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EFFECTS OF CHANGES IN THE UNEMPLOYMENT INSURANCE ELIGIBILITY REQUIREMENTS ON JOB DURATION – SWEDISH EVIDENCE

by

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Effects of Changes in the Unemployment Insurance Eligibility Requirements on Job Duration — Swedish Evidence[†]

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Abstract:

This paper investigates the impact of the unemployment insurance (UI) entrance requirement on employment duration among earlier unemployed in Sweden. I exploit changes in the rules taking place in 1994 and 1997 to study behavioural adjustments in the timing of job separation in 1992, 1996, and 1998 respectively. Performing across-year analyses with years involving different working requirements, I find evidence of clustering of job exits at the time of UI qualification. By using predicted hazard rates for each week, I calculate an approximate 2.9-week extension in average employment duration between 1996 and 1998, due to the 5-week prolonging of the entrance requirement.

JEL classification: J22, J65, J68

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1. Introduction

Most research about the impact of the unemployment insurance (UI) system has focused on the replacement ratio or the length of the entitlement period. These parameters have been embodied in job-search models to explain labour supply. But the UI system also consists of eligibility requirements that could also affect labour market behaviour. The entrance requirement (ER) is the number of weeks a person must work to become eligible for UI benefits. To what extent does the ER influence employment duration, that is, the time period in which the person is employed?

Several studies, among them Cousineau (1985) and Kesselman (1985), note that such a connection may exist on the employee side.[1] Kesselman notes that "there are... some workers in all industries and regions who prefer a lifestyle of intermittent work combined with regular unemployment spells subsidised by UI benefits." Such a work pattern fits the description of seasonal jobs. The variation in the extent of activity could be demand- driven (tourist industry) but more likely due to within-year fluctuations in production costs (construction work, farming, forestry, fishery).[2] Also, firms that are aware of the UI regulations know that the UI system will attenuate the workers' separation costs and can therefore employ workers for short periods to meet short-term needs. So the behaviour of rational agents on both sides of the labour market could account for the UI system. In these cases, benefit receipt primarily acts to redistribute income and leisure for actors "playing the system" — and not as an insurance.

Internationally, only a few studies have focused on the ER and its impact on employment duration. Baker & Rea (1994), Christofides & McKenna (1996), Green & Riddell (1997), and Green & Sargent (1998) used employment hazards to study UI incentives in spell duration. They all used data from the Canadian *Longitudinal Labour Market Activity Survey* to construct large samples of job duration. Christofides & McKenna found evidence of that a significant number of jobs were terminated when the ER was satisfied in 1986/87. Green & Riddell and Baker & Rea make use of a temporary extension in the ER from 10 to 14 weeks in 1990. Both found evidence of increased hazard rates the week of fulfilling the eligibility requirement. Green & Riddell estimated a 1.5-week increase in the average duration of employment in re-

[1] Both studies refer to the Canadian labour market.

[2] Edebalk & Wadensjö (1978).

gions with unemployment rates over 11.5%. Baker & Rea conclude that the effect that they observe may in part be due to the awareness of the UI system in Canada and Canadians' high degree of familiarity with the programme. So similar results should extend to countries in which the work force has knowledge about UI. They also argue that UI-programme awareness will be highest in industries or regions with employment instability. The reason is that frequent unemployment spells distribute information about the UI system among the work force. Finally, Green & Sargent found substantial UI-related impacts on the job-spell hazard rate in seasonal but not in non-seasonal industries.

In 1996, the Swedish UI system required that to qualify for benefits, applicants must have worked 5 calendar months within a 12-month period. [3] In July 1997, this rule was changed to 6 calendar months. The reason for extending the ER was that the Swedish government wanted applicants to have a closer affiliation with the labour market in order to receive UI compensation. So the change is primarily directed toward people outside the UI system — those who have not yet satisfied the work requirement a first time. But the extension also affects job duration in general because all of those, who initiate job spells, have the incentives to fulfil the minimum requirement. The main object of this paper is to investigate the ER's influence on the timing of job exits among those inside the UI system, i.e., those have fulfilled the ER at least once. I exploit changes in the rules taking place in 1994 and 1997 to study behavioural adjustments in the timing of job separation in 1992, 1996, and 1998 respectively. To establish the length of employment spells, I use unemployment register data and information on the duration between the end of one unemployment spell, and start of another.

Using a flexible piece-wise constant exponential hazard model to pick up weekly job exits, I find evidence of an increased job turnover rate at the time of fulfilling the ER in all three years. The approximate 5-week extension of the ER in 1997 generated an estimated 3-week prolonging of the average employment spells between 1996 and 1998. Restricting the analysis to one sector and one region, each characterised by relatively large circular flows between jobs and unemployment and likely high awareness of the UI system, indicates that changes in the ER is particularly important in industries and regions with seasonal pattern in the production process.

