Unemployment Insurance in Welfare States: The Impacts of Soft Duration Constraints and Activation Policies

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Abstract

Based on a major reform of Norwegian unemployment insurance (UI), we examine the behavioral impacts of soft and hard UI duration constraints. A constraint is interpreted as hard if a claimant cannot rely on any form of income support after exhaustion, and as soft if UI is replaced by follow-on benefits or paid activation. We find that both hard and soft constraints raise the job hazard significantly, with surprisingly similar spikes at UI exhaustion. Participation in activation programs raises the probability of eventually finding a job, but at the cost of lengthening the overall job-search period.

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 to the most generous and unconditional form of income support only. 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 various UI duration constraints and sanction practices within a well-developed welfare state economy, i.e., Norway. In order to identify and estimate the causal effects of interest, we exploit a major reform in the UI maximum duration regulations that was implemented in 1997. Our analysis contains two important novelties in relation to the existing literature. First, rather than examining how job finding rates respond to the exhaustion of UI entitlements as such, we investigate how the magnitude of the behavioral responses depend on the “harshness” of the UI constraint; i.e., on what kind of support (if any) the claimants are offered after exhaustion. Although existing empirical contributions are accumulated from UI systems that differ a lot in the prevalence of post-UI-support, we are not aware of any attempts to systematically examine the implications of these differences for the behavioral responses towards UI exhaustion. The second novelty is that we examine the prevalence of unintended side-effects of duration constraints and sanctions, particularly in the form of benefit shifting. This topic has so far been virtually ignored in the literature, despite its potential empirical importance.
(as much as 25 percent of the completed unemployment spells in our data end in a transition to another type of benefit). Our empirical basis is Norwegian 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 education.

An important methodological issue that arises in analyses of UI 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. There is, however, an important conceptual difference between the role of unemployment duration with respect to, e.g., discouragement and scarring, on the one hand, and the role of unemployment duration with respect to exhaustion of UI 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 frequently into and out of unemployment. 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 a single spell in terms of discouragement or scarring 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 distort the estimated impacts of institutional reforms and UI exhaustion. The reasons for this are, first, that the unmeasured parts of unemployment durations (from previous spells) are in general not equally distributed between pre and post reform periods; and, second, that (remaining) UI entitlements may serve as an unintended proxy for unmeasured past unemployment.

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 UI 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 seeker), 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 nonparametric 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.

Since the model allows for various treatments as well as for multiple final outcomes of job search, a proper effect assessment cannot be limited to examination of isolated shifts in hazard rates. For example, it is possible that a particular policy reduces the hazard rate into employment while at the same time raises the probability that the spell ends with a transition to employment. Hence, to examine the impacts of UI regulations, we use the estimated model to simulate the spells’ durations and outcomes under alternative assumptions regarding UI institutions. Similar simulation exercises are used to evaluate the impacts of participation in ALMP, facilitating an assessment of how heterogeneous lock-in and post-treatment effects translate into effects on the probability distribution of final outcomes and on the average duration of unemployment. Statistical inference is based on a parametric bootstrap procedure.

A main finding of our paper is that an “activity-oriented” UI regime – with a high likelihood of activation, strictly duration-limited entitlements of non-activity-conditioned UI, 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

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.
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 social programs. 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, and reduces the probability that it ends in a transition to another social program by three percentage points.

The next section briefly reviews the existing evidence. Section 3 provides a description of data, institutions, and UI reform, and defines the state space. Section 4 presents the statistical model, Section 5 discusses the results, and Section 6 examines robustness. Section 7 concludes.

2 Relation to previous research

The existing empirical literature has established that the threat of losing UI benefit entitlements within the near future has a significant impact on the claimants’ exit rates from unemployment. This has been shown for the U.S. (Katz and Meyer, 1990; Meyer, 1990; Card and Levine, 2000), for Central Europe (Hunt, 1995; Winter-Ebmer, 1998; Lalove, Van Ours, and Zweimüller, 2006), as well as for Scandinavia (Geerdsen, 2006; Carling et al., 1996; Carling, Holmlund and Vejsiu, 2001; Røed, Jensen, and Thursie, 2008). Somewhat surprisingly, it is not an unequivocal pattern that the behavioral responses are strongest in the countries where the individual economic consequences of UI exhaustion are largest. Hunt (1995), for exam-
ple, concludes that the behavioral impact of UI exhaustion in Germany is of the same magnitude as in the U.S., despite the existence of follow-on benefits in Germany. Unfortunately, evidence collected from different countries tends to be based on different sources of identification. While U.S. evidence relies heavily on variation in UI regulations across states, European evidence tend to be based on reforms targeted at particular demographic groups. In the German case, for example, the behavioral impacts were identified on the basis of a benefit duration extension for elderly workers. Hence, the empirical results may to some extent have been driven by the (fully intended) usage of the UI system as an informal route towards early retirement during economic slumps. Indeed, Fitzenberger and Wilke (2009) show that conditional on the job seekers’ eventual return to employment the reforms in Germany had no effect at all on the speed at which this happened.

In the Scandinavian countries, UI exhaustion is typically followed either by paid activation or by various forms of follow-on benefits. Evidence from Denmark (Geerdsen, 2006) and Sweden (Carling et al., 1996; Carling, Holmlund and Vejsiu, 2001, Røed, Jensen, and Thoursie, 2008) indicates that UI exhaustion followed by paid activation causes a large spike in the job hazard just before the activation requirement is enforced. 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 was divided into two distinct parts, separated by a quarantine (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) indicates 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.

