor market programs (ALMP). Sanctions are imposed (usually at the discretion of case workers) when claimants fail to respond to summons by the employment office, when suitable job offers are rejected, or when job search efforts are deemed inappropriate. But normally, a sanction means that benefits are terminated or reduced for a relatively short period of time only, e.g., 4-8 weeks.

The purpose of the present paper is to evaluate empirically the behavioral effects of soft constraints and mild sanctions within a well-developed welfare state economy, i.e., Norway. We focus on two questions. The first is the extent to which the various policy measures elicit the intended behavioral responses in terms of more vigorous job search and, consequently, higher job finding rates. The second is the extent to which they generate the unintended side effect of benefit shifting. The latter of these topics has so far been virtually ignored in the literature, despite its potential empirical importance (25 percent of the completed unemployment spells in our data end in a transition to another type of benefit). In order to identify the causal effects of interest, we take advantage of the fact that Norway has been through substantial reforms in its UI maximum duration regulations. Our empirical basis is a set of merged administrative register data that track all registered unemployment spells and their outcomes on a monthly basis from November 1993 to October 2001 (1.1 million spells). The data allow us to investigate the impact of UI regimes on the transition rates into employment, to other kinds of publicly provided benefits, to active labor market programs, and to ordinary educational activities.

As we explain in the next section, there exists a rich, yet inconclusive, literature on the behavioral impact of benefit duration constraints and sanctions in European UI systems. The inconclusiveness emanates from institutional differences and data limitations, as well as from methodological difficulties. An important issue that arises in analyses of benefit exhaustion effects is how to control for other sources of duration dependence in the hazard rates. Unemployment duration may have a direct effect on the escape rates from unemployment for a number of reasons, such as discouragement, loss of general skills, and statistical discrimination against long-term unemployed. For a given individual, one additional month of insured unemployment obviously also implies that the point of exhaustion has come one month closer, hence there is a fundamental colinearity problem associated with these two variables. This is why institutional reforms become important for identification; the existence of regime shifts breaks the perfect correlation between spell duration and benefit exhaustion among claimants. There is, however, an important conceptual difference between the role of unemployment duration with respect to discouragement and statistical discrimination, on the one hand, and the role of unemployment duration with respect to exhaustion of benefit entitlements, on the other: While the latter of these duration concepts has a clear-cut interpretation and can be accurately measured (given sufficiently reliable data), the former is both vague and ill-defined. For example, a number of persons move into and out of unemployment several times during a short time interval. While benefit exhaustion rules regulate the conditions under which repeated spells are to be counted as belonging to the same maximum benefit duration period or not, there are no rules that can be called upon to tell us whether they should also be counted as one spell in terms of, e.g., discouragement or discrimination effects.

The standard procedure used by econometricians is to reset the “duration clock” every time a new spell starts, implying that persons moving frequently into and out of unemployment never become “long-term unemployed”. This procedure may be inadequate in terms of capturing the true pattern of structural duration dependence. And, more seriously in the present context, it may be responsible for inducing a bias in parameters that are identified on the basis of institutional reforms. The reason for this is that since the level of unemployment typically varies over time, it is generally not the case that the unmeasured parts of unemployment durations (from previous spells) are equally distributed between pre and post reform periods. If the analysis controls for the duration of the ongoing spell, but fails to consider the duration of past spells, the estimated spell duration parameters will fail to pick up the true duration dependence effects. And even more seriously, if benefit exhaustion is accurately recorded, this variable will serve as a proxy for past unemployment, hence its impact on the employment hazard will not only reflect a causal exhaustion effect.

In the present paper, we take advantage of the fact that our data contain information on individual unemployment exposure during a four-year period prior to each new unemployment spell, and we control for "lagged" duration (related to previous spells), as well as for "ongoing" duration in our efforts to identify benefit exhaustion effects. Around 65 percent of the entrants into unemployment in our data have been unemployed before during the last four years, and as much as 43 percent of the entrants completed a previous spell less than one year prior to the start of the new one. We analyze repeated spells within a simultaneous modeling framework; hence, "lagged" unemployment duration is treated as related to "current" unemployment duration not only through its causal effect, but also through the persistence of unobserved covariates.[1] We set up a competing risks hazard rate model with six endogenous events. Three of these events, i.e., full employment, a shift to another type of benefit, and a start of ordinary education, are final, in the sense that they terminate the unemployment spell.[2] The other three events, i.e., a start of ALMP participation, a benefit sanction, and access to some part-time work (while still registering as job searcher), do not terminate a spell, but are allowed to have causal effects on subsequent hazard rates. Unobserved heterogeneity is modeled in terms of a joint discrete mixture distribution, estimated by means of the non-parametric maximum likelihood estimator (NPMLE); see Lindsay (1983) and Heckman and Singer (1984). In contrast to most existing applications, we impose no a priori restrictions on the number of support points. As a result, we end up with as much as 41 support points for our six-dimensional vector of unobserved heterogeneity.

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[1] Note that there is an "initial conditions problem" associated with lagged unemployment that was experienced prior to the start of our modeling period. We return to this issue later on.

[2] The term "full employment" is used to denote employment that is combined with termination of the registration as unemployed job seeker. A number of individuals get some (occasional) part-time work while continuing to search for more satisfactory employment. This is defined as a transition to part-time work without terminating the spell.

A main finding of our paper is that an "activity-oriented" UI regime – with a high probability of required ALMP participation, duration-limited unconditional UI entitlements, and high sanction probabilities – delivers substantially shorter unemployment spells than a more "income-insurance-oriented" regime. In line with the existing literature, we find that the job hazard rises significantly during the very last months of the UI entitlement period. More interestingly, the behavioral response seems to be almost the same regardless of the "harshness" of the duration constraint. A soft constraint offering generous UI benefit renewal options or follow-on benefits generates basically the same responses as a hard constraint offering no further income support from the employment office. A possible interpretation of this finding is that all constraints that have been used in Norway are really soft, given the existence of alternative transfer schemes. However, the predicted fall in average unemployment duration associated with a reduction in the length of the UI period through the imposition of a soft constraint is around half a day for every week's reduction. This effect is of exactly the same magnitude as that reported by Card and Levine (2000) on the basis of an extended benefit program in New Jersey, USA. We find this similarity intriguing, given that for most job-seekers, the soft constraint imposed in Norway does not really affect the absolute duration limit of UI benefits at all; it only advances the moment at which some form of activity is demanded. The favorable result of "activity-orientation" is obtained despite that *actual* participation in labor market programs is found to lengthen the participant's expected unemployment duration (including the participation period) by approximately five weeks. Actual ALMP participation also affects the distribution of final outcomes (destinations). In particular, it raises the probability that the participant's spell eventually ends in employment by around two percentage points.

The next section briefly reviews the existing evidence. Section 3 provides a description of the data, the state space, and of the policy reform used to identify the effects of interest. Section 4 presents the statistical model, and Section 5 discusses the results. Section 6 concludes.

2 Previous Research

Existing empirical evidence from the U.S. clearly establishes that the threat of losing UI benefit entitlements within the near future has a significant impact on the claimants' exit rates from the unemployment system (Katz and Meyer, 1990; Meyer, 1990; Card and Levine, 2000). However, in a recent overview of the literature, Card, Chetty and Weber (2007) show that the reported spikes in hazard rates tend to be much smaller when the spells are measured by the time to next job than when they are measured by the time spent in the unemployment system. Evidence from Europe is also less clear-cut. An influential early contribution to this literature is Hunt (1995), who, based on a set of UI duration extensions for elderly workers in West Germany during the 1980's, concludes that the behavioral impact of the maximum benefit duration in Germany is of the same magnitude as in the U.S., despite the existence of follow-on benefits in Germany after UI exhaustion. Similar, though somewhat weaker, responses are reported for Austria by Winter-Ebmer (1998), also based on a benefit extension for elderly workers. The generality of these findings may be limited, however, since they are based on reforms that to some extent were aimed at coaxing elderly people to leave the labor market, in the hope that this would create jobs for younger folks. Hence, the results may to some extent be driven by the (fully intended) usage of the UI system as an informal route towards early retirement. Indeed, Fitzenberger and Wilke (2004) show that conditional on the job seekers' eventual return to employment the reforms in West Germany had no effect at all on the speed at which this happened.

There is also some quasi-experimental evidence from Denmark, based on a sequence of reforms that imposed stricter activity requirements. The maximum duration of unconditional UI benefits in Denmark has gradually been reduced from four to one year, after which continued income support is conditional on participation in ALMP. The empirical evidence suggests that these reforms had a large positive effect on the transition rate from unemployment to employment, particularly in the period just prior to exhaustion of unconditional benefits (Geerdsen, 2006). Sweden has for a long time practiced a UI system with a relatively short unconditional UI benefit period (60 weeks). After that, continued income support is conditional on participation in ALMP. There is some evidence (Carling *et al.*, 1996; Carling, Holmlund and Vejsiu, 2001, Røed, Jensen, and Thoursie, 2008) indicating that there is a spike in the job hazard rate in the period just prior to exhaustion of unconditional benefits. The findings in Carling Holmlund and Vejsiu (2001), however, are challenged by Johansson and Selén (2002) on the ground that the employment concept used includes some forms of subsidized employment, and that the results critically hinges on these outcomes.

In contrast to Denmark and Sweden, the Norwegian UI system has no clearly defined activation period. For most of the 1990's, the benefit period has been divided into two distinct parts, separated by a quarantine period (see next section for details). After 1991, an exemption rule made job seekers entitled to escape the quarantine, insofar as they were not themselves to blame for their continued unemployment. Existing evidence (Røed and Zhang, 2003; 2005) indicate that there was a conspicuous spike in the job finding rate around the time of exhaustion of the first benefit period, despite the generous exemption rules and renewal options. The only reform-based evidence from Norway, however, is provided by Bratberg and Vaage (2000), who evaluate the introduction of the exemption rule from the quarantine, which they argue, in practice, extended the length of the UI period to more than three years. And interestingly, their main conclusion is that the softening of the UI duration constraint did not affect the transition rate into employment at all.

Direct evidence regarding the impact of sanctions is limited. Based on randomized trials in four U.S. states, Ashenfelter, Ashmore, and Deschênes (2005) report that tighter control on UI claimants' job search behavior had little impact on the total duration of such claims. Abbring, Van den Berg, and Van Ours (2005) use the timing of events approach (Abbring and Van den Berg, 2003) to examine the impact of sanction practices for unemployed job seekers in the Netherlands. Their finding is that sanctions, in terms of punitive benefit curtailments in response to inadequate job search, have substantial favorable effects on re-employment rates. There is, to our knowledge, no Scandinavian evidence on this issue.