[3] In Sweden, a first-time applicant must work to qualify for benefits. A second-time (or more) applicant can qualify through participating in labour market programmes.

The next section describes Sweden's UI system and explains the ER for all years studied. The following section presents a simple, static, labour supply model. This serves as theoretical motivation in which the laid-out UI incentives predict job-termination clustering at the minimum number of required weeks of work. Section 4 contains some descriptive statistics concerning the degree of circular flow on the labour market and its importance in this context. Section 5 presents the data. Section 6 outlines the empirical framework, and Section 7 presents the results. The last section sums up and makes some concluding remarks.

2. Unemployment benefit in Sweden

The Swedish unemployment benefit system has two parts: i) *Basic insurance*, whereby compensation is available for those who are *not* members of a UI fund and are age 20, and ii) *Income-loss insurance*, whereby a person must have paid membership dues into a UI fund during a period of at least 12 months--the *membership condition* rule.[4]

From July 1, 1989 to July 1, 1994, applicants had to be employed 75 days (at least 3 hours a day) in 4 calendar months during the last 12 months in order to qualify for UI benefits. The 12 months are called the reference period. Between January 1, 1995 and July 1, 1997, the ER was a minimum of 80 days of employment (at least three hours a day) occurring during 5 calendar months in the 12-month reference period. In practice, the two rules were rather similar; the difference was that work (or equivalent) had to occur in one more month. In 1997, the requirement was changed to include work in at least 6 calendar months during a 12-month period and at least 70 hours each month. Or, a person had to work at least 450 hours during a composite period of 6 calendar months and at least 45 hours each month. The restriction implies that working in the 15 January – 15 June interval is enough to receive the UI provision from July 1, 1997. In practice, this is only a 5-month period, but because work has occurred during 6 calendar months, the ER is fulfilled. In the same way, 4 months was sufficient between 1996 and 1997, and 3 months was enough 1989-1994.[5]

[4] I do not describe the contents of the basic insurance in any detail because basic insurance recipients are excluded from the analysis later. The reason is the differing ERs for basic insurance receivers and for those who received income-loss insurance in 1996.

[5] For information in this section part, see SFS 1988:645, SFS 1994:1673, and SFS 1997:238. Details about the temporary rule between July 1 1994 and January 1 1995 are not given here.

In 1992, qualifying applicants received 90% of their daily earnings; in 1996, 75%; and in 1998, 80%. The benefit period is 300 days (5 days per week, i.e., 60 weeks). An applicant, age 55, (age 57 from January 1998) is entitled to 450 days of benefits. To receive any compensation, the entrance requirement (ER) must also be fulfilled. From January 1, 1996, working is the only way to become eligible as a first-time applicant. For a second-time applicant, the requalifying condition applies. Then participation in labour market training, vocational rehabilitation, education financed by training allowance and military service, also enable an applicant to qualify. UI entitled who quit employment without a legitimate reason are disqualified from benefits for nine weeks.[6]

3. Theoretical motivation

In the following, I present a static labour supply model earlier used by Moffitt & Nicholson (1982) and Green & Riddell (1997) that outlines the effects of UI for workers.[7] The model assumes that unemployment is voluntary and that agents have limited time horizons in which they consider their budget opportunities and choose the number of weeks to be employed and unemployed, respectively. The individual maximises utility, which is a function of total net income (consumption) and leisure over the period. The model assumes a continuous distribution of preferences generating a corresponding distribution of employment spells.

Figure 1 shows the budget constraint for an unemployed person with UI benefits (*CDE*), and without (*CB*). I use a one-year time horizon in the figure to put focus on seasonal unemployment, where work is concentrated to a limited period each year. Prior to exhaustion, an additional unemployed week reduces income by W-B, where $B=Wr$ is weekly UI benefit. After benefit exhaustion, at *D*, an additional week of unemployment suggests the loss of income corresponding to the net potential weekly wage (W). $HMIN$ denotes the minimum number of weeks a person must work to become entitled for UI benefits. Two particular responses suggest job-termination clustering at $HMIN$:

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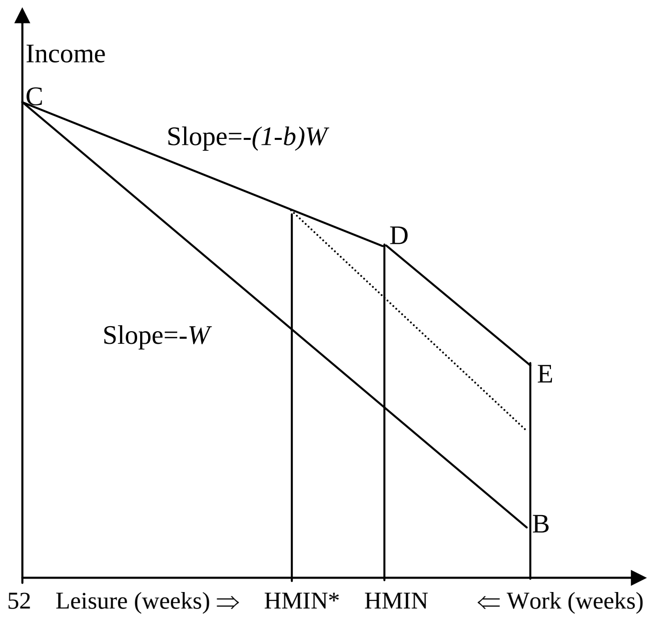
[6] For information in this section part, see SFS 1989:331, SOU 1996:150, SFS 1997:238, SFS 1987:226, and SFS 1995:1636.

[7] Christofides & McKenna (1996) present a model that includes the potential influence of the ER on both worker's and firm's behaviour. The model presumes that quits and layoffs are behaviourally distinct. However, both in Canada and Sweden, individuals who quit risk a waiting period before receiving benefits. It is therefore reasonable to expect implicit contracts between workers and firms as the main source of ER effects.

1. Many of those who in the absence of UI wish to work less than $HMIN$, would in the presence of UI want to work additional weeks to qualify for benefits. This primarily concerns those with spells a few weeks short of the ER and not people ending jobs well before.
2. Many of those who, in the presence of UI, would work beyond $HMIN$, would in the presence of UI face both income and substitution effects that imply a reduction in work to $HMIN$.

Figure 1: Budget constraint for individuals, 52-week horizon



A change in the required weeks of work is expected to shift the mass point in the figure from $HMIN$ to $HMIN^*$. Given an extension of four weeks, the return for a person at the initial kink that adjusts to the new ER is $4W+(x \cdot 0.8 \cdot W)$, where x is the number of unemployment weeks. Depending on the distribution of individual preferences, some people will also reduce their labour supply or choose to withdraw from the labour force. The height of the new mass point and the effect on average employment duration is hard to predict.

4. The circular flow on the labour market

This section focuses on the unemployment dynamics in Sweden between 1994 and 1997. In Table 1, the first row reports the total number of unemployment weeks in each year. The second row gives the number of weeks attributable to first-time unemployed — either receiving benefits or not. The latter group is a target group in the government's requirement for more work in order to receive compensation. The contribution from this particular group to the stock of unemployment weeks is halved over the studied years (from 15.4 to 7.6%). Dividing the first-time registered into UI receivers and non-UI receivers respectively (rows 3-6), the reduction is derived from the former group. Note also the average unemployment spell in this group is 6-8 weeks more extensive than the average spell in the group of non-receivers, and that the average spell length for both groups is gradually diminishing.

The table also provides an estimate of the degree in which unemployment is attributable to persons who were employed for a relative short period (at least twice between 1994 and 1997) and were also openly unemployed the remaining days of a 360-day period (rows 7).[8] This is the work pattern that we would expect among seasonally unemployed, where jobs are concentrated to a certain period each year. The share is stable around 1 per cent which indicates that this could be a relatively small problem on aggregate. Including repeated participation in LMPs, seasonal unemployment amounts to only 3.6-4.1 per cent of aggregate unemployment (row 9). The relative strict definition of circular flow keeps the measures down.

Rows 10 and 11 report the number of unemployment weeks derived from people entering jobs and LMP, respectively. The decreasing number preceding LMPs in row 12 is mainly due to a 10-week drop in the average unemployment period foregoing the programme start between 1994 and 1997. In turn, this affects the corresponding spell of unemployment before entering a job (row 11), because the time for job search is reduced. The shorter unemployment periods need not influence the magnitude of circular flow. But if an unemployed person is encouraged to take more temporary jobs (or to take jobs of short duration) to avoid programme participation, the circular flow between jobs and unemployment could increase. Figure 2 shows the

[8] Only start of the employment period is restricted to the particular calendar year. So the number of unemployment weeks in these rows only roughly refers to the particular calendar year.