As we show in the present paper, a substantial fraction of unemployment spells is terminated without a job actually being found. In a recent overview of the literature, Card, Chetty and Weber (2007) show that the reported spikes in exit 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. This suggests that the large spikes typically encountered at benefit exhaustion to some extent reflect exits from the labor force rather than
entries into employment, e.g., in the form of transitions to regular education or to alternative social programs. However, most of the existing empirical evidence either builds on statistical models where all transitions from unemployment are aggregated into a single “exit” state or models where non-employment transitions are collected into a single “out-of-labor-force state” or right-censored. Consequently, there is little evidence regarding the impact of UI exhaustion on benefit shifting and transitions to education. For Norway (Røed and Zhang, 2005; Henningsen, 2008) and Sweden (Larsson, 2006), there is some evidence that the take-up rate of sickness insurance benefits increases as the point of UI exhaustion comes closer. But the quantitative importance of this and other potential side-effects of UI exhaustion remains unexplored.

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 proposed by Abbring and Van den Berg (2003) to examine the impact of sanctions on 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. Based on a similar approach, Svarer (2007) finds that sanctions significantly raise the exit rate from unemployment in Denmark. To our knowledge, no evidence exists for Norway.

In summary, our reading of the existing literature is that although UI exhaustion has been shown to cause spikes in the exit rates from unemployment under quite different economic circumstances, it is unclear how the magnitude of the estimated behavioral responses relates to the magnitude and type of “punishment” actually imposed. To our knowledge, no previous studies have aimed at comparing directly the behavioral responses generated by different types of duration constraints distinguished by their “harshness” (the existence of follow-on benefits) and associated activity requirements. It is also unclear how spikes in the benefit shifting propensity and in the transition rate to education might have affected existing empirical findings as well as their interpretation. We are also not aware of previous studies that examine and evaluate the UI exhaustion spikes for transitions to all frequently occurring destination states simultaneously.
3 Data, institutions, 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 the claimants’ characteristics as well as on time of entry into unemployment, 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 13,000 Euros per year (2008). 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 52,000 Euros. These basic rules have been stable throughout the period (in real terms), apart from some minor increases in the minimum income requirements. However, the rules regulating maximum benefit duration were reformed in 1997. Before 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, 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 17,000 Euros per year). Individuals with full benefit entitlements and at least three years of work experience during the four years prior to the year of entry into unemployment also became entitled to a so-called “wait-period benefit” after UI exhaustion. Wait-period benefits paid around two thirds of previous UI benefits, with no maximum duration limit.3 Job seekers 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.

3 After the end of our observation window, UI regulations have again been reformed. The maximum duration of UI benefits was reduced from 156 to 104 weeks in 2003. Wait-period benefits were abolished in 2008.
Table 1
A Summary of the UI Benefit Regimes in Norway 1993-2002

<table>
<thead>
<tr>
<th>Regime</th>
<th>Description</th>
<th>Requirement</th>
<th>Applies to spells starting</th>
</tr>
</thead>
<tbody>
<tr>
<td>I</td>
<td>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)</td>
<td>Satisfying UI income criterion, or has just completed military service</td>
<td>Before August 1995</td>
</tr>
<tr>
<td>II</td>
<td>Intermediate regime: 80 week UI period, followed by the possibility of applying for a new 78-week period (on slightly reduced benefits).</td>
<td>Satisfying UI income criterion</td>
<td>August 1995-December 1996</td>
</tr>
<tr>
<td>III</td>
<td>New regime, without eligibility for wait-period benefits: 156 week UI period.</td>
<td>Satisfying the highest of the two UI income criteria</td>
<td>From January 1997</td>
</tr>
<tr>
<td>IV</td>
<td>New regime, with eligibility for wait-period benefits: 156 week UI period, followed by indefinite wait-period benefits (two thirds of UI benefits)</td>
<td>Satisfying the highest of the two UI income criteria and a requirement of strong labor force attachment during past three years</td>
<td>From January 1997</td>
</tr>
<tr>
<td>V</td>
<td>New regime for persons with low past income: 78 week UI period.</td>
<td>Satisfying the lowest of the two UI income criteria</td>
<td>From January 1997</td>
</tr>
<tr>
<td>VI</td>
<td>New regime for individuals completing military service: 24 week UI period.</td>
<td>Just completed military service</td>
<td>From January 1997</td>
</tr>
</tbody>
</table>
| 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 |

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. 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).

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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.
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.

A number of alternative social programs are relevant for Norwegian job seekers, particularly if their joblessness is somehow related to health problems. In order to obtain UI benefits, a job seeker must be healthy and ready for full-time work on short notice. An unemployed job seeker who is deemed (by a General Practitioner) not to fulfill this requirement can claim sickness benefits instead. Sickness benefits then pay exactly the same amount of money as the UI benefits. However, when a person claims sickness benefits, the limited UI period is not spent and there are no obligations to search actively for work. Sickness benefits can be maintained for up to one year. After that, a person who is still considered too sick to work can apply for rehabilitation or disability benefits. Both these programs have replacement ratios slightly above UI insurance, but they generally require a more thorough medical examination. The eligibility rules are similar to those for unemployment insurance. Persons who are not eligible for any of these benefits can apply for means tested social security assistance. There are no national standards regarding the amount of money that can be paid out, but social assistance will normally pay significantly less than unemployment insurance and health related insurance.