There is also little evidence regarding the impact of UI benefit exhaustion on the take-up rates of other social security benefits. For Norway (Røed and Zhang, 2005; Henningsen, 2008) and Sweden (Larsson, 2006), there exists some evidence that the take-up rate of sickness insurance benefits increases as the point of UI exhaustion comes closer.

3 Data and Policy Reform

The data that we use comprise all new unemployment spells recorded in Norway during the period from November 1993 to October 2001, with information on past unemployment back to 1989. Throughout this period, a number of different UI regimes have been at work, depending on individual characteristics as well as on time, see Table 1. The Norwegian UI system is compulsory. The requirement for being entitled to UI benefits is (with some exceptions) defined in terms of earnings in the calendar year directly preceding the year in which the unemployment spell started, or the average of the last three years (see Røed, and Zhang, 2005, for details). These earnings must exceed approximately 8,000 USD per year. If this condition is satisfied, the UI benefit is set to 62.4 percent of previous earnings, up to a ceiling (in the base earnings) of around 40,000 USD. These basic rules have been stable throughout the period, apart from some minor increases in the minimum income requirements. However, the rules regarding maximum benefit duration, and the way they have been practiced have changed substantially. Until 1997, the standard maximum benefit duration was 80 weeks. But, after a 13-week quarantine period, a new 80-week period could be granted at a benefit level 10 percent lower than in the first period. It was possible to apply for benefits even in the quarantine period (i.e., an exemption). It was also possible to apply for a benefit extension of 13 weeks after exhaustion of the second benefit period. The absolute maximum duration was 80+13+80+13=186 weeks. We therefore view this system as having a 186-week absolute duration limit, but with soft constraints imposed after 80 and 173 weeks. In January 1997 this system was replaced by a single maximum benefit period of 156 weeks for most of the job seekers, but only 78 weeks for individuals with low previous earnings (below approximately 15,000 USD per year). However, individuals with the longest UI entitlement periods who also had a strong attachment to the labor force prior to the unemployment spell are, according to this new system, also entitled to a so-called "wait-period benefit" after the 156-week benefit period is exhausted.[3] Wait-period benefits pay around two thirds of previous UI benefits, but they have no maximum duration limit. Wait-period benefits are only supposed to be handed out if no suitable ALMP can be offered, and job seekers rejecting ALMP participation will lose their wait-period benefit entitlement.

Throughout the period, there has been a special rule regarding former state-employees, who became unemployed due to reorganizations within the public sector. These individuals have been entitled to maximum benefit durations from 3 to 17 years, depending on age, without the need to apply for extensions.[4] There have also been separate rules for individuals who have completed military service, in that they have been exempted from the standard eligibility requirements, but also had shorter maximum benefit duration (26 weeks). All the benefit claimants examined in this paper were obliged to accept job or ALMP offers deemed (by the case worker) to be appropriate, and rejection of such offers could provoke a sanction in the form of an 8-week quarantine period without benefits. In practice, such quarantines were rarely imposed during the first months of a job search period.

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[3] "Strong attachment" is defined as at least three years of work experience during the four years prior to the year of entry into unemployment.

[4] The benefit level is in these cases calculated as 66% of the wage rate at the time of job loss multiplied by min(tenure,30) divided by 30; i.e. the replacement ratio is 66% for individuals with at least 30 years tenure in the state sector.

Table 1
A Summary of the UI Benefit Regimes in Norway 1993-2002

Regime	Description	Requirement	Applies to spells starting
I	Old regime: 80 week UI period, followed by 13 week quarantine (subject to exemption) followed by new 80-week period (on slightly reduced benefits) followed by an additional 13 week period (on application)	Satisfying UI income criterion, or has just completed military service	Before August 1995
II	Intermediate regime: 80 week UI period, followed by the possibility of applying for a new 78-week period (on slightly reduced benefits).	Satisfying UI income criterion	August 1995-December 1996
III	New regime, without eligibility for wait-period benefits: 156 week UI period.	Satisfying the highest of the two UI income criteria	From January 1997
IV	New regime, with eligibility for wait-period benefits: 156 week UI period, followed by indefinite wait-period benefits (two thirds of UI benefits)	Satisfying the highest of the two UI income criteria and a requirement of strong labor force attachment during past three years	From January 1997
V	New regime for persons with low past income: 78 week UI period.	Satisfying the lowest of the two UI income criteria	From January 1997
VI	New regime for individuals completing military service: 24 week UI period.	Just completed military service	From January 1997
VII	Regime for individuals previously employed in the state sector: Long benefit periods, depending on age at the time of job loss: <35: 3 years 35-39: 5 years 40-44: 7 years 45-49: 9 years >49: Until retirement at 67 years	Lost a job in the state sector due to reorganization	Whole period (a reduction was imposed for new entrants after March 2002)
VIII	No benefits	No requirements	Whole period

Labor market programs have, throughout the period, been particularly targeted at the long-term unemployed. In the pre-1997 system, the Public Employment Service (PES) also had a practice of using labor market programs as a sort of work-test in relation to applications for exemptions from the 13-week quarantine period and for a second 80-week UI benefit period. An implication of this policy was that many individuals approaching the first 80-week limit could not count on an automatic extension of the benefit period. A requirement of participation in ALMP in return for continued income support was a real “threat” (or opportunity). Individuals with an active unemployment spell (in the first UI benefit period) at the time of the reform (January 1997) remained in the old benefit regime, and had to apply for a second benefit period. But the 13-week quarantine period was removed for this group.

Table 2 provides an overview of the data. In total, 665,068 individuals experienced some kind of unemployment from November 1993 to October 2001, divided into 1,145,777 spells. An interesting feature of the data is that a number of individuals have experienced repeated unemployment spells in different UI regimes.

Table 2
Overview of Spells and Individuals

Number of individuals	665,068
Number of spells	1,145,777
Percent of individuals with more than one spell	41.16
Percent of individuals with spells in more than one regime	24.99

The data have point-in-time structure, such that unemployment status is updated by the end of each calendar month. In the empirical analysis this implies that we have to measure job search duration in terms of months, rather than weeks, and that we also have to “translate” the various maximum duration regulations to a monthly scale. There are four different ways in which an unemployment spell can end during the observation period: a job is obtained, the job search period is terminated and another type of benefit is taken up instead (sickness benefit, rehabilitation benefit, disability benefit, or social assistance), an ordinary education is started, or the job-seeker dies or leaves the country (or we lose track of the individual in question for unknown reasons). There are also three other events of interest that occur, but which do not imply that the spell ends: a labor market program activity is started off, some part-time work is obtained (but not sufficient to terminate the job search) or a benefit sanction is imposed. The latter can of course only happen as long as benefits are claimed. Our data represent a substantial improvement over register data used in previous Norwegian (and other register-based) studies, in that we identify the destination of each transition out of unemployment more accurately. In particular, we believe that the present dataset is the first to identify all forms of benefit shifting, including transfers to social assistance (paid for by the municipalities). In contrast to many previous studies on Scandinavian register data (Røed and Zhang, 2003; 2005; Geerdsen, 2006), we have also identified all job transitions positively (either from new registrations in the so-called employee register or from verifiable earnings) rather than “inferring them” from the lack of other

Table 3
Descriptive Statistics

	All	Regime (see Table 1) I	II	III	IV	V	VI	VII	VIII
Total number of spells	1145777	244295	99699	108049	53531	242639	21186	1906	374472
Mean duration (months)	6.23	8.42	5.47	5.95	5.24	6.52	3.47	15.63	5.14
Percent of spells completed with a transition in the observation period	78.01	83.4	75.76	75.09	73.34	82.34	81.45	68.47	73.66
Percent of completed spells ending in:									
Employment	57.58	64.57	69.24	80.42	61.43	64.83	56.98	68.35	36.70
Other benefit	24.68	19.92	17.73	13.94	22.77	20.85	10.66	30.04	37.16
Education	17.74	15.52	13.03	5.63	15.80	14.33	32.35	1.61	26.13
Percent of spells right censored*	21.99	16.60	24.24	24.91	26.66	17.66	18.55	31.53	26.34
Percent of spells involving:									
Discretionary sanction	1.40	2.66	1.71	1.57	1.26	1.88	0.64	-	-
ALMP	18.08	21.75	9.39	8.86	9.92	14.72	8.43	15.32	24.56
Part-time work	23.27	35.55	27.64	31.14	27.09	30.72	18.03	39.19	6.68
Average transition rate in the first duration month to:									
Employment	12.47	10.89	14.00	18.13	19.20	14.63	16.50	6.93	8.54
Other benefit	5.25	2.89	4.24	3.58	2.42	4.50	2.62	1.99	8.98
Education	5.08	3.13	4.12	3.92	1.42	4.98	12.34	0.16	7.96
Share with unemployment experience in the last four years (percent)	65.75	76.81	75.89	69.92	56.53	73.91	56.80	53.67	52.91
For individuals with unemployment experience:									
Average time since the end of the last spell	12.39	11.33	13.69	14.03	15.27	11.90	12.60	6.39	10.85
Average number of months unemployed	11.74	13.77	12.84	10.81	7.99	12.63	6.62	18.80	10.38
<u>Selected means and fractions (taken over spells)</u>									
Men (percent)	51.00	53.36	53.31	48.27	54.16	39.05	97.78	49.06	44.88
Married (percent)	23.14	26.56	24.14	20.50	34.84	22.88	1.00	59.39	18.71
Educational attainment (years)	11.54	11.62	11.66	11.71	11.70	11.56	11.74	10.89	11.30
Work experience (years)	4.54	6.19	5.99	3.48	11.41	3.52	0.27	19.04	1.14
Age (years)	29.14	30.47	30.86	28.81	36.00	28.93	21.32	45.14	25.67
Immigrants from OECD countries (percent)	3.19	2.32	2.48	4.43	3.02	3.19	0.23	1.31	4.10
Immigrants from Non OECD countries (percent)	10.15	3.94	5.19	11.05	5.20	9.37	1.72	2.36	19.22

*The most frequent observed reasons for right-censoring are death, emigration, childbirth, military service, and that the spell exceeds the end of the observation period. In 58 percent of the censoring-cases, the reason is unobserved.

positively identified transitions. This has previously been shown to be of great importance when modeling transition rates out of unemployment near the end of the UI-period; see Card, Chetty and Weber (2007). Table 3 provides a descriptive overview of the data. A key point to note is that only around 58 percent of the completed spells end in a direct transition to a job; 25 percent end in take-up of a new benefit, and 18 percent end in ordinary education. There are substantial differences in average spell duration between the different UI regimes, from 3.5 months for previous military personnel with a maximum benefit duration of only 24 weeks (regime VI) to 15.6 months for individuals with the most unlimited benefit duration (regime VII). These differences do of course not necessarily reflect causal regime-effects. It is evident from Table 3 that there are large differences in the composition of entrants into the various regimes.