Table 1: Total unemployment weeks 1994-97 allocated on different types of unemployment, 1000s weeks. Numbers in parentheses show the share of total number of unemployment weeks in each year (row 1)

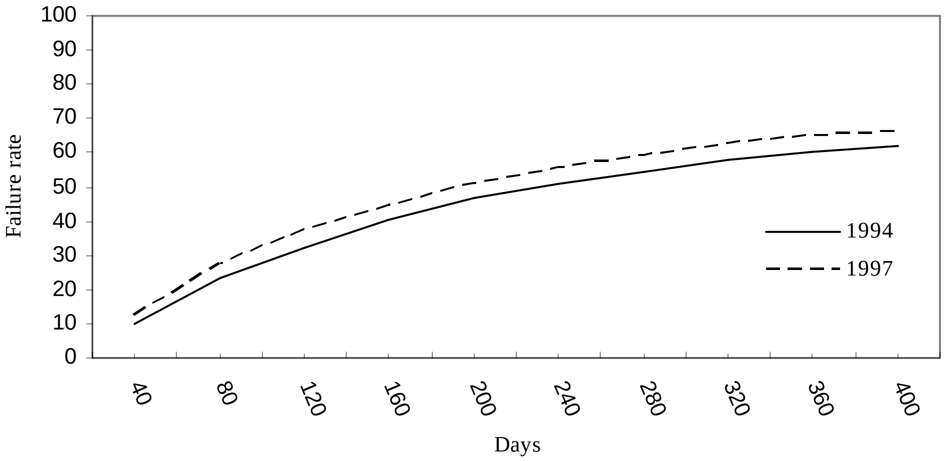
	1994	1995	1996	1997
1) Total number of unemployment weeks in a calendar year.	22,478	22,038	20,715	18,723
2) Total number of unemployment weeks for people registered as unemployed for the first time since 1991.[9]	3,417 (15.4%)	2,462 (11.2%)	1,756 (8.5%)	1,421 (7.6%)
3) receiving UI compensation	2,081 (9.4%)	1,385 (6.3%)	952 (4.6%)	571 (3.0%)
4) mean duration of unemployment spells (weeks).	21.7	20.6	18.5	17.1
5) not receiving UI compensation	1,082 (4.9%)	934 (4.2%)	707 (3.4%)	826 (4.4 %)
6) mean duration of unemployment spells (weeks).	14.8	14.1	10.3	9.0
7) Total number of unemployed weeks for people who, at least twice in the years 1994-97, worked for 3-9 months (composite time) and were unemployed the remaining days of a 360-day period.	234 (1.1%)	272 (1.2%)	272 (1.3%)	195 (1.0%)
8) Total number of unemployed weeks for people who, at least twice in the years 1994-97, participated in a labour market programme 3-9 months (composite time) and were unemployed the remaining days of a 360-day period.	600 (2.7%)	628 (2.8%)	555 (2.7%)	470 (2.5%)
9) Total number of weeks of circular flow (3+4).	833 (3.8%)	900 (4.1%)	827 (4.0%)	665 (3.6%)
10) Total number of unemployment weeks in which a person with UI compensation enters a job.	5,226 (23.6%)	4,538 (20.6%)	4,120 (19.9%)	3,842 (20.5%)
11) mean duration of unemployment spells (weeks).	14.3	13.6	12.5	10.9
12) Total number of unemployed weeks where a person with UI compensation enters a LMP.	6,051 (27.3%)	5,906 (26.8%)	4,896 (23.6%)	3,701 (19.8%)
13) mean duration of unemployment spells (weeks).	21.5	18.6	15.1	11.8

Notes: (1) The sample size is 5% of the population, so all measures are multiplied by 20 to get estimates at the level of the population. (2) The sample includes individuals between ages 18-65. Source: Own computations from the longitudinal data from the Swedish Labour Market Board.

[9] This was the first year of the longitudinal data base.

elapsed time before a person who left unemployment for a job returns to unemployment. The job duration in 1997 is significantly shorter than in 1994.[10,11]

Figure 2: The employment spell failure rates before returning to unemployment in 1994 and 1997.



Note: (1) The sample size is 5% of the population, so all measures are multiplied by 20 to get estimates at the level of the population. (2) The sample includes individuals between 18 and 65. Source: Own computations from the longitudinal data from the Swedish Labour Market Board.

5. The data

Studying employment spells I use unemployment data and the duration between the end of one unemployment period, and the start of another. The database *Händel*, administered by the Swedish Labour Market Board (SLMB), consists of continuous information about every unemployment and programme spell of all people registered at the employment offices from August

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[10] The test performed is a log-rank test (Allison, 1995); the test statistic is distributed as $\chi^2(1)$ and takes a value of 70.8.

[11] Figures A1 and A2 in Appendix A illustrate that recurrent unemployment is more frequent in certain industries and regions. Comparing regions, local labour markets tend to have a higher circular-flow level. With the seasonal aspect in mind, this is no surprise because these markets are located in the northern part of the country where the winter season affects the job pattern. Among job categories, manufacturing and mining is above average while administrative work is well below the same. Farming, forestry, and fishery have a high share of circular-flow behaviour (9-12%) due to extreme working conditions. Figure A2 does not depict theses industries.