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 around one quarter of the individuals have experienced repeated unemployment spells in different UI regimes. 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 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 (thereby generating a slight inaccuracy). 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 im-
posed. The latter can of course only happen as long as UI 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. In contrast to previous studies on Scandinavian register data (Røed and Zhang, 2003; 2005; Geerdens, 2006), we also identify 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 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 2

<table>
<thead>
<tr>
<th>Overview of Spells and Individuals</th>
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<tbody>
<tr>
<td>Number of individuals</td>
</tr>
<tr>
<td>Number of spells</td>
</tr>
<tr>
<td>Percent of individuals with more than one spell</td>
</tr>
<tr>
<td>Percent of individuals with spells in more than one regime</td>
</tr>
</tbody>
</table>

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 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; there are large differences in the sorting of entrants into the various regimes. It is evident from Table 3 that the 1997 UI-reform coincided with a significant shift in activation and sanction policies, implying less emphasis on activation and monitoring, and more emphasis on income insurance. The unconditional ALMP participation rate was more than twice as high in Regime I as it was in Regimes III and IV, and the sanction rate was more than 50 percent higher in Regime I. In the pre-1997 system, the Public Employment Service (PES) 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). The 1997 UI reform illustrates an intimate structural relationship between UI design, activation policies, and sanction practices in Norway, arising from the dominant view that there exists a lower bound on the income level that can be offered to unemployed job seekers who lost their previous work involuntarily, regardless of their observed search behavior. In practice, this implies that credible UI termination threats can be made only to the extent that paid activation is offered instead. Hence, the reduction in ALMP-intensity and the removal of the “soft” duration constraint after 80 weeks of job search can be viewed as two sides of the same coin.

Norwegian labor market programs can be divided into four main groups; i) labor market training, ii) temporary public employment, iii) temporary wage subsidies targeted at the private sector, and iv) work practice schemes. Labor market training and temporary wage subsidies are the most commonly used programs for adults, whereas work practice schemes are almost exclusively reserved for youths. The duration of the participation periods vary somewhat across the different programs; mean completed treatment durations are around 4-6 months for training programs, wage subsidies, and work practice schemes, and around 8 months for public employment. Participants in ALMP receive payments similar to typical UI benefit levels, and UI claimants do not draw on their benefit entitlements while participating (unless they prefer to maintain UI benefits instead of ALMP payment).

<table>
<thead>
<tr>
<th>Table 3</th>
<th>Descriptive Statistics</th>
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<tbody>
<tr>
<td></td>
<td>All</td>
</tr>
<tr>
<td></td>
<td>I</td>
</tr>
<tr>
<td>Total number of spells</td>
<td>1145777</td>
</tr>
<tr>
<td>Mean duration (months)</td>
<td>6.23</td>
</tr>
<tr>
<td>Percent of spells completed with a transition in the observation period</td>
<td>78.01</td>
</tr>
<tr>
<td>Percent of completed spells ending in: Employment</td>
<td>57.58</td>
</tr>
<tr>
<td>Percent of spells involving: Discretionary sanction</td>
<td>1.40</td>
</tr>
<tr>
<td>Part-time work</td>
<td>23.27</td>
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</tbody>
</table>

Average transition rate in
Table 3

Descriptive Statistics

<table>
<thead>
<tr>
<th>Regime (see Table 1)</th>
<th>All</th>
<th>I</th>
<th>II</th>
<th>III</th>
<th>IV</th>
<th>V</th>
<th>VI</th>
<th>VII</th>
<th>VIII</th>
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<tbody>
<tr>
<td>the first duration month to:</td>
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<td></td>
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<td></td>
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<tr>
<td>Other benefit</td>
<td>5.25</td>
<td>2.89</td>
<td>4.24</td>
<td>3.58</td>
<td>2.42</td>
<td>4.50</td>
<td>2.62</td>
<td>1.99</td>
<td>8.98</td>
</tr>
<tr>
<td>Education</td>
<td>5.08</td>
<td>3.13</td>
<td>4.12</td>
<td>3.92</td>
<td>1.42</td>
<td>4.98</td>
<td>12.34</td>
<td>0.16</td>
<td>7.96</td>
</tr>
<tr>
<td>Share with unemployment experience in the last four years (percent)</td>
<td>65.75</td>
<td>76.81</td>
<td>75.89</td>
<td>69.92</td>
<td>56.53</td>
<td>73.91</td>
<td>56.80</td>
<td>53.67</td>
<td>52.91</td>
</tr>
<tr>
<td>For individuals with unemployment experience:</td>
<td></td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Average time since the end of the last spell</td>
<td>12.39</td>
<td>11.33</td>
<td>13.69</td>
<td>14.03</td>
<td>15.27</td>
<td>11.90</td>
<td>12.60</td>
<td>6.39</td>
<td>10.85</td>
</tr>
<tr>
<td>Average number of months unemployed</td>
<td>11.74</td>
<td>13.77</td>
<td>12.84</td>
<td>10.81</td>
<td>7.99</td>
<td>12.63</td>
<td>6.62</td>
<td>18.80</td>
<td>10.38</td>
</tr>
<tr>
<td>Selected means and fractions (taken over spells)</td>
<td></td>
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<td></td>
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<tr>
<td>Men (percent)</td>
<td>51.00</td>
<td>53.36</td>
<td>53.31</td>
<td>48.27</td>
<td>54.16</td>
<td>39.05</td>
<td>97.78</td>
<td>49.06</td>
<td>44.88</td>
</tr>
<tr>
<td>Married (percent)</td>
<td>23.14</td>
<td>26.56</td>
<td>24.14</td>
<td>20.50</td>
<td>34.84</td>
<td>22.88</td>
<td>1.00</td>
<td>59.39</td>
<td>18.71</td>
</tr>
<tr>
<td>Educational attainment (years)</td>
<td>11.54</td>
<td>11.62</td>
<td>11.66</td>
<td>11.71</td>
<td>11.70</td>
<td>11.56</td>
<td>11.74</td>
<td>10.89</td>
<td>11.30</td>
</tr>
<tr>
<td>Work experience (years)</td>
<td>4.54</td>
<td>6.19</td>
<td>5.99</td>
<td>3.48</td>
<td>11.41</td>
<td>3.52</td>
<td>0.27</td>
<td>19.04</td>
<td>1.14</td>
</tr>
<tr>
<td>Age (years)</td>
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<td>36.00</td>
<td>28.93</td>
<td>21.32</td>
<td>45.14</td>
<td>25.67</td>
</tr>
<tr>
<td>Immigrants from OECD countries (percent)</td>
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<td>2.32</td>
<td>2.48</td>
<td>4.43</td>
<td>3.02</td>
<td>3.19</td>
<td>0.23</td>
<td>1.31</td>
<td>4.10</td>
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<tr>
<td>Immigrants from Non OECD countries (percent)</td>
<td>10.15</td>
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<td>9.37</td>
<td>1.72</td>
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<td>19.22</td>
</tr>
</tbody>
</table>

*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.