4 The Statistical Model

We set up a multivariate mixed proportional hazard rate model (MMPH) with six competing events $k=1,...,6$: employment ($k=1$), another benefit ($k=2$), education ($k=3$), ALMP ($k=4$), a benefit sanction (loss of UI benefits before the benefit period has expired) ($k=5$), and part-time work (while still searching for more work) ($k=6$). The first three events terminate the spell. The latter three events do not terminate the spell, but are assumed to have causal effects on future hazard rates, both during and after the event.[5] The model is designed to disentangle the causal effects of these endogenous events from the observationally similar impacts of non-random sorting into them. The sorting processes are generated by the individuals' own choice behavior, as well as by administrative practices (for sorting into ALMP and sanctions) and employer behavior (for sorting into part-time work).

4.1 Specification of hazard rates

As we observe labor market status by the end of each month only, we set up the statistical model in terms of grouped hazard rates (Prentice and Gloeckler, 1978; Meyer, 1990). We write the integrated period-specific hazard rates φ_{kit} as functions of observed (time-varying) variables and

[5] Note that we treat the loss of benefits during the quarantine period of the old UI benefit regime as an endogenous sanction (since a large fraction of the claimants do not lose their benefits at this point), while the loss of benefits after complete UI exhaustion is treated as an exogenous event. It is clear that not all individuals are under risk of experiencing all six events all the time. For example, it is obviously only benefit claimants that can be subject to a sanction. We also assume that only individuals below 35 years of age are under risk of starting ordinary education (very few individuals above this age make a transition to education, but those who do are right-censored).

unknown parameters represented by index functions w_{kit}, and (time-invariant) unobserved individual characteristics v_{ki}:

$$\varphi_{kit} = \int_{t-1}^{t} \theta_{kis} ds = \exp\left(w_{kit} + v_{ki}\right), \quad k = 1, \ldots, 6, \tag{1}$$

where θ_{kis} is the underlying continuous-time hazard rate, which is assumed to be constant within each unit time period.

Before we specify the model in more detail, we introduce and discuss some of the key explanatory variables that will be included in the index functions w_{kit}. Time has two dimensions in our model; *calendar time* and *process time*. The calendar time dimension reflects business cycle and seasonal fluctuations and changes in government priorities (e.g., regarding the ALMP capacity and sanction practices). In the model, the effects of calendar time are represented in the most flexible way possible within the MMPH framework, i.e., by including a separate dummy variable for each of the 96 calendar months in our time window. These dummy variables are denoted $s_{it.}$ Process time (time since the spell started) affects the hazard rates through two different channels. First, it affects the cost of continued job search through the depletion of UI entitlements (Mortensen, 1977; Van den Berg, 1990). We label this “UI-generated duration dependence”. The characteristics of this duration dependence obviously depend on the specific properties of the UI regime. Second, unemployment duration may also affect hazard rates directly, through, e.g., discouragement, statistical discrimination, and administrative priorities regarding sanction practices and the allocation of program slots. We label this “intrinsic duration dependence”. A key contribution of this paper is to decompose empirically these two sources of duration dependence. Both the UI-generated and the intrinsic duration dependencies are modeled in a semi-parametric fashion, by means of comprehensive sets of dummy variables. As discussed in the introduction to this paper, intrinsic duration dependence may interact with recent unemployment experiences that do not belong to the current spell. To account for this possibility, we model intrinsic duration dependence as a function of three factors: i) the overall unemployment exposure during the four year period prior to the current spell, ii) the time that elapsed from the end of the last completed spell to the start of the current spell, and iii) the duration of the ongoing spell. These factors are interacted by estimating a separate piece-wise constant within-spell baseline hazard for 16 different entrant types defined by their previous unemployment exposure. In total, we use 160 dummy variables, denoted d_{it}, to represent the impacts of ongoing and past unemployment duration; see Appendix 1, Table A1, for details.[6]

The UI-generated duration dependence is essentially represented by a set of time-varying dummy variables, denoted b_{it}, reflecting initial UI entitlements and the remaining time until UI exhaustion. Separate dummy variable sets are used to count down to the different "soft" and the "hard" constraints. There are basically four types of duration constraints occurring in our data:

- A new UI period can be granted immediately after exhaustion, but with a 10 per cent benefit reduction.
- An eight week quarantine period is imposed, after which a new UI period can be granted with a 10 percent benefit reduction
- UI benefits are replaced by indefinite wait-period benefits, approximately 30-40 percent below original UI entitlements.
- There are no more benefits available.

We also include separate dummy variables for spells characterized by (almost) indefinite benefits and spells characterized by no UI benefits at all. A precise definition of the 41 UI dummy variables that we use – and their relation to the various regimes – is provided in Appendix 1, Table A2. Since assignment of UI regime is not generally randomized (conf. Section 3), not all the UI dummy variables can be assumed exogenous. Hence, some of the estimated parameters will not have a purely causal interpretation. We are going to assume, however, that differences in UI dummy variables that solely results from system reform do reflect causality. In particular, we will interpret the impacts directly associated with the UI system reform in 1997 as causal, controlled for the individual characteristics that determine assignment to the various post-reform regimes. Individuals with characteristics implying assignment to regime I prior to the reform were assigned to regimes III, IV, V and VIII after the reform, depending on their previous income and work experience. We control for this sorting by including dummy variables for (hypothetical) regime-assignment in the post 1997 UI system throughout the data period.

We assume that the events of ALMP, a sanction, and part-time work may have causal effects on the other hazard rates both during their occurrences (on-treatment effects) and after-

[6] In addition to the variables described in Table 4, we also include a single interaction term between spell duration (measured in month) and a variable measuring the cyclical situation at the time of entry. The business cycle variable is collected from Gaure and Røed (2007), and normalized to zero for a "mean" cyclical situation.

wards (post-treatment effects); see Røed and Raaum (2006). During their occurrences, the effects are allowed to vary with the progressing duration of the events.[7] Afterwards, the effects are allowed to vary with the completed duration of the events. All these effects are represented by 42 time-varying event indicators, denoted z_{it}; see Appendix 1, Table A3 for details. Finally, the model contains observed and unobserved individual characteristics. Observed characteristics, denoted x_{it}, may be time-varying, and the 104 variables we use include information about gender, age, work-experience, educational attainment, family situation, factors that determine regime affiliation, etc. A complete description of all variables used in the model and the way they are defined, specified, and interacted, is provided on our web-page http://www.frisch.uio.no/docs/UI_in_welfare_states.html.

We can now write the index functions as

$$w_{kit} = \sigma_{kt} s_{it} + \lambda_{kd} d_{it} + \delta_k b_{it} + \alpha_k z_{it} + \beta_k x_{it}, \qquad (2)$$

where $(\sigma_{kt}, \lambda_{kd}, \delta_k, \alpha_k, \beta_k),\ k = 1,...,6,$ are the parameters to be recovered from the data. Some of the explanatory variables in (2) are clearly endogenous, in the sense that they depend on the outcomes of the statistical process under consideration, and, hence, on unobserved heterogeneity. This is the case for the duration variables d_{it} (regardless of whether the duration belongs to the current or to previous spells) as well as for the state variables z_{it} (recording current or completed ALMP, sanctions, and part-time work). Apart from unemployment duration generated prior to the start of our observation window (which we return to below), this endogeneity is taken into account by means of modeling all events simultaneously, with a joint distribution of unobserved heterogeneity. This procedure has also become known as the "timing-of-events approach"; see Abbring and Van den Berg (2003).

Unobserved characteristics $v_i = (v_{1i}, v_{2i}, v_{3i}, v_{4i}, v_{5i}, v_{6i})$ enter into the model as person-specific "intercepts", designed to reflect variation in hazard rates that is not accounted for by observed individual characteristics. To the extent that unobserved characteristics are correlated to observed covariates, they will "contaminate" the parameters associated with these covariates. Hence, some of the explanatory variables serve a dual control purpose in the model; they capture

[7] Note that we treat the *duration* of these events – in the absence of a direct transition to one of the final destination states – as exogenously given, i.e., we do not model the statistical process of their termination. While we will argue that this is a reasonable assumption for ALMPs and sanctions, it is clearly not reasonable for part-time work. Hence, the estimated impact of part-time work *duration* may reflect a combination of sorting and causality.

both the causal effect of these variables and the extent to which they are correlated to unobserved characteristics.

4.2 Identification

The model is non-parametrically identified, not only on the basis of the mixed proportional hazard assumption (Abbring and Van den Berg, 2003), but also on the basis of repeated spells (Van den Berg, 2001; Abbring and Van den Berg, 2003) and time-varying explanatory variables (McCall, 1994; Brinch, 2007). Exploitation of repeated spells for identification purposes is in general a questionable strategy, since it rests on the assumption that, conditional on observed covariates, the unobserved characteristics of individuals do not change between two spells (Røed and Raaum, 2006). This is a justifiably restriction only when the potential causal linkages between spells (in terms of, say, lagged duration dependence) are properly modeled. As follows from the discussion above, we believe this to be the case in the present model. We nevertheless consider the substantial variation in hazard rates caused by exogenous time-varying covariates to be our main source of model identification; see Gaure, Røed and Zhang (2007). The most important time-varying covariates in our model are clearly the calendar time dummies, capturing cyclical fluctuations in labor demand and changes in government priorities regarding the usage of labor market programs and sanctions. Cyclical fluctuations alone are responsible for large exogenous shifts in the job finding rates, with hazards being roughly twice as high during a cyclical peak than during a cyclical trough, ceteris paribus; see Gaure and Røed (2007). The identification of the impacts of endogenous events (ALMP, sanctions, and part-time work) relies on the assumption of “no anticipation” (Abbring and Van den Berg, 2003). This implies that individuals do not anticipate the *realization* of the stochastic process determining these events. They may, however, have full knowledge regarding the determinants of this process and adjust their behavior accordingly. Hence, we do not rule out ex ante effects, e.g., in the sense that job search activity responds to changes in the ALMP or sanction hazard rates.