1991 and onward. *Händel* also includes individual characteristics such as gender, age, education, desired profession, experience in desired profession, citizenship, county, and disability. The unemployment exit cause is also available. The database contains no specific information on employer or previous employer. I use three separate samples of individuals who left unemployment for jobs in the years 1992, 1996, and 1998. From the database *AK-stat*, administered by the UI funds, I match on data on UI compensation type and previous income. These data are, however, only available for the years 1996 and 1998. For 1992, I use *Händel* data to establish whether a person is eligible for UI benefits or not.[12]

Lacking exact data on employment spells could cause bias calculating the days of work. For instance, if a person initiates an education spell after a few weeks of work, employment duration is upward biased. For that matter, I exclude the youngest age group (18-24), where students are over-represented. Another reason for excluding this group is that students tend to have short jobs in the summer, which are of less interest in this study.

Another potential source of bias is the fact that this study is limited to single composite periods of work. As stated earlier, the ER can be satisfied through one, single, work period and several shorter periods. A person starting an employment spell with 10 insured weeks only needs 6 more weeks to satisfy the ER in 1992. Because I only include one observation per person in each sample, I assume that individuals enter employment with no accumulated insured weeks. This leads to underestimation of the true time in employment.[13] Two arguments suggest that this problem is only minor. First, besides working, LMP-participation also entitles to benefits. Combining programmes and work to fulfil the ER is common behaviour among the repeatedly unemployed. This study focuses on the relationship between employer and employee, and restricting to composite periods seems justified because I can then distinguish between workers and programme participants. Second, Green & Sargent (1998) find that ER effects mainly occurs in seasonal industries. This suggests that workers, who take advantage of the system, work

[12] Thoursie (1997) found inconsistencies comparing those registered as UI receivers in *Händel*, and those actually collecting benefits in *AK-stat*. See also study 3 in this dissertation.

[13] If a person satisfies the sample criterion more than once in a year and thus has multiple employment spells, the included observation is randomly selected. I do so to avoid systematic differences in job duration within a particular year. It is plausible that employment spells initiated in the summer are shorter than jobs starting in other months.

the exact number of weeks in one, single spell — rather than in several shorter periods across the entire reference period. [14]

The analysis only includes those entitled to income-loss insurance in the unemployment period preceding the job spell. This further accentuates the focus on people who have earlier working experiences and the habit of “playing the system”. I also restrict to persons with Swedish citizenship. Finally, in contrast to Green & Sargent (1998), I have no explicit information about seasonal and non-seasonal jobs, only on regular and temporary jobs. However, since data are unreliable in this respect and jobs of various spells occur under both definitions, I choose to not distinguish between different types of registered jobs. Spells that did not end before May 31, 1999 are censored.

5.1 Identifying the initial week of eligibility

We must find out whether or not a person has worked long enough to fulfil the UI requirement. The working requirement in 1992 involved 75 days of work in 4 calendar months. Because 75 days (15 weeks) always includes 4 calendar months, all job spells of 75 days meet the ER. In 1996 and 1998, the required number of calendar months in which work must occur implies a variation in the ER. Initialising a spell early in the month calls for additional weeks of work when trying to reach the fifth (1996) or sixth (1998) month. Table 2 illustrates this. Register information about the start dates of the spells is available, so this variation is considered in the analysis. Note that in 1996, one day (three hours) of work in one month was enough to take that particular month into account when fulfilling the ER. The 45 hours/month requirement in 1998 creates a 4-week spread assuming that people work ordinary weeks (5 days, 40 hours). Depending on job type and industry, a person could fulfil the hours/month requirement in two days. Because the data lack exact information of the job spell, the hours/month specification in 1998 makes the identification of the first week of eligibility less reliable.

[14] When the reference period is determined, time when the applicant has been prevented from working due to: 1) certified illness, 2) military service, 3) labour market training, 4) vocational rehabilitation, or 4) training for which training allowance can be received, are excluded. So the reference period is generally longer than 12 calendar months.

Table 2: Initial week of eligibility 1992, 1996 and 1998 by day of start of employment

1992		1996		1998	
Start date (day)	*HMIN92=1 (Weeks)*	*Start date (day)*	*HMIN96=1 (Weeks)*	*Start date (day)*	*HMIN98=1 (Weeks)*
1-31	16	1-9	18	25-28	25
-	-	10-31	17	29-1	24
-	-	-	-	2-11	23
-	-	-	-	12-18	22
-	-	-	-	19-24	21

5.2 Sample characteristics

The original samples represent 40 per cent of the unemployment spells ending with the individual leaving for jobs in each year. All spells longer than 30 weeks and/or in progress as of May 31, 1999 are censored. Employment spells ending in ways other than unemployment are also censored. A favourable labour market situation thus implies a larger amount of censored spells. This is reflected in Table 3 where 1996, the year with highest aggregate unemployment, reports the lowest proportion of censored spells.[15]

The distribution of employment duration is clearly affected by the distance to the stop date in 1999. Disregarding the third quintile, the 1996 spells are generally shorter compared to the other years. This corresponds to the lower share of censored spells in 1996. The proportions of females, people living in big cities, individuals with university experience, and spells initiated in the summer months are all rather constant among the years.