4 Statistical model and identification

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. The model is designed to disentangle the causal effects of these endogenous events from the observationally similar impacts of

\[\text{Note that we treat the loss of benefits during the quarantine period of the old UI benefit regime as an endogenous sanction (since a large fraction of the claimants do not lose their benefits at this point), while the loss of benefits after complete UI exhaustion is treated as an exogenous event. It is clear that not all individuals are under risk of experiencing all six events all the time. For example, it is obviously only benefit claimants that can be subject to a sanction. We also assume that only individuals below 35 years of age are under risk of starting ordinary education (very few individuals above this age make a transition to education, but those who do are right-censored).}\]
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 The 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 \( \varphi_{kit} \) as functions of observed (time-varying) variables and unknown parameters represented by index functions \( w_{kit} \) and (time-invariant) unobserved individual characteristics \( v_{ki} \):

\[
\varphi_{kit} = \int_{t_{k-1}}^{t_k} \theta_{kit} ds = \exp\left( w_{kit} + v_{ki} \right), \quad k = 1, \ldots, 6,
\]

where \( \theta_{kit} \) 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 some of the key explanatory variables that will be included in the index functions \( w_{kit} \), and discuss the causal mechanisms they are designed to represent.

Time has two dimensions in our model; calendar time and process time. The calendar time dimension reflects, e.g., business cycle and seasonal fluctuations and changes in government priorities 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 \( s_i \). 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 allocation of program slots. We label this “intrinsic 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 de-
pendence 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. We also include a single interaction term between spell duration and a business cycle indicator.\footnote{The business cycle indicator is measured at the time of entry. It is collected from Gaure and Røed (2007) and normalized to zero for a “mean” cyclical situation.}

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; see Appendix 1, Table A2 for details. Separate dummy variable sets are used to count down to the different “soft” and the “hard” constraints. In addition, we include a single interaction term between spell duration (measured in month) and a variable measuring the cyclical situation at the time of entry. 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.
- A 13 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 regime-specific dummy variables. Note that individuals with characteristics implying assignment to Regime I prior to the 1997 UI-reform were assigned to Regimes III, IV, or V 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 window. Hence, the estimated impacts of the “genuine” regime dummy variables can be given a sort of difference-in-difference interpretation; they reflect the changes in hazard rates that occurred for the reform-affected job seekers at the time of the reform and onwards, relative to the changes that occurred for the job seekers who were not affected by the reform, ceteris paribus.
The impacts of endogenous events – ALMP, sanction, and part-time work – are repre-
sented by 42 time-varying event indicators, denoted \( z_{it} \); see Appendix 1, Table A3. The spec-
ification allows for different impacts during their occurrences (on-treatment effects) and af-
fterwards (post-treatment effects); see Røed and Raaum (2006). During their occurrences, the
effects are also 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.

Finally, the model contains observed and unobserved individual characteristics. Ob-
served 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 www.frisch.uio.no/docs/UI_in_welfare_states.html.

We can now write the index functions as

\[
  w_{kit} = \sigma_{kit} s_{it} + \lambda_{kit} d_{it} + \delta_{k} b_{it} + \alpha_{k} z_{it} + \beta_{k} x_{it}, \quad k = 1, \ldots, 6, \tag{2}
\]

where \((\sigma_{ki}, \lambda_{ki}, \delta_{k}, \alpha_{k}, \beta_{k})\) are the parameters to be recovered from the data. Some of the ex-
planatory variables in (2) are clearly endogenous, in the sense that they depend on the out-
comes of the statistical process under consideration, and, hence, on unobserved heterogene-
ity. This is the case for the duration variables \( d_{it} \), the UI exhaustion indicators \( b_{it} \), and the state
variables \( z_{it} \). Apart from unemployment duration generated prior to the start of our observa-
tion 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.

Unobserved characteristics \( v_i = (v_{i1}, v_{i2}, v_{i3}, v_{i4}, v_{i5}, v_{i6}) \) 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 really
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 both the causal effect of these variables and the extent to which they are
correlated to unobserved characteristics.

\(^7\) 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.
To make the exposition simple, the notation used in (1) and (2) suppresses the fact that the set of potential transitions (the risk-set) depends on the UI status indicators $b_{it}$ and on the event-indicators $z_{it}$. For example, a person without UI benefits is not under risk of a sanction. And a person already experiencing either ALMP, part-time work, or a sanction, is currently not at risk of making that particular transition. This is of course taken into account when we set up the likelihood function.

4.2 Identification

The model raises a number of intricate identification issues related to the disentanglement of causal effects from potential sorting on unobserved heterogeneity. The causal effects of interest here are the impacts of process time (duration dependence), the impacts of endogenous events (ALMP, part-time work, sanction), the impacts of initial conditions (unemployment from previous spells), and the impacts of UI regime. In addition, we seek to decompose the impacts of process time into an intrinsic component and a UI-generated component.