The causal effects of the various soft and hard UI duration constraints (the exhaustion effects) are identified by the variation in duration dependence patterns across UI regimes. Note, however, that the causal effects of the 1997 UI reform on the *initial level* of the hazard rates are disentangled from the calendar time effects through a proportionality assumption, i.e., it is assumed that the calendar time effects are the same for all spells, regardless of which UI regime they belong to. In particular, we require equal calendar time effects for spells that were affected by the reform (new UI insured spells) and spells that were not (or only moderately) affected by the reform (uninsured spells, spells with extended benefits due to previous state sector employment, and spells that were in progress at the time of the reform). Given that this assumption is questionable, we investigate the robustness of our findings with respect to modifications of it; see Section 4.6 below.

4.3 The likelihood function

In order to set up the likelihood function for this model, we need to transform the integrated hazard rates into period-specific transition probabilities. The probability that individual *i* makes a transition to state *k* during period *t* is equal to:

$$p_k(w_{kit}+v_{ki})=\left(1-\exp\left(-\sum_{k\in K_{it}}\exp\left(w_{kit}+v_{ki}\right)\right)\right)\frac{\exp\left(w_{kit}+v_{ki}\right)}{\sum_{k\in K_{it}}\exp\left(w_{kit}+v_{ki}\right)}, \qquad (3)$$

where K_{it} is the set of feasible transitions for individual *i* in period *t*.[8] Let y_{kit} be an outcome indicator variable, which is equal to 1 if the corresponding observation ended in a transition to state *k*, and zero otherwise, and let Y_i be the complete set of outcome indicators available for individual *i* (all periods at which individual *i* has been at risk of making a transition of some sort). The contribution to the likelihood function formed by a particular individual, conditional on the vector of unobserved variables v_i can then be formulated as:

$$L_i(v_i)=\prod_{y_{kit}\in Y_i}\left[\begin{array}{l}\prod_{k\in K_{it}}\left[\left[\left(1-\exp\left(-\sum_{k\in K_{it}}\exp(w_{kit}+v_{ki})\right)\right)\frac{\exp(w_{kit}+v_{ki})}{\sum_{k\in K_{it}}\exp(w_{kit}+v_{ki})}\right]^{y_{kit}}\right]\\ \times\left[\exp\left(-\sum_{k\in K_{it}}\exp(w_{kit}+v_{ki})\right)\right]^{\left(1-\sum_{k\in K_{it}}y_{kit}\right)}\end{array}\right]. \qquad (4)$$

[8] This can be derived from the continuous time hazards - which given the within-period constancy assumption are equal to the unit-interval integrated hazards - as follows:

$$\int_{t-1}^{t}\exp(w_{kit}+v_{ki})\exp\left(-\sum_{k\in K}\int_{t-1}^{u}\exp(w_{kit}+v_{ki})ds\right)du=\int_{t-1}^{t}\left(\exp(w_{kit}+v_{ki})\exp\left(-\sum_{k\in K}(u-(t-1))\exp(w_{kit}+v_{ki})\right)\right)du$$

$$=\left(1-\exp\left(-\sum_{k\in K}\exp(w_{kit}+v_{ki})\right)\right)\frac{\exp(w_{kit}+v_{ki})}{\sum_{k}\exp(w_{kit}+v_{ki})}.$$

In order to arrive at the marginal likelihood, we need to integrate unobserved heterogeneity v_i out of Equation (4). The way we do this takes into account that there is a left-truncation problem represented in our data (spells starting and ending in the same month are never recorded), implying that individuals are included in the dataset conditional on having a spell that survives to the first observation point. We use Bayes' theorem to derive the appropriate distribution of unobserved heterogeneity. However, since we do not have information about the exact duration an individual has been at risk at the time of sampling, we need an additional assumption regarding the pattern of inflows. We assume here that the entrances to the origin state are uniformly distributed within each calendar month.[9] Let $\bar{t}_i$ be the inflow month for individual *i* and let $w_{i\bar{t}_i} = (w_{1i\bar{t}_i}, w_{2i\bar{t}_i}, w_{3i\bar{t}_i}, w_{4i\bar{t}_i}, w_{5i\bar{t}_i}, w_{6i\bar{t}_i})$. The conditional density of unobserved heterogeneity $f(v_i \mid d \geq 1)$ is then related to the unconditional density $f(v_i)$ by

$$f(v_i \mid d \geq 1) = \frac{\Pr(\sum_k y_{ki\bar{t}_i} = 0 \mid w_{i\bar{t}_i} + v_i)}{\underset{v_i}{E} \Pr(\sum_k y_{ki\bar{t}_i} = 0 \mid w_{i\bar{t}_i} + v_i)} f(v_i), \tag{5}$$

where

$$\begin{aligned} \Pr(\sum_k y_{ki\bar{t}_i} = 0 \mid w_{i\bar{t}_i} + v_i) &= \int_0^1 \exp(-(1-s)) \sum_k \exp\left(w_{ki\bar{t}_i} + v_{ki}\right) ds \\ &= \frac{1 - \exp\left(-\sum_k \exp\left(w_{ki\bar{t}_i} + v_{ki}\right)\right)}{\sum_k \exp\left(w_{ki\bar{t}_i} + v_{ki}\right)}. \end{aligned} \tag{6}$$

We use a non-parametric approach to account for unobserved heterogeneity, to make sure that the results are really driven by the data and not by unjustified restrictions. In practice, this implies that the vectors of unobserved attributes are discretely distributed (Lindsay, 1983) with the number of mass-points chosen by adding points until it is no longer possible to increase the likelihood function (Heckman and Singer, 1984). Let *Q* be the (a priori unknown) number of

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[9] Since we never observe exits in the inflow months, we also have to make an assumption regarding the "duration effect" in this month. We assume here that it is equal to the duration effect in the first observed month, i.e. $\lambda_{kg0} = \lambda_{kg1}$. A similar assumption has to be made regarding the calendar time effect corresponding to the very first inflow month in the dataset (1993.10), which is assumed to be equal to the subsequent month.

support points in this distribution and let $\{v_l, q_l\}$, $l = 1,2,...Q$, be the associated location vectors and probabilities. In terms of observed variables, the likelihood function is then given as

$$L = \prod_{i=1}^{N} \sum_{l=1}^{Q} q_l \frac{\Pr(\sum_k y_{ki\bar{t}_i} = 0 \mid w_{i\bar{t}_i} + v_l)}{\sum_{l=1}^{Q} q_l \left[\Pr(\sum_k y_{ki\bar{t}_i} = 0 \mid w_{i\bar{t}_i} + v_l) \right]} L_i(v_l), \quad \sum_{l=1}^{Q} q_l = 1, \tag{7}$$

where $L_i(v_l)$ is given in (4) and $\Pr(\sum_k y_{ki\bar{t}_i} = 0 \mid w_{i\bar{t}_i} + v_i)$ is given in (6).

4.4 Estimation

Our estimation procedure is to maximize (7) with respect to all the model and heterogeneity parameters repeatedly for alternative values of *Q*. We start out with *Q=1*, and then expand the model with new support points until the likelihood can no longer be improved.[10] The scope for adding additional points is, at all stages of the process evaluated by means of simulated annealing (Goffe, Ferrier, and Rogers, 1994) as well as by full estimation based on randomly selected starting-values for heterogeneity parameters. Our optimization algorithm is thoroughly described and assessed in Gaure, Røed and Zhang (2007). For the present model, we ended up with 41 support points in the heterogeneity distribution. However, after the inclusion of around 15-20 support points, there were only minor changes in the parameters of interest (only the heterogeneity distribution itself changed). The selected model contains 2,877 parameters, out of which 286 characterize the distribution of unobserved heterogeneity (6x41=246 location parameters and 40 probabilities).

4.5 Sensitivity analysis

Before we present the results from this model, we briefly discuss two potential problems with our modeling strategy that have led us to estimate alternative models as part of a sensitivity analysis. The first problem is that of initial conditions regarding lagged unemployment for entrants during the first four years of our data-period. Some of these entrants experienced non-modeled unemployment prior to the start of our observation window in November 1993. According to our model, past unemployment duration has been affected by the same unobserved characteristics as

[10] For practical and computational reasons, we consider this to be the case when the log-likelihood increases by less than 0.01.

current unemployment duration. Consequently, our estimates of the causal effects of past unemployment are in general biased. One way to deal with this problem is to limit the analysis to individuals who started their "unemployment career" in our data-period, i.e., those who did not have any unemployment experiences at all during the four years prior to their first spell in the period from 1993 to 2001. We have estimated such a model. In this exercise we lose 42.1 percent of the individuals and 47.4 percent of the spells in the dataset. As it turns out, most of the results from this estimation are very similar to the results based on the complete dataset. The only difference of substantive interest is related to the effects of "lagged" unemployment, particularly for individuals with very long lagged spells. Given the large loss in the number of observations – which may also entail some selection problems – we stick to the full dataset as our main source for the presentation of results in the next section. However, we briefly return to the model with only firs-time entrants when we discuss the results regarding intrinsic duration dependence.

The other potential problem with our modeling strategy is that the effects of the 1997 UI reform on the level of the hazard rates are identified on the basis of a questionable proportionality assumption regarding calendar time effects; see Section 4.2. In particular, one may worry that non-insured job seekers – who are typically individuals with little previous work-experience – respond differently to cyclical fluctuations in labor demand than insured job seekers. We have examined this question by estimating a version of the model in which calendar time effects are estimated separately for spells with and without UI benefits. The resultant sets of calendar time effects for the three final-destination hazards and for the ALMP hazard are provided in Appendix 2. Our reading of these estimates is that the calendar time effects are indeed very similar for insured and uninsured spells, with some possible exceptions in the beginning of the data-period (for the other-benefit hazard) and at the end (for the employment hazard and the other-benefit hazard) . To assess the impact of these possible violations of the proportionality assumption on estimated reform effects, we have also estimated the model on the subset of UI spells only. For this subset, the foundation for separating reform and time effects is obviously weak, since it rests on a combination of the coexistence of regimes (generated by UI spells that were in progress at the time of reform) and the small number of spells that were unaffected by the 1997-reform. Again, we find that the parameter estimates are largely in accordance with those from the main model. There are, however, some changes in the estimated effects of UI institutions that we return to in the next section.