[15] Aggregate unemployment was 4.8%, 8.1%, and 6.5% in 1992, 1996, and 1998, respectively.

Table 3: Sample characteristics 1992, 1996 and 1998

	1992	1996	1998
Number of spells	51,632	49,102	46,281
% Censored	52.5	43.9	53.3
% Female	44.9	46.5	46.1
Duration of employment spell (days):			
25% Quintile 1	84	63	71
50% Median	201	154	183
75% Quintile 3	602	370	293
Age (average)	36.5	37.6	38.0
% living in big cities	41.1	39.6	40.3
% experience of university	19.0	16.8	17.1
% spells initiated in June-August	30.7	35.2	37.5

Source: Own computations from the longitudinal data from the Swedish Labour Market Board.

6. Empirical framework

To study the job spells, I use the piece-wise constant exponential hazard model for each of the three samples.[16] The baseline hazard of this model is flexible and does not follow a specific distribution. Employment duration can enter through weekly dummies picking up the theoretically predicted spikes in the employment hazard. Assuming that several background factors have a multiplicative effect on the hazard rate, the general specification is:

$$\log \theta(t) = x'\gamma + \sum_{m=1}^{M} \beta_m d_m(t) \qquad (1)$$

where $\theta(t)$ is the employment hazard, x is a vector of explanatory variables with corresponding coefficient vector γ, d_m are indicators of the time interval (week) into which t falls, i.e., $d_m = 1$ if and only if t is in the m^{th} interval. β_m is a coefficient vector.

As x-variables, I use gender, age, educational level, desired profession, experience in desired profession, county type, month in which the spell begins, previous unemployment, re-

[16] See Lancaster (1990).

gional unemployment, and past earnings (from job previous to this).[17] Duration is entered through a step function with separate dummy variables for each of the first 30 weeks.

In 1992, there was no variation in the ER due to when in the month the job started. So a potential ER effect is captured by a dummy variable corresponding to the 16th week in the step function (β_{16}), which is the initial week of eligibility that year.

In 1996 and 1998, the situation is different. The variation in the first entitlement week (see Table 2) makes it possible to distinguish between the general flow back to unemployment, represented by the step function, and the specific consequence of the UI fulfilment. I do this by including (besides the step function) a separate time-varying variable that accounts for information about start date. In 1996, $HMIN_{y=96}^{r=96}$ (*y* refers to the particular year studied, and *r* denotes the year of the UI rule) then equals 1 in week 17 or 18, and zero otherwise. In the same way, $HMIN_{y=98}^{r=98}$ takes the value 1 a particular week between 21-25 and zero in all the others.

Extending equation 1 with these time-varying variables gives:

$$\log\theta(t) = x'\gamma + z(t)'\lambda + \sum_{m=1}^{M}\beta_m d_m(t) \qquad (2)$$

where the middle term corresponds to the vector of time-dependent dummies. Note that the time-invariant *x* covariates determine the hazard level for a given set of characteristics. The baseline hazard together with *z(t)*, in which the UI variables capture the ER effects, deals with the variation over time. The individual variation in the UI requirement in 1996 and 1998 thus helps to separate these two types of duration dependence.

6.1 Within-year testing of ER effects

Some factors suggest that finding an ER effect is more complex than restricted to a spike at $HMIN$. Due to the single-spell restriction in this study, those who initiate spells with insured weeks become eligible for benefits before $HMIN$. This makes the exit rate pattern in the weeks leading up to $HMIN$ hard to predict. Also, timing job exit to a certain week is difficult. Some people may even prefer timing their separation a few weeks above the $HMIN$ to insure against

[17] Information on regional unemployment consists of yearly averages of unemployment at the county level. Previous unemployment refers to the total time as openly unemployed until job start.

involuntary absence from work — illness, for example. Depending on the degree of risk aversion in the population, the hazard rate after $HMIN$ could exceed the exit rate at $HMIN$. Finally, a drop in the hazard immediately after UI fulfilment also indicates a behavioural effect. So an ER effect could show as an increase, or a drop, in the hazard rate at $HMIN$ or in the weeks surrounding $HMIN$.

To study the exit rate in the weeks around $HMIN$, I construct variables that correspond to the average of exit rates 3-5 and 1-2 weeks before the ER, and 1-2 and 3-5 weeks after the ER. In 1992, with no variation in the ER, this implies reconstructing the step function using aggregate dummies for the weeks 11-13 (β_{11}), 14-15 (β_{14}), 17-18 (β_{17}), and 19-21 (β_{19}). I use single dummy variables for the remaining weeks up to 30 weeks.