The causal impacts of process time and of endogenous events have been shown to be non-parametrically identified on the basis of the proportional hazard assumption; see Van den Berg (2001) and Abbring and Van den Berg (2003). Since the proportionality assumption is questionable, however, we would like to have a stronger – and more data-based – foundation for disentangling causality from sorting. Fortunately, our data offer two additional sources for nonparametric identification that do not rely on the proportionality assumption. The first is the presence of time-varying exogenous explanatory variables; see McCall (1994) and Brinch (2007). Time-varying covariates naturally provide a sort of exclusion restriction in the sense that past values of these variables have no direct causal effect (conditional on their current values), and therefore correlate with current outcomes only via the sorting process. Hence, they indirectly provide the information required for disentangling the causal treatment and duration effects from impacts of unobserved sorting. The most important time-varying covariates in our data are clearly the calendar time dummies, capturing, e.g., cyclical and seasonal fluctuations in labor demand and changes in government priorities regarding the usage of labor market programs and sanctions. Cyclical fluctuations alone were responsible for large exogenous shifts in the job finding rates during our data period, with employment hazards being roughly twice as high during a cyclical peak than during a cyclical trough, ceteris paribus; see Gaure and Røed (2007). As we showed in Section 3, there were also significant changes over time in activation and sanction policies, with particularly large shifts coinciding
with the 1997 UI reform. Based on extensive Monte Carlo trials, Gaure, Røed, and Zhang (2007) show that calendar time variation in treatment propensities and final destination hazards are powerful tools for nonparametric identification of duration dependencies and effects of endogenous events.

An additional source of nonparametric identification comes from exploitation of repeated spells; see Abbring and Van den Berg (2003). Around 41 percent of the individuals in our data are represented with more than one spell (see Table 2). In general, we will argue that exploitation of repeated spells is a questionable identification strategy, since it rests on the assumption that, conditional on observed covariates, the unobserved characteristics of individuals do not change between subsequent spells. This restriction is justifiably only when the potential causal linkages between spells (in terms of, say, lagged duration dependence) are properly modeled; see Røed and Raaum (2006). As follows from the discussion above, we believe this to be the case in the present model; hence the repeated spells do contribute to nonparametric identification.

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 requires that individuals do not anticipate the realization of the stochastic process determining these events. Note, however, that knowledge regarding the determinants of these processes (e.g., in the form of model-consistent expectations) does not violate the no-anticipation assumption. Hence, we do not rule out that job search activity responds to changes in the ALMP or sanction hazards. We do rule out, however, that the agents possess private ex ante information regarding the actual timing of events. We will argue that the no anticipation assumption is defendable for the events of sanctions and ALMPs, since these measures are typically implemented quickly once the decisions are made. We realize, however, that the assumption may be violated for transitions to part-time work, potentially rendering the estimated effects of these events unreliable.

Our attempt to estimate the impacts of past unemployment (during the four year period prior to the start of a current spell) poses a particularly challenging identification – or initial conditions – problem. Past unemployment is obviously correlated to the unobserved characteristics that affect the outcome of a current spell; hence it cannot be assumed exogenous. To some extent, our model actually solves this problem, since unemployment durations

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8 Note that we do not require the calendar time variation in the treatment propensity to be independent of the cyclical variation in, e.g., the employment hazard. Since we include a full set of calendar time dummy variables in all hazard rates, the non-independent variation is fully (nonparametrically) controlled for in the model.
are indeed modeled as endogenous. Hence, the duration of a past unemployment spell can be viewed as a modeled event, just as the event of another type of “treatment”, and we can call upon the standard timing-of-events literature (Abbring and Van den Berg, 2003) to claim identification. There are two problems with this argument, however. The first is that many individuals have experienced unemployment in the four-year period prior to our data-window, and these events are not endogenously modeled. The impact of this initial conditions problem can be assessed by restricting the analysis to individuals for whom we actually observe the very first unemployment spell; i.e., delete all individuals who experienced unemployment between October 1989 and October 1993. Given our 8 year observation window and the high frequency of multiple spells, we would still have sufficient repeated spells in the data to examine their effects on subsequent spells. This is among the sensitivity analyses that we perform in Section 6 of this paper. The results from this analysis suggest that our failure to model past unemployment durations fully does cause a small bias in the estimated impacts of past unemployment, but that it does not significantly affect other parts of the model. The second problem is that we do not have a model for unemployment incidence. Hence, if the likelihood of having a repeated spell is correlated to the unobserved characteristics determining its outcome, we expect the causal effects of past unemployment to be biased. This problem can hardly be satisfactory solved within the present modeling framework; hence we interpret the estimated effects of past unemployment with some caution.

Given that the impacts of process time are identified, we disentangle intrinsic from UI-generated duration dependence on the basis of the assumption that intrinsic duration dependence is the same across UI regimes. While we are confident that the foundation for identifying exhaustion effects within the different UI regimes is solid, we realize that identifying the impacts of being allocated to different UI-regimes on the initial level of the hazard rates is a more challenging task. In particular, we acknowledge that the causal effects of not being entitled to benefits at all (Regime VIII) are unidentified, given that we have no random-assignment-like variation in the assignment of job seekers to this regime. We will argue, on the other hand, that the causal impacts of belong to the post-reform Regimes III, IV, or V, as opposed to the pre-reform Regime I, is potentially identified. Identification rests on a particular feature of the proportionality assumption, however, namely that all other calendar time effects are the same across UI regimes. In particular, we require equal calendar time effects for spells that were affected by the 1997-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). Since uninsured spells are dominated by youths and other persons with little or no labor market experience, this assumption may be questionable. In the sensitivity analysis in Section 6, we estimate a version of the model where we allow calendar time effects to vary by claimant-status. The results indicate that the calendar time effects were indeed very similar for claimants and non-claimants.

It follows from the discussion above that the UI reform is strictly required only to identify the effects of the various UI regimes and to compare the impacts of UI exhaustion for hard and soft constraints. In addition, the UI reform contributes to the identification of intrinsic duration dependence, since it makes the correlation between spell duration and UI exhaustion weaker. Finally, viewed as a shock to the activation and sanction policies, the reform contributes to the exogenous time-variation in sanction and ALMP transition rates. To assess the identifying role of the reform, we have also estimated the model on post-reform data only; see Section 6. It turns out that the reform is of critical importance for the identification of sanction effects, of moderate importance for the identification of ALMP effects, and of no importance for the identification of intrinsic duration dependence.