5 Results

This section presents the key results. We first examine the mechanisms of intrinsic and UI-generated duration dependence. We then turn to the direct impact of active labor market programs (ALMP) and part-time work. Most of the results are illustrated graphically, and, for expository reasons, without confidence intervals (we report standard errors or confidence intervals for the key results, though). Given our extremely large dataset, statistical uncertainty is not a major issue in this analysis. Due to space limitations, we do not present results regarding impacts of individual characteristics, such as gender, age, family situation, education, work-experience, and unobserved heterogeneity. A complete list of estimates, with standard errors, is provided on our web-page http://www.frisch.uio.no/docs/UI_in_welfare_states.html. Here, we also present complete results for the model without unobserved heterogeneity and for the sensitivity analyses.

5.1 Intrinsic duration dependence

Figure 1 presents our estimates regarding intrinsic duration dependence in the three final destination hazards for a job seeker embarking on his/hers first unemployment spell. We use the first potential exit month as reference (equal to unity), implying that the curves show the level of the hazard rates relative to the first month (exp(parameter estimates)). The results in Figure 1 indicate that there is strong monotonically negative duration dependence in the employment and education hazards. In the benefit-shift hazard there is weak negative intrinsic duration dependence during the first half year, after which there is weak positive duration dependence, particularly after two years. Although the latter effects are rather small compared to the duration dependence in the other two hazards, they are highly statistically significant.[11]

[11] The results presented here refer to individuals becoming unemployed during a “normal” state of the business cycle. The model also includes a linear interaction term between spell duration and the cyclical environment at time of entry; see Section 4. It turns out that the better the cyclical environment the stronger is the negative duration dependence in the job hazard. Apart from that, there are only minor cyclical variations in intrinsic duration dependence.

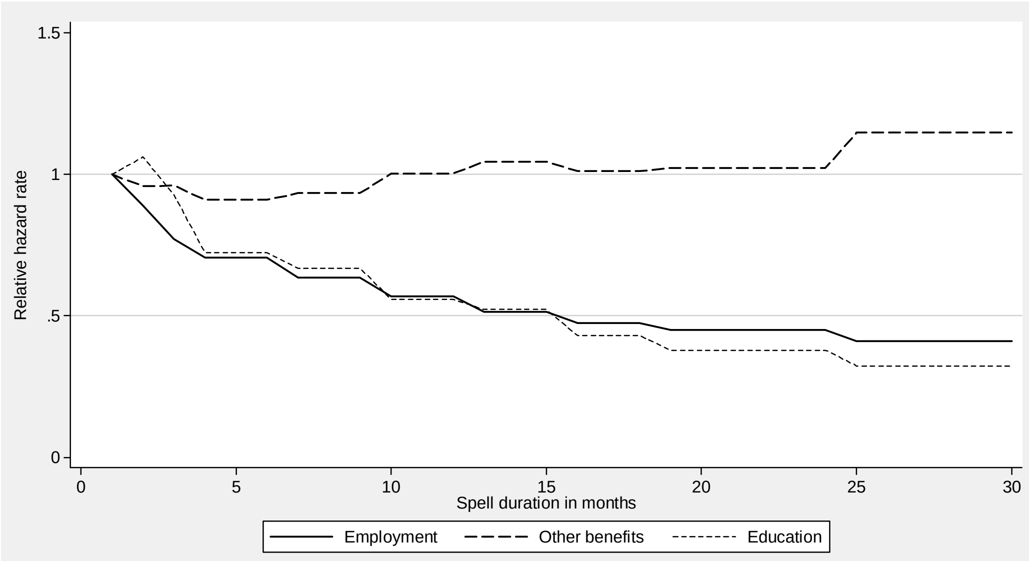

Figure 1. Estimated intrinsic duration dependence for individuals with no previous unemployment (during the four years prior to the spell)
Note: The graph shows relative hazard rates normalized to unity in the first duration month.

Figure 2 presents the estimated intrinsic duration dependence for individuals with previous unemployment experience (during the last four years) that was completed 1-6 months prior to the start of the current spell. Recall that duration is measured as elapsed time of the *current* spell, so that past unemployment may change the hazard level to start with as well as its duration profile. The reference point is still the first potential exit month for individuals without any previous unemployment; i.e., the first month in Figure 1. Hence, if a curve starts at a level different from unity, it indicates that the past unemployment experience has a causal effect on the starting level of the hazard rate in the current spell. The key messages coming out of Figure 2 is that there is a substantial causal effect of past unemployment spells on current hazard rates. In particular, the employment hazard starts out at a lower level, and the benefit-shift hazard starts out at a higher level, the more a job seeker has been exposed to unemployment in the past. Similar duration profiles were estimated for individuals with past unemployment experience concluded more than 6 months prior to the current spell; see Table A1 in Appendix 1. It turned out, however, that the time since completion of the last spell has little impact on current hazard rates (not shown).

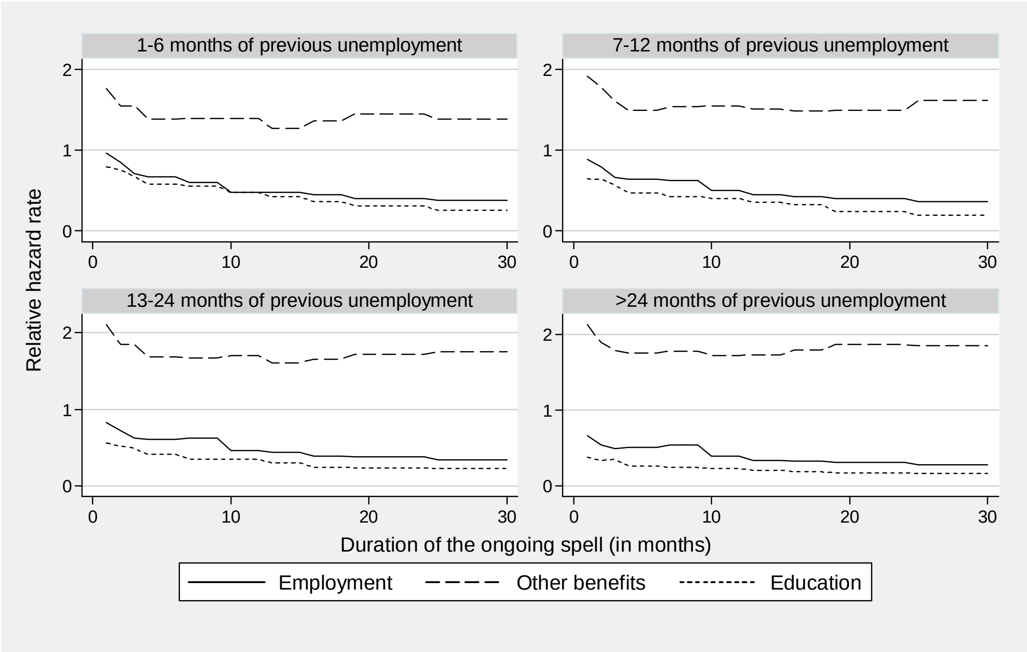

Figure 2. Intrinsic duration dependence in current spell for individuals with previous unemployment experience completed 1-6 months prior to the current spell. By total lagged unemployment duration (1-6, 7-12, 13-24, >24 months).
Note: The panels correspond to the groups defined in Appendix 1, Table A1. The figures show estimated hazard rates relative to the first month for an individual with no previous unemployment; see Figure 1.

As explained in Section 4.5, we have also estimated our model for the subset of individuals with no unemployment exposure during the four-year period prior to their first spell in our data-window (to circumvent the initial conditions problem related to unemployment experienced prior to our observation window). The results from this model are roughly in line with the results presented above. For job seeker with a lot of past unemployment, however, we find that the estimated negative causal impact of lagged unemployment becomes somewhat smaller (typically a few percentage points) in the reduced model. Complete results are available at http://www.frisch.uio.no/docs/UI_in_welfare_states.html.

5.2 The impact of UI institutions

We now turn our attention to the impact of UI institutions. In order to make the results directly comparable to those of intrinsic duration dependence, we present the estimated effects of the UI entitlement dummy variables as functions of spell duration for entrants starting with complete UI periods in the four main UI regimes; see Figure 3. Note that the hazards are constrained to be piece-wise constant during most of the UI exploitation period, but that a completely flexible baseline takes over (with separate dummies for each month) in the six-month periods just prior to exhaustion; conf. Table A2, Appendix 1. The effect of actually having exhausted UI benefits is assumed constant and equal across regimes, unless wait-period benefits are substituted for UI benefits (Regime IV). The period just after the application process in Regimes III and IV (corresponding to months 4-18 in the two lower panels of Figure 3) is used as a reference, so that the graphs indicate hazard rates relative to the hazard rates during this phase of the UI period in the new UI regimes. The points of temporary and final UI exhaustion are marked in Figure 3 as vertical short-dashed and long-dashed lines, respectively.

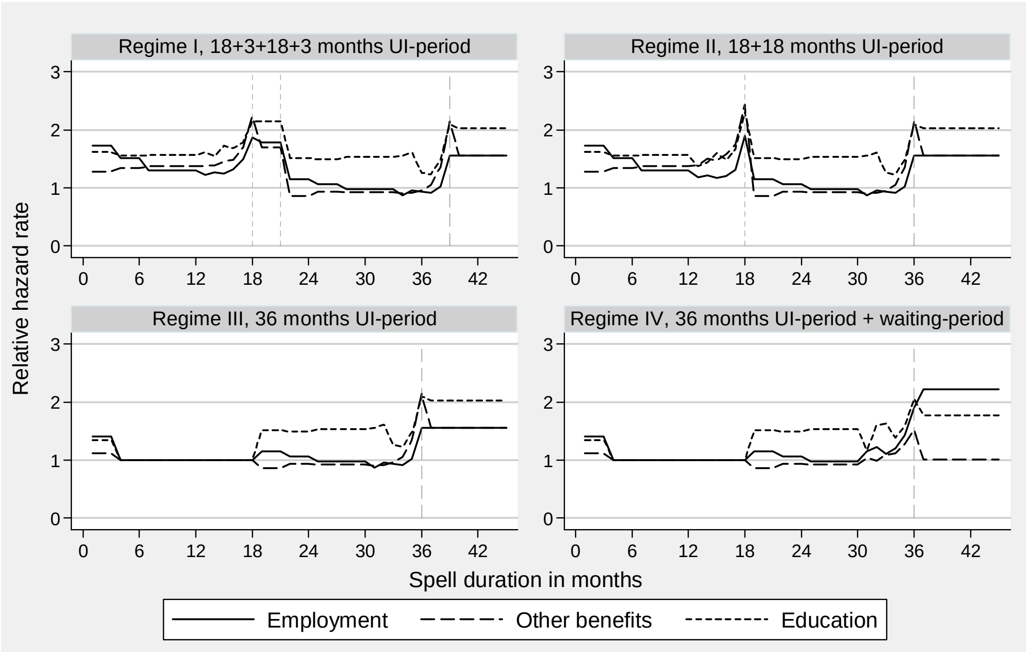

Figure 3. Estimated UI-generated duration dependence for Regimes I, II, III, and IV
Note: Hazard rates are displayed relative to those applying through months 4-18 in Regimes III and IV. Short-dashed and long-dashed vertical lines indicate the possibility of temporary and final UI exhaustion, respectively.