In 1996 and 1998, when variation in the ER is present, I specify separate time-varying variables that correspond to an average of $HMIN-(3-5)$, $HMIN-(1-2)$, $HMIN+(1-2)$ and $HMIN+(3-5)$ for each year. The step functions in 1996 and 1998 are specified as single dummy variables up to 30 weeks.

To summarise, the equation estimated for 1992 involves no *z(t)* variables. Instead, β_{16} captures the flow back to unemployment the first week of eligibility. To evaluate the differences in exit intensity the weeks around the week of eligibility, I test the hypotheses in Table 5.[18] An ER effect suggests that for a specific year at least one of these hypotheses is rejected. This corresponds to the above discussion concerning increasing and decreasing hazard rates around $HMIN$.

[18] A 1-degree-of-freedom Wald chi-square statistic is calculated by the following formula: $(b_1 - b_2)^2 / [s.e.(b_1)]^2 + [s.e.(b_2)]^2 - 2*(\text{cov}[b_1, b_2])$, where b_1 and b_2 are the β-estimates.

Table 4: Tests of the transition rates from job to unemployment between weeks in 1992, 1996 and 1998

Test	1992	1996	1998
$coeff\,(HMIN-(3-5)) =$ $coeff\,(HMIN-(1-2))$	$(\beta_{11})=(\beta_{14})$	$coeff\,(HMIN^{r=96}_{y=96}-(3-5)) =$ $coeff\,(HMIN^{r=96}_{y=96}-(1-2))$	$coeff\,(HMIN^{r=98}_{y=98}-(3-5)) =$ $coeff\,(HMIN^{r=98}_{y=98}-(1-2))$
$coeff\,(HMIN-(1-2)) =$ $coeff\,(HMIN)$	$(\beta_{14})=(\beta_{16})$	$coeff\,(HMIN^{r=96}_{y=96}-(1-2)) =$ $coeff\,(HMIN^{r=96}_{y=96})$	$coeff\,(HMIN^{r=98}_{y=98}-(1-2)) =$ $coeff\,(HMIN^{r=98}_{y=98})$
$coeff\,(HMIN) =$ $coeff\,(HMIN+(1-2))$	$(\beta_{16})=(\beta_{17})$	$coeff\,(HMIN^{r=96}_{y=96}) =$ $coeff\,(HMIN^{r=96}_{y=96}+(1-2))$	$coeff\,(HMIN^{r=98}_{y=98}) =$ $coeff\,(HMIN^{r=98}_{y=98}+(1-2))$
$coeff\,(HMIN+(1-2)) =$ $coeff\,(HMIN+(3-5))$	$(\beta_{17})=(\beta_{19})$	$coeff\,(HMIN^{r=96}_{y=96}+(1-2)) =$ $coeff\,(HMIN^{r=96}_{y=96}+(3-5))$	$coeff\,(HMIN^{r=98}_{y=98}+(1-2)) =$ $coeff\,(HMIN^{r=98}_{y=98}+(3-5))$

6.2 Across-year testing of ER effects

An ER effect dispersed over the weeks surrounding the week identified as the initial week of ER fulfilment could be difficult to identify since no drastic spike or drop in the hazard rate need to be present. Performing across-year estimations make possible to identify deviations in the hazard *levels* in the weeks of interest.

Pooling data from different years involves applying a common step function for both years and constant covariate coefficients. Besides the parameter specifying the ER for the particular year investigated, a year dummy captures the differences in the labour market situations between the years. To identify the ER effect, an interaction term taking the value one when returning to unemployment at the time of the ER the particular year of the ER, is specified.

To give an example, when analysing the job exits at the ER in 1992 using 1992 and 1996 data, i.e., testing $coeff\,(\,HMIN^{r=92}_{y=92}\,) \leq coeff\,(\,HMIN^{r=92}_{y=96}\,)$, I use the following information:

- $\beta_{16} \bullet d_{16}$, where d_{16} is a dummy variable that takes the value 1 the 16th week.
- $\gamma \bullet Year92$, where $Year92$ is a dummy variable that takes the value 1(0) if a person initiates a job in 1992 (1996).
- $\delta \bullet (d_{16} \bullet Year92)$ is an interaction term that takes the value 1 in week 16 in 1992, or zero otherwise.

If $\delta > 0$ the 1992 hazard is above the 1996 hazard at the 16th week. If $\delta < 0$ the opposite holds.

Similarly, comparing the exit rates at the time of the ER in 1996 correspondingly implies involvement of the 1996 ER. We then test $coeff(HMIN_{y=92}^{r=96}) \geq coeff(HMIN_{y=96}^{r=96})$. I use Wald´s test that is distributed as $\chi^2(1)$.