4.3 Likelihood function and estimation algorithm

The likelihood function for this model is derived in Appendix 2. Since our data are interval-censored and contain information on labor market state by the end of each month only, it is set up in terms of monthly transition probabilities. The interval censoring also creates a left-truncation problem, as spells starting and ending in the same month are never recorded. This implies that individuals are included in the dataset conditional on having a spell that survives the month of entry. In the appendix, we show how we can use Bayes’ theorem to derive the appropriate conditional distribution of unobserved heterogeneity on the basis of the unconditional distribution (at entry). We use a non-parametric approach to account for the unconditional distribution of unobserved heterogeneity. 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). 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 pa-

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9 For practical and computational reasons, we consider this to be the case when the log-likelihood increases by less than 0.01.
rameters. 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).

5 Results

This section presents selected results. We first examine the mechanisms of intrinsic and UI-generated duration dependence. We then turn to the direct impact of sanctions, active labor market programs, 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 at www.frisch.uio.no/docs/UI_in_welfare_states.html. On this site, we also present complete results for the model without unobserved heterogeneity and for a number of sensitivity analyses; see Section 6.

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/her 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.\textsuperscript{10}

![Figure 1. Estimated intrinsic duration dependence for individuals with no previous unemployment (during the four years prior to the spell)](image)

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.

\textsuperscript{10} 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 4.1. 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.
It turned out, however, that the time since completion of the last spell has little impact on current hazard rates (not shown).

![Figure 2](image-url)

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.

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.
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 III, 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.

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

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. More intensive usage of activation and sanctions before the 1997 reform than afterwards is also confirmed by the estimated calendar time effects (not shown). 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).

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. 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 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

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11 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 \( \hat{b}_s \) be the vector of point-estimates. The parameters drawn for trial \( s \) are then given as \( b_s = \hat{b} + Lz_s \).
number of transitions to other benefits, and correspondingly minor reductions in the number of transitions to employment and education.

<table>
<thead>
<tr>
<th>Regime</th>
<th>Mean unemployment duration</th>
<th>Duration difference (Regime III/IV – I)</th>
<th>Percent of spells ending in percentage point difference (Regime III/IV – I)</th>
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</thead>
<tbody>
<tr>
<td>I</td>
<td>5.86</td>
<td>1.57 [1.47, 1.69]</td>
<td>73.94 [-0.65, 0.14]</td>
</tr>
<tr>
<td>III/IV</td>
<td>7.43</td>
<td>1.61 [1.49, 1.76]</td>
<td>73.65 [-0.25, 0.0]</td>
</tr>
</tbody>
</table>

Table 4
The predicted impacts of the 1997 reform

<table>
<thead>
<tr>
<th>Final outcome</th>
<th>Employment</th>
<th>Other benefit</th>
<th>Education</th>
</tr>
</thead>
<tbody>
<tr>
<td>Regime I</td>
<td>5.41</td>
<td>8.72</td>
<td>5.26</td>
</tr>
<tr>
<td>Regime III/IV</td>
<td>7.02</td>
<td>10.25</td>
<td>6.52</td>
</tr>
</tbody>
</table>

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

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 $\sigma_t$. 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.

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 that these individuals were subject to particularly adverse labor demand shocks, we have no reason to believe that previous state-employees represent a particularly hard-to-employ group. Many of the reorganizations resulted from increased competition and privatization, rather than from a negative general shift in the demand for the labor services in question.
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.
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 to zero (but the other parameters are generated from our model). The 100x2 simulations are performed on the basis of drawings from the joint normal distribution of parameter estimates. For simplicity, 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 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

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>Final outcome</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>With effect</td>
<td>Without effect</td>
</tr>
<tr>
<td>Mean unemployment duration</td>
<td>5.98</td>
<td>5.64</td>
</tr>
<tr>
<td>Duration difference per treatment (With-without)*</td>
<td>1.27</td>
<td>[1.04, 1.50]</td>
</tr>
<tr>
<td>Percent of spells ending in positive</td>
<td>57.05</td>
<td>56.45</td>
</tr>
<tr>
<td>Percentage point difference per treatment (With-without)*</td>
<td>2.04</td>
<td>[0.46, 3.35]</td>
</tr>
</tbody>
</table>

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. We emphasize that the estimated effects of part-time employment should be interpreted with care, since the so-called no anticipation assumption is likely to be violated for these transitions; see Section 4.2.

6 Sensitivity analysis

This section briefly summarizes how the results change when we modify some of the key identifying assumptions. Complete results for all the sensitivity analyses are available at [www.frisch.uio.no/docs/UI_in_welfare_states.html](http://www.frisch.uio.no/docs/UI_in_welfare_states.html).

6.1 Initial condition and effects of past unemployment

The first sensitivity analysis relates to the initial condition 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 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 that the
estimated negative causal impact of lagged unemployment becomes somewhat smaller (typi-
cally a few percentage points smaller reduction in the employment hazard) in the reduced
model. This is particularly the case for very long past spells, and reflects that failure to ap-
propriately model past unemployment as endogenous contributes to exaggerating its adverse
impacts on subsequent spells. To find out whether this problem may have affected the size of
our estimated reform effects, we repeated the simulations reported in Section 5.2 based on the
model with only individuals with no unemployment exposure during the four-year period
prior to their first spell included. The estimated overall impact of the 1997-reform on average
unemployment duration was then equal to 1.41 months, rather than the 1.57 months reported
in Table 5.