A key message coming out of the results presented in Figure 3 is that the rises in hazard rates occurring in the run-up to UI exhaustion are very similar across the various types of constraints. In particular, the employment hazard rises with approximately 50 percent during the 1-3 months just prior to exhaustion, regardless of the harshness of the exhaustion constraint. The education and benefit-shift hazards rise even more (except in Regime IV). The education hazard also responds earlier than the other hazards towards the prospect of UI exhaustion. The reason for this is probably that educational activities typically start at particular times during a year; hence they cannot be "timed" individually to the same extent as other transitions.

We find it intriguing that the behavioral responses towards the different types of constraints represented in Figure 3 are so similar. Recall that there are four quite different types of constraints for which we estimate completely separate exhaustion effects. The mildest constraint is the one occurring at 18 months in Regime II, after which the claimant is entitled to an additional 18 month UI period, although with a 10 percent reduction in the benefit level. The second mildest constraint is the one occurring at 36 months in Regime IV, after which the claimant is entitled to indefinite wait-period benefits, but at a substantially reduced level. A somewhat harder constraint is the one occurring at 18 months in Regime I, after which UI benefits are potentially terminated for a three month period before a new 18 month period is admitted (with a 10 percent reduction in the benefit level). The hardest constraints are the ones occurring at 39 months in Regime I and at 36 months in Regimes II and II, after which there are no more benefits to be had from the PES. After exhaustion, hazard rate movements clearly depend on the nature of the constraint actually imposed. In cases where a new UI period is granted (Regimes I and II), the employment hazard declines to a level slightly below the level that prevailed prior to the exhaustion-generated increase in the first UI period. After final UI exhaustion (Regimes I, II, and III), all hazard rates settle at levels substantially above the reference level. A somewhat surprising result is that the employment hazard increases even more after UI exhaustion for job seekers entitled to wait-period benefits (Regime IV).[12]

An additional point to note from Figure 3 is that employment hazards display a slightly U-shaped pattern with respect to the exploitation of UI benefits. The hazard tends to be relatively high during and just after the UI application process. It then drops to a somewhat lower level at which it remains for quite a while, before it again starts to increase in the run-up to exhaustion. The higher employment hazard during and just after the application process is likely to result

[12] This may reflect that these individuals have been under particularly hard pressure to participate in ALMP. We suspect, however, that the finding also stems from non-random regime assignment. Among job-seekers with entitlement to wait-period benefits, there are simply more individuals that are really able to find employment when they come under pressure to do so than there are among job-seekers without this entitlement. But this is not revealed until the pressure actually sets in. Consequently, it may not be fully captured by our regime eligibility control variables (which only controls for constant differences in hazard rate levels).

from the eligibility review and the intensive counseling process often accompanying the handling of a UI application.

A final result coming out of Figure 3 is that the all hazard rates are generally higher in Regimes I and II than in Regimes III and IV, even at the start of UI periods. Recall that we have included a full set of calendar time dummy variables in the model, so these impacts are essentially identified through the presence of job-seekers who were unaffected by the UI reform. A possible explanation for the higher final-destination hazards for UI claimants prior to the UI reform is provided by Figure 4, where we show how the main regimes discussed above affected the hazard rates to ALMPs and sanctions. It is evident that Regimes I and II (with a soft constraint after 80 weeks) were characterized by particularly high sanction and ALMP probabilities in the run-up to temporary benefit exhaustion and in the quarantine period. It is also evident from the descriptive statistics provided in Table 3, as well as by the estimated calendar time effects (not shown) that both sanctions and ALMPs were used much more actively before the 1997-reform than afterwards.[13] Hence, we may characterize Regime I and II as being "activity oriented", while Regimes III and IV are more "income-insurance oriented". And even though the causal impacts of *actual* ALMP participation and *actual* sanctions are captured by separate parameter sets (to which we return below), and hence not embedded in the regime profiles described in Figure 3, we may expect that the higher likelihood of being sanctioned or enrolled into ALMP in the activity oriented pre-1997 UI regimes contributed to increase the hazard rates out of unemployment for job-seekers belonging to these regimes; see Black *et al.* (2003).

[13]The unconditional ALMP participation probability was more than twice as high in Regime I as it was in Regimes III and IV. The sanction probability was more than 50 percent higher in Regime I.

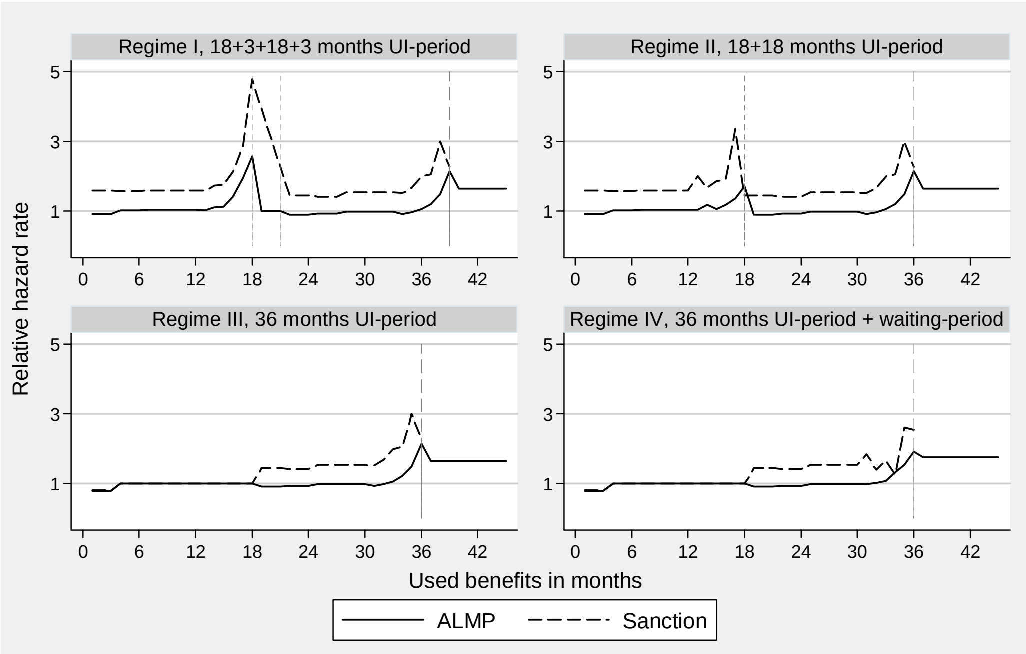

Figure 4. Estimated UI-generated duration dependence in ALMP and sanction hazards for regimes I, II, III, and IV.

In order to take a closer look at the overall impacts of the 1997 UI-reform, we perform a simulation exercise; i.e., we use the estimated model to simulate the progression of insured unemployment spells that actually started after the reform (Regimes III and IV) under two alternative assumptions regarding the UI system: i) the correct assumption that the spells belonged to Regimes III and IV, and ii) the counterfactual assumption that they belonged to Regime I. By comparing the two simulation results, we can then assess the reform's impact on mean unemployment duration as well as on the distribution of final outcomes (destinations). In order to obtain confidence intervals for our simulation results, we use a parametric bootstrap procedure, i.e., we draw parameter estimates repeatedly from their joint normal distribution.[14] In total, we make 100 simulations for the correct and counterfactual assumptions, respectively, and calculate 98 percent confidence intervals for the statistics of interest. Some key results are presented in Table

[14] Note that we make drawings from the vector of 2,591 parameters attached to observed covariates only, since the parameters describing the unobserved heterogeneity are not normally distributed; see Gaure, Røed and Zhang (2007). We thus condition on the drawings of unobserved heterogeneity. The drawings of parameter estimates are made by means of the Cholesky decomposition; *i.e.*, let L be a lower triangular matrix, such that the covariance matrix is $V = LL'$. Let z_s be a vector of 2,591 drawings from the standard normal distribution collected for trial s. Let $\hat{b}$ be the vector of point-estimates. The parameters drawn for trial s are then given as $b_s = \hat{b} + Lz_s$.

4. The reform caused a significant increase in the duration of unemployment spells, from an average of 5.86 to 7.43 months, i.e., by 26.8 percent. Interpreted as a change in the maximum duration period from 80 to 156 weeks, this implies that for every week of maximum UI duration extension, the expected length of an unemployment spell increases by half a day. Interestingly, this effect is of exactly the same magnitude as that found by Card and Levine (2000) on the basis of an extended benefit program in New Jersey (USA). This similarity is somewhat surprising, given that the Norwegian reform did not raise the absolute maximum UI period at all; it apparently only removed a requirement to apply for a second period halfway through it and postponed activity requirements. Nevertheless, the regime change increased the expected duration to all final-destination states. The reform had only minor impacts on the distribution of final outcomes. It caused a small increase in the number of transitions to other benefits, and correspondingly minor reductions in the number of transitions to employment and education.

Table 4
The predicted impacts of the 1997 reform

	All		Final outcome					
			Employment		Other benefit		Education	
	Regime I	Regime III/IV	Regime I	Regime III/IV	Regime I	Regime III/IV	Regime I	Regime III/IV
Mean unemployment duration	5.86	7.43	5.41	7.02	8.72	10.25	5.26	6.52
Duration difference (Regime III/IV – I)	1.57 [1.47, 1.69]		1.61 [1.49, 1.76]		1.53 [1.21, 1.78]		1.25 [1.06, 1.43]	
Percent of spells ending in			73.94	73.65	14.08	14.56	11.02	10.59
Percentage point difference (Regime III/IV – I)			-0.25 [-0.65, 0.14]		0.49 [0.13, 0.88]		-0.46 [-0.79, -0.20]	

Note: Number in brackets [ ] are 98 percent confidence interval.