7. Results

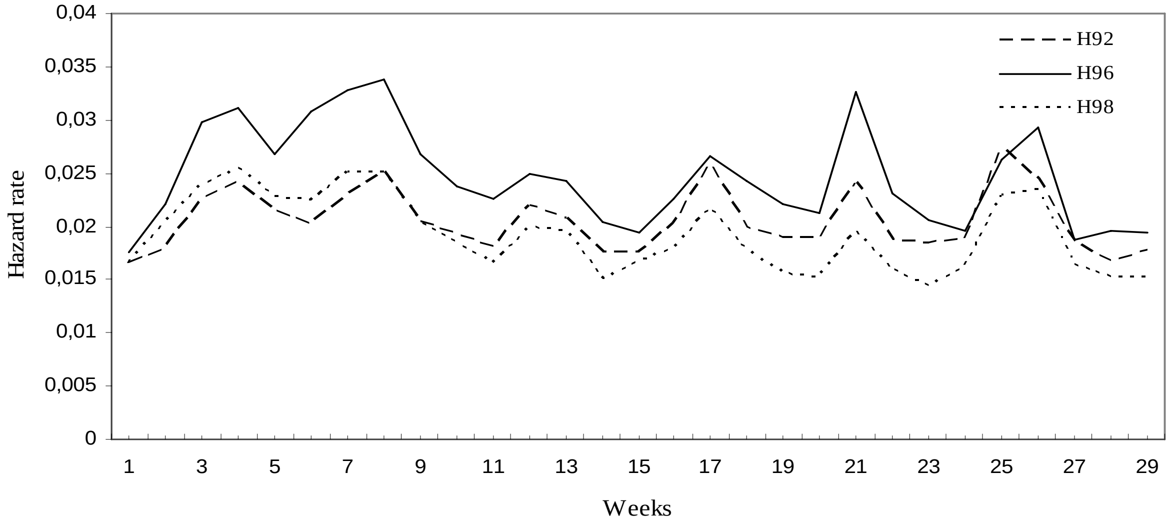
I proceed with the empirical analysis as follows. I begin by presenting the baseline rate of job-to-unemployment transitions and also the baseline rate of programme-to-unemployment transitions for each year in Section 7.1. They do not consider any individual, job, or labour market differences and thus provide only a benchmark for the completely specified model. Section 7.2 reports the covariate effects on survival in employment. In section 7.3 the UI related parameters are introduced and within-year analyses of job turnovers in the weeks surrounding UI fulfilment are performed. Section 7.4 reports between-year analyses in job turnover with different UI rules, and Section 7.5 restricts the same analysis to one sector (farmers), and one region (Norrbotten). Finally, Section 7.6 put the ER extension in 1997 into some perspective calculating the effect on average employment duration.

7.1 Baseline job-to-unemployment and LMP-to-unemployment hazards

The flexible specification of the baseline hazard allows for many spikes for different reasons. Spikes can occur due to seasonality in the labour market and local employment initiatives that provide many jobs of fixed duration. The simple baseline hazard does not distinguish between any of the potential sources of the spikes. Generally, an adjustment is apparent if the potential mass point corresponding to the UI condition moves from the old to the new minimum requirement.

The job hazards in Figure 3 are generally higher in the first few months, probably corresponding to the large number of temporary jobs in the summer. After the initial months, both the 1992 and the 1996 hazards show higher frequencies of job separation at 17 weeks, which are possible ER effects in those years. The same holds for the increase at the 21st and the 25th week in 1998. The time pattern is quite similar for all years. The ratio between the 17-week hazard and the 21-week hazard is 0.82 for 1996 and 1.1 for 1998.

Figure 3: Baseline transition rates from job to unemployment 1992, 1996 and 1998



An adjustment due to the latest change in the ER suggests a higher ratio for 1996 than for 1998. This creates doubts about how the increased exit rates at these particular weeks should be interpreted. It is quite possible that they are the result of something other than the ER. The spikes at 25-26 weeks could be caused by non-extended trial employments.[19]

Overall, the 1996 hazard is clearly above that of 1992 and 1998. This probably reflects the less favourable labour market situation. The null hypothesis that the survivor functions are the same for 1996 and 1998 (Figure 4), for all *t*, is rejected at the 1% significance level.[20]

Next, I explore UI fulfilment by participating in labour market programmes. Because all programmes entitle for benefits, we would expect adjustments in programme duration due to the change in the ER between 1992, 1996, and 1998. The samples consist of 9,149, 5,953 and 3,993 programme spells initiated in 1992, 1996, and 1998, respectively.[21] Labour market training was the dominating programme in 1992. In 1996 and 1998, work-experience and work-

[19] A trial employment is an employment where the firm after six months must decide whether to