6.2 Identification of regime effects – the proportionality assumption

The second sensitivity analysis concerns the identification of the regime effects, which to a
large extent was based on the assumption that calendar time effects were the same for UI
claimants and non-claimants. We have examined the validity of this assumption by estimat-
ing calendar time effects separately for the two groups. The resultant sets of calendar time
effects for the three final-destination hazards and for the ALMP hazard are provided in Figure
6. 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 benefit-shift hazard) and at the end (for the employment hazard and the bene-
fit-shift hazard). To assess the impact of these possible violations of the proportionality as-
sumption on estimated reform effects, we have also estimated the model on the subset of UI
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 unem-
ployment duration was 1.27 months.
Figure 6. 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.

6.3 The role of the 1997 reform in identifying the model

To assess the role of the UI reform in identifying the model, we have also estimated the model on post-reform data only. This model can of course not identify the effects of the reform itself (the regime effects), but – as argued in Section 4.2 – it does formally satisfy the conditions for identification of the effects of intrinsic duration dependence and endogenous events. It turns out that the estimated intrinsic duration dependence is virtually unchanged when only post-reform data are used instead of the complete dataset, with the exception that the estimated benefit-shift and sanction hazards become slightly more subject to positive duration dependence. The estimated impacts of endogenous events are more substantively altered when we rely on the reduced dataset, however. First, the positive impact of a sanction on the employment hazard disappears completely (and even become slightly negative). Second, the estimated post-program effects of ALMP on the employment hazard become more positive (the favorable proportional post-program shifts due to participation are raised by ap-
proximately 30 percent in comparison to the model based on the complete dataset). Both these changes in estimated results may indicate that the significant exogenous time-variation in activation and sanction risk generated by the reform is indeed important for identification of these effects, since it implies less reliance on the proportionality assumption. However, we cannot rule out that the reform also caused a shift in the actual causal effects of these events (which our model does not allow for). In particular, we may speculate that the significant reduction in the usage of activation after the reform contributed to a rise in its quality.

7 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 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.

References


Appendix 1

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

<table>
<thead>
<tr>
<th>I. Past unemployment</th>
<th>II. Ongoing unemployment</th>
</tr>
</thead>
<tbody>
<tr>
<td>Definition of groups according to past unemployment during the 48 months just prior to the start of the current spell</td>
<td>Group-specific baseline hazards divided into the following piece-wise constant parts (duration in months)</td>
</tr>
<tr>
<td>(fraction of spells belonging to each group in parentheses)</td>
<td></td>
</tr>
<tr>
<td>Time since last unemployment spell was completed (# months)</td>
<td>No previous unemployment</td>
</tr>
<tr>
<td>0</td>
<td>Group 1</td>
</tr>
<tr>
<td></td>
<td>(34.25)</td>
</tr>
<tr>
<td>1-6</td>
<td>Group 2</td>
</tr>
<tr>
<td></td>
<td>(8.52)</td>
</tr>
<tr>
<td>7-12</td>
<td>Group 6</td>
</tr>
<tr>
<td></td>
<td>(5.68)</td>
</tr>
<tr>
<td>13-24</td>
<td>Group 10</td>
</tr>
<tr>
<td></td>
<td>(5.68)</td>
</tr>
<tr>
<td>&gt;24</td>
<td>Group 14</td>
</tr>
<tr>
<td></td>
<td>(6.25)</td>
</tr>
</tbody>
</table>

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 $16 \times 10 = 160$ dummy variables.

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

<table>
<thead>
<tr>
<th>Variable description</th>
<th>Relevant for Regimes, Conf. Table 1</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 16-18 months left until potential temporary benefit exhaustion (with or without quarantine), or in the application process (given eligibility)</td>
<td>I, II</td>
</tr>
<tr>
<td>2 13-15 months left until potential temporary benefit exhaustion (with or without quarantine)</td>
<td>I, II</td>
</tr>
<tr>
<td>3 7-12 months left until potential temporary benefit exhaustion (with or without quarantine)</td>
<td>I, II</td>
</tr>
<tr>
<td>4 6 months left until potential temporary benefit exhaustion (with quarantine)</td>
<td>I</td>
</tr>
<tr>
<td>5 5 months left until potential temporary benefit exhaustion (with quarantine)</td>
<td>I</td>
</tr>
<tr>
<td>6 4 months left until potential temporary benefit exhaustion (with quarantine)</td>
<td>I</td>
</tr>
<tr>
<td>7 3 months left until potential temporary benefit exhaustion (with quarantine)</td>
<td>I</td>
</tr>
<tr>
<td>8 2 months left until potential temporary benefit exhaustion (with quarantine)</td>
<td>I</td>
</tr>
<tr>
<td>9 1 month left until potential temporary benefit exhaustion (with quarantine)</td>
<td>I</td>
</tr>
<tr>
<td>10 6 months left until potential temporary benefit exhaustion (without quarantine)</td>
<td>I</td>
</tr>
<tr>
<td>11 5 months left until potential temporary benefit exhaustion (without quarantine)</td>
<td>I</td>
</tr>
<tr>
<td>12 4 months left until potential temporary benefit exhaustion (without quarantine)</td>
<td>I</td>
</tr>
<tr>
<td>13 3 months left until potential temporary benefit exhaustion (without quarantine)</td>
<td>I</td>
</tr>
<tr>
<td>14 2 months left until potential temporary benefit exhaustion (without quarantine)</td>
<td>I</td>
</tr>
<tr>
<td>15 1 month left until potential temporary benefit exhaustion (without quarantine)</td>
<td>I</td>
</tr>
<tr>
<td>16 13 week quarantine imposed (without UI benefits) after 80 weeks</td>
<td>I</td>
</tr>
<tr>
<td>17 Exempted from 13 week quarantine after 80 weeks</td>
<td>I</td>
</tr>
<tr>
<td>18 13 week benefit extension granted after the second 80 week period</td>
<td>I</td>
</tr>
<tr>
<td>19 &gt;60 months left until final benefit exhaustion or entitled to benefits until retirement age (previous state employees)</td>
<td>VII</td>
</tr>
<tr>
<td>20 37-60 months left until final benefit exhaustion (previous state employees)</td>
<td>VII</td>
</tr>
<tr>
<td>21 34-36 months left until final benefit exhaustion (with or without entitlement to wait-period benefits) or in the application process (given eligibility)</td>
<td>III, IV, VII</td>
</tr>
<tr>
<td>22 19-33 months left until final benefit exhaustion (with or without entitlement to wait-period benefits)</td>
<td>III, IV, VII</td>
</tr>
</tbody>
</table>
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 month 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 month 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>
<thead>
<tr>
<th>Event</th>
<th>Variable description</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Program participation</strong></td>
<td></td>
</tr>
<tr>
<td>Ongoing</td>
<td>10 dummy variables accounting for duration of <em>ongoing</em> program participation: 0 (no ongoing ALMP), 1 month, 2 months,….8 months, &gt;8 months.</td>
</tr>
<tr>
<td>Completed</td>
<td>10 dummy variables accounting for duration of <em>completed</em> program participation (within the same spell): 0 (no completed ALMP), 1 month, 2 months,….8 months, &gt;8 months. Completed program participation is set to zero upon start of a new ALMP participation.</td>
</tr>
<tr>
<td><strong>Part time work</strong></td>
<td></td>
</tr>
<tr>
<td>Ongoing</td>
<td>10 dummy variables accounting for duration of <em>ongoing</em> part-time work: 0 (no ongoing part-time work), 1 month, 2 months,….8 months, &gt;8 months.</td>
</tr>
<tr>
<td>Completed</td>
<td>10 dummy variables accounting for duration of <em>completed</em> part-time work (within the same spell): 0 (no completed part-time work), 1 month, 2 months,….8 months, &gt;8 months. Completed part-time work is set to zero upon start of a new part-time job.</td>
</tr>
<tr>
<td><strong>Sanctions</strong></td>
<td></td>
</tr>
<tr>
<td>Ongoing</td>
<td>One dummy indicating ongoing UI sanction</td>
</tr>
<tr>
<td>Completed</td>
<td>One dummy indicating completed sanction</td>
</tr>
</tbody>
</table>
Appendix 2