These simulations do not take into account that the pre-reform system (Regime I) also entailed a higher level of ALMP participation in general, as reflected in the calendar time parameters σ_{3t}. As we show in Section 5.4, actual ALMP participation tends to increase unemployment duration somewhat. We therefore repeated the counterfactual simulations discussed above with the post reform calendar time participation effects also adjusted to the pre reform level (implying a general rise in the ALMP participation hazards around 40 percent). But this only caused a 0.1 month increase in average unemployment duration. Hence, while the degree of actual ALMP participation has little effect on average spell duration (consistent with the evidence presented in

Section 5.4 below), their role as a “looming” availability-for-work-test in relation to UI eligibility seems to be of paramount importance.

As discussed in Section 4.2, the identification of reform effects hinges on the assumption that calendar time effects are the same for all job seekers, regardless of which UI-regime they belong to; see Section 4. We have examined the robustness of our findings with respect to this assumption by estimating the model on the subset of UI insured spells only. In this case, the identification of the reform effect is primarily driven by spells that were in progress at the time of the reform, in addition to the existence of previous state-employees, who were unaffected by the reform. For most parameter estimates, the exclusion of spells without UI insurance causes only small changes. However, the estimated adverse effects of the 1997 UI reform become somewhat smaller. Based on simulation exercises on this alternative model/dataset, we find that the impact of the 1997-reform on average unemployment duration was 1.27 months, rather than the 1.57 months reported in Table 7. We have also made the same type of simulations based on the model with only individuals with no unemployment exposure during the four-year period prior to their first spell included; see Section 5.1. Based on these simulations, the impact of the 1997-reform on average unemployment duration was equal to 1.41 months. Hence, all our models and simulations predict an overall impact around 5-6 weeks.

A particularly interesting regime that we have not commented on so far is Regime VII, with special (generous) treatment of ex state-employees who lost their jobs due to organizational changes. Some of these individuals were subject to a virtually unlimited maximum UI duration (up to 18 years). As expected, these individuals have extremely low employment hazard rates. For persons with a 3-6 year entitlement period, the employment hazard is 43 percent lower than for the reference, and for persons with more than a 6 year entitlement period, it is 70 percent below the reference level. Although we cannot rule out compositional differences between these and other job seekers (remember that the unobserved covariates in our model are designed to capture heterogeneity which is orthogonal to observed characteristics at the moment of inflow), we have no reason to believe that previous state-employees represent a particularly hard-to-employ group. Unlimited UI entitlement clearly has a damaging effect on job search behavior.

A more surprising result, perhaps, is that individuals with no benefit entitlement at all (Regime VIII) have employment hazard rates approximately equal to the reference level. They have, however, much higher hazard rates into other benefits (twice the reference level) and to education (80 percent higher than the reference level). Again, there may be unobserved differences between UI claimants and non-claimants that render the causal interpretation of these differences questionable.

5.3 Discretionary sanctions

As we showed in Section 3, around two percent of the UI spells were subject to a discretionary sanction, i.e., a temporary loss of UI benefits due to inappropriate search behavior or unwillingness to accept (suitable) jobs or program offers. The normal duration of a sanction is 8 weeks. Our estimates indicate that a sanction causes an immediate rise in the job hazard by 80 percent, a rise in the ALMP hazard by 22 percent, and a rise in the education hazard as large as 200 percent. The estimated rise in the job hazard is of similar magnitude as that found by Abbring *et al.* (2005), based on the much milder sanctions regime in the Netherlands (the average sanction in the Netherlands amounts to a reduction in the replacement rate of around 20 percentage points for a period of three months). This may indicate that the toughness of the sanction may be of secondary importance, at least within some range. The large effect on the education hazard probably reflects that some individuals collect UI benefits while they wait for a planned education to start, hence they may not really be interested in a job just yet. Sanctions also raise the hazard to part-time work by 42 percent. Only the benefit-shift hazard falls during a sanction, by 34 percent. Most of the effects are short-lived, however. After the sanction is completed (and the job seeker again receives benefits) only the hazards to ALMP and to a new sanction remain at a higher level than before the sanction took place.

5.4 The effects of program participation and part-time work

Figure 5 displays how program participation and access to part-time work causally affect the three final-destination hazards. Participation in ALMP reduces the employment and education hazards significantly during the first phase of the participation period. As the participation period progresses the negative effects become smaller, and they turn positive after approximately six months. Unsurprisingly, participation in ALMP significantly reduces the probability of taking up another type of benefit. After completion of ALMP, there is a significant rise in the employment hazard compared to the situation prior to ALMP participation. The favorable effect is larger the longer the duration of the completed program.

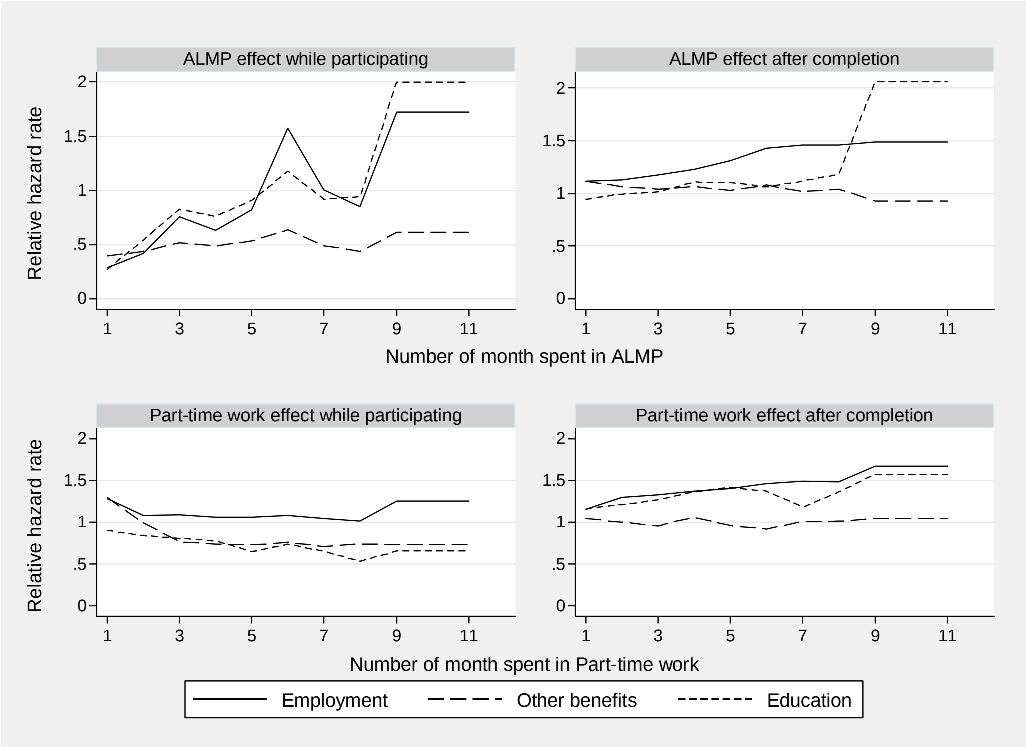

Figure 5. Estimated effects on final-destination hazard rates of ALMP-participation and part-time employment.
Note: The effects on the hazard rates are measured relative to no participation. The panels to the left show the effects of ongoing participation/part-time work as functions of ongoing duration. The panels to the right show the effects of completed participation/part-time work as functions of completed duration.

In order to evaluate the overall impact of ALMP participation on unemployment duration and on the distribution of final outcomes, we perform a new simulation exercise. This time we compare outcomes and durations based on our estimated model to the outcomes and durations generated when all treatment effect parameters are set equal to zero (but the other parameters are generated from our model).[15] The 100x2 simulations are performed on the basis of drawings from the joint normal distribution of parameter estimates. It is assumed that all the spells started during 1997. The results are provided in Table 5. The overall impact of the program effects for individuals who became unemployed in 1997 was to lengthen the average duration of unemployment (including the participation period) by approximately 1.2 months per treatment. Since the number of treatments per spell in our simulations was around 0.26, the total impact of the program effects was to increase mean duration by around 0.3 months. The fraction of treated

[15] Note that we do not compare a world with programs with a world without programs, since we do not remove the impacts that the existence of ALMP may have on individuals who never actually participate.

spells ending with employment was raised by around 2 percentage points as a result of the program effects. Hence, ALMP participation seems to imply longer unemployment durations, but slightly more favorable final outcomes.

Table 5
The predicted impacts of ALMP effects

	All		Final outcome					
			Employment		Other benefit		Education	
	With effect	Without effect	With effect	Without effect	With effect	Without effect	With effect	Without effect
Mean unemployment duration	5.98	5.64	5.81	5.42	7.17	7.02	5.98	5.64
Duration difference per treatment (With-without)*	1.27 [1.04, 1.50]		1.50 [1.19, 1.77]		0.62 [0.12, 1.15]		1.85 [1.38, 2.31]	
Percent of spells ending in			57.05	56.45	24.45	25.26	17.67	17.37
Percentage point difference per treatment (With-without)*			2.04 [0.46, 3.35]		-3.15 [-4.23, -1.92]		1.12 [-0.04, 2.08]	

Note: Number in brackets [ ] are 98 percent confidence interval.
*Differences per treatment is obtained by dividing the differences between the two regimes with the number of treatments per spell (0.26).

Access to part-time employment raises the employment hazard somewhat initially; see the lower panels in Figure 5. The effect fades out relatively quickly, however, until the part-time employment relationship has ended. There is a favorable employment-experience effect involved, in that the employment hazard rises afterwards. Part-time employment has only minor effects on the other final-destination hazards.

6 Concluding Remarks

The main findings of this paper can be summarized as follows: First, activity-oriented UI regimes - with high “risk” of program participation and/or a UI sanction, and a relatively short initial maximum UI benefit period - reduces unemployment duration substantially with only minor effects on the distribution of final destinations. Second, the harshness of duration-constraints and sanctions is of minor importance; the behavioral impact seems to be almost the same regardless of whether the threat is to terminate the benefit completely or only to reduce it slightly (or to terminate it for only a short period of time). Third, although limitations in (unconditional) UI duration clearly has a substantial impact on job search behavior throughout the unemployment spell, the direct response towards benefit exhaustion is myopic; there are few signs of hazard increases until just before UI exhaustion. Fourth, unemployment duration from the current as well as past spells has a negative causal impact on the employment hazard and a positive causal impact on the benefit-shift hazard.

There seems to be plenty of scope for welfare states to design UI insurance systems so that moral hazard problems are counteracted by “activity requirements” rather than by threats of complete benefit termination. However, there is a cost involved in terms of implementing ALMPs and in terms of summoning and counseling the job searchers more frequently. Our results indicate that actual participation in ALMP leads to an increase in overall unemployment duration, but also to slightly more favorable final outcomes. A full analysis of costs and benefits of ALMPs is beyond the scope of this paper, but such an analysis would have to include the value of the human capital investment being made and the value of work being done during program participation, as well as the costs of arranging the programs; see Røed and Raaum (2006).

A potentially undesirable side-effect of a more activity-oriented UI system is that it increases the rate of benefit substitution, i.e., it raises the hazard to other types of benefits (rehabilitation, disability, social assistance). However, since it also raises the hazard rates to employment and ordinary education, it does not seem to increase significantly the overall probability that a spell ends with a transition to another benefit. More (but mild) pressures on UI claimants make “everything” happen faster without altering the final outcome (destination) of the search process.

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Appendix 1

Table A1
Definition of Spell Duration Dummy Variables (d_{it})

I. Past unemployment Definition of groups according to past unemployment during the 48 months just prior to the start of the current spell (fraction of spells belonging to each group in parentheses)						II. Ongoing unemployment Group-specific baseline hazards divided into the following piece-wise constant parts (duration in months)
Time since last unemployment spell was completed (# months)	Total unemployment during past 4 years (# months)					
	No previous unemployment	1-6	7-12	13-24	>24	
0	Group 1 *(34.25)*	-	-	-	-	1 2
1-6	-	Group 2 *(8.52)*	Group 3 *(5.69)*	Group 4 *(7.72)*	Group 5 *(5.33)*	3 4-6
7-12	-	Group 6 *(5.68)*	Group 7 *(3.59)*	Group 8 *(4.66)*	Group 9 *(2.18)*	7-9 10-12
13-24	-	Group 10 *(5.68)*	Group 11 *(3.13)*	Group 12 *(3.26)*	Group 13 *(0.80)*	13-15 16-18
>24	-	Group 14 *(6.25)*	Group 15 *(2.26)*	Group 16 *(1.00)*	-	19-24 >24

Note: The spells duration dummy variables represent interaction terms of the 16 groups in part I and the 10 baseline steps in part II; hence, in total, we use 16×10=160 dummy variables.

Table A2
Definition of UI Entitlement Dummy Variables (b_{it})

	Variable description	Relevant for Regimes, Conf. Table 1
1	16-18 months left until potential temporary benefit exhaustion (with or without quarantine), or in the application process (given eligibility)	I, II
2	13-15 months left until potential temporary benefit exhaustion (with or without quarantine)	I, II
3	7-12 months left until potential temporary benefit exhaustion (with or without quarantine)	I, II
4	6 months left until potential temporary benefit exhaustion (with quarantine)	I
5	5 months left until potential temporary benefit exhaustion (with quarantine)	I
6	4 months left until potential temporary benefit exhaustion (with quarantine)	I
7	3 months left until potential temporary benefit exhaustion (with quarantine)	I
8	2 months left until potential temporary benefit exhaustion (with quarantine)	I
9	1 months left until potential temporary benefit exhaustion (with quarantine)	I
10	6 months left until potential temporary benefit exhaustion (without quarantine)	II
11	5 months left until potential temporary benefit exhaustion (without quarantine)	II
12	4 months left until potential temporary benefit exhaustion (without quarantine)	II
13	3 months left until potential temporary benefit exhaustion (without quarantine)	II
14	2 months left until potential temporary benefit exhaustion (without quarantine)	II
15	1 months left until potential temporary benefit exhaustion (without quarantine)	II
16	13 week quarantine imposed (without UI benefits) after 80 weeks	I
17	Exempted from 13 week quarantine after 80 weeks	I
18	13 week benefit extension granted after the second 80 week period	I
19	>60 months left until final benefit exhaustion or entitled to benefits until retirement age (previous state employees)	VII

20	37-60 months left until final benefit exhaustion (previous state employees)	VII
21	34-36 months left until final benefit exhaustion (with or without entitlement to wait-period benefits) or in the application process (given eligibility)	III, IV, VII
22	19-33 months left until final benefit exhaustion (with or without entitlement to wait-period benefits)	III, IV, VII
23	16-18 months left until final benefit exhaustion (with or without entitlement to wait-period benefits) for those starting on a new period	V
24	16-18 months left until final benefit exhaustion (with or without entitlement to wait-period benefits) for the rest	I, III, IV
25	13-15 months left until final benefit exhaustion (with or without entitlement to wait-period benefits)	I, III, IV, V
26	7-12 months left until final benefit exhaustion (with or without entitlement to wait-period benefits)	I, III, IV, V
27	6 months left until final benefit exhaustion (with entitlement to wait-period benefits)	IV
28	5 months left until final benefit exhaustion (with entitlement to wait-period benefits)	IV
29	4 months left until final benefit exhaustion (with entitlement to wait-period benefits)	IV
30	3 months left until final benefit exhaustion (with entitlement to wait-period benefits)	IV
31	2 months left until final benefit exhaustion (with entitlement to wait-period benefits)	IV
32	1 months left until final benefit exhaustion (with entitlement to wait-period benefits)	IV
33	6 months left until final benefit exhaustion (without entitlement to wait-period benefits)	I, II, III, V, VI
34	5 months left until final benefit exhaustion (without entitlement to wait-period benefits)	I, II, III, V, VI
35	4 months left until final benefit exhaustion (without entitlement to wait-period benefits)	I, II, III, V, VI
36	3 months left until final benefit exhaustion (without entitlement to wait-period benefits)	I, II, III, V, VI
37	2 months left until final benefit exhaustion (without entitlement to wait-period benefits)	I, II, III, V, VI
38	1 months left until final benefit exhaustion (without entitlement to wait-period benefits)	I, II, III, V, VI
39	Receiving wait-period benefits (after exhaustion of UI benefits)	IV
40	Has exhausted UI benefits, does not receive wait-period benefits	I, II, III, V, VI
41	Not entitled to benefits at all	VIII

Table A3

Definition of variables describing ongoing and completed events (z_{it})

Event	Variable description
Program participation	
Ongoing	10 dummy variables accounting for duration of *ongoing* program participation: 0 (no ongoing ALMP), 1 month, 2 months,…, 8 months, >8 months.
Completed	10 dummy variables accounting for duration of *completed* program participation (within the same spell): 0 (no completed ALMP), 1 month, 2 months,…, 8 months, >8 months. Completed program participation is set to zero upon start of a new ALMP participation.
Part time work	
Ongoing	10 dummy variables accounting for duration of *ongoing* part-time work: 0 (no ongoing part-time work), 1 month, 2 months,…, 8 months, >8 months.
Completed	10 dummy variables accounting for duration of *completed* part-time work (within the same spell): 0 (no completed part-time work), 1 month, 2 months,…, 8 months, >8 months. Completed part-time work is set to zero upon start of a new part-time job.
Sanctions	
Ongoing	One dummy indicating ongoing UI sanction
Completed	One dummy indicating completed sanction

Appendix 2

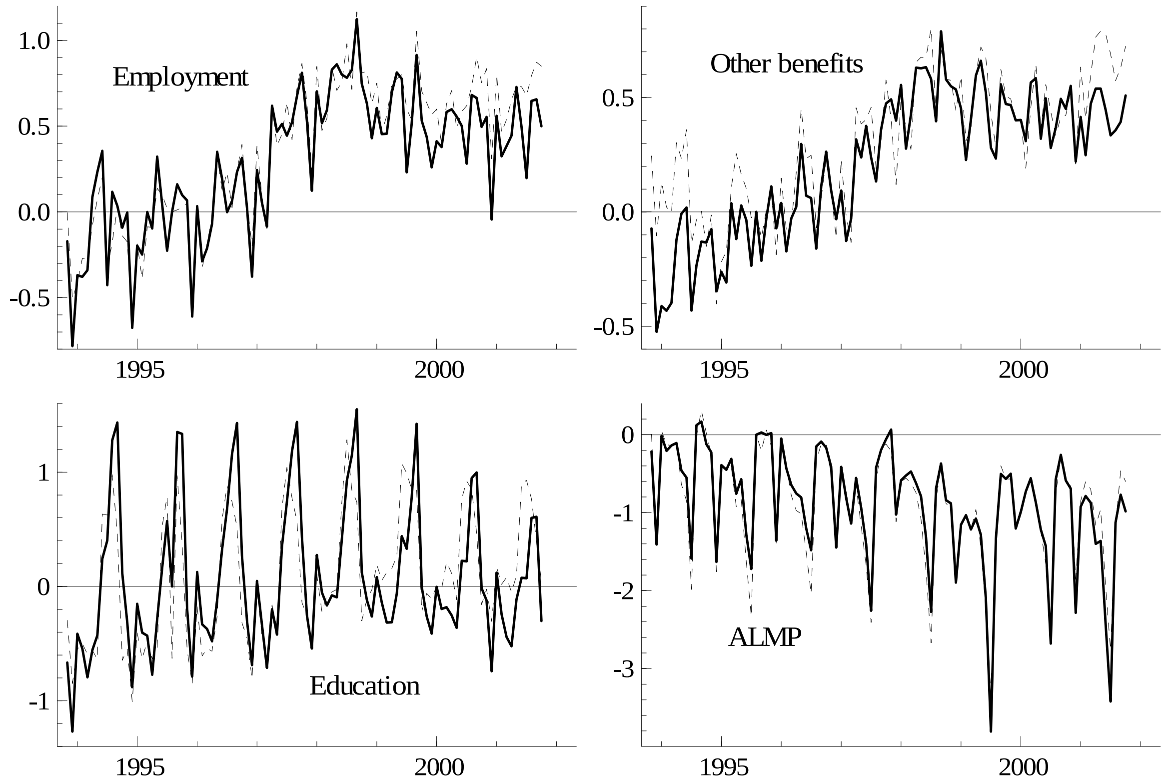

Figure A1 Estimated calendar time effects for insured (solid lines) and non-insured (dotted lines) spells.

Note: The estimates are based on the same model as that described in Section 4, except that calendar time effects are estimated separately for insured spells (Regimes I-VII) and non-insured spells (Regime VIII). To save computational resources, the estimation was terminated after 12 support points were included in the heterogeneity distribution, as there were no indication of further changes in the estimated calendar time effects.