Based on the hazard rates defined in Equations (1) and (2), the probability that individual $i$ makes a transition to state $k$ during time period $t$ is equal to:

$$p_k(w_{kit} + v_{ki}) = \left(1 - \exp\left(-\sum_{k \in K_{it}} \exp(w_{kit} + v_{ki})\right)\right) \frac{\exp(w_{kit} + v_{ki})}{\sum_{k \in K_{it}} \exp(w_{kit} + v_{ki})}, \quad (A1)$$

where $K_{it}$ is the set of feasible transitions for individual $i$ in period $t$. 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:

$$L_i(v_i) = \prod_{y_{kit} \in Y_i} \left[ \left(1 - \exp\left(-\sum_{k \in K_{it}} \exp(w_{kit} + v_{ki})\right)\right) \frac{\exp(w_{kit} + v_{ki})}{\sum_{k \in K_{it}} \exp(w_{kit} + v_{ki})} \right] ^ {y_{kit}} \times \left[ \exp\left(-\sum_{k \in K_{it}} \exp(w_{kit} + v_{ki})\right)\right] ^ {1 - \sum_{k \in K_{it}} y_{kit}}. \quad (A2)$$

In order to arrive at the marginal likelihood, we need to integrate unobserved heterogeneity $v_i$ out of Equation A2. The way we do this takes into account that there is a left-truncation problem represented in our data, implying that individuals are included in the data-set 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 period.

---

12 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:

$$\int_0^t \exp(w_{it} + v_{it}) \exp\left(-\sum_{k \in K_{it}} \int_0^t \exp(w_{it} + v_{it}) \, ds\right) \, du = \int_0^t \left[\exp(w_{it} + v_{it}) \exp\left(-\sum_{k \in K_{it}} (u - (t-1)) \exp(w_{it} + v_{it})\right)\right] \, du$$

$$= \left(1 - \exp\left(-\sum_{k \in K_{it}} \exp(w_{it} + v_{it})\right)\right) \frac{\exp(w_{it} + v_{it})}{\sum_{k \in K_{it}} \exp(w_{it} + v_{it})}.$$
Let \( t_i \) be the inflow month for individual \( i \) and let \( w_{it} = (w_{i1}, w_{i2}, w_{i3}, w_{i4}, w_{i5}, w_{i6}) \). The conditional density of unobserved heterogeneity \( f(v_i | d \geq 1) \) is then related to the unconditional density \( f(v_i) \) by

\[
f(v_i | d \geq 1) = \frac{\text{Pr}(\sum_k y_{kit} = 0 \mid w_{it} + v_i)}{E \{ \text{Pr}(\sum_k y_{kit} = 0 \mid w_{it} + v_i) \}} f(v_i), \tag{A3}
\]

where

\[
\text{Pr}(\sum_k y_{kit} = 0 \mid w_{it} + v_i) = \int_0^\infty \exp(-(1-s)) \sum_k \exp(w_{kit} + v_k) ds
\]

\[
= 1 - \exp\left(-\sum_k \exp(w_{kit} + v_k)\right) \tag{A4}
\]

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

\[
L = \prod_{i=1}^N \sum_{l=1}^Q q_l \frac{\text{Pr}(\sum_k y_{kit} = 0 \mid w_{it} + v_i)}{\sum_{l=1}^Q q_l \left[ \text{Pr}(\sum_k y_{kit} = 0 \mid w_{it} + v_i) \right]} L_i(v_i), \quad \sum_{l=1}^Q q_l = 1, \tag{A5}
\]

where \( L_i(v_i) \) is given in A2 and \( \text{Pr}(\sum_k y_{kit} = 0 \mid w_{it} + v_i) \) is given in A4.

---

13 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. \( \lambda_{a0} = \lambda_{a1} \). 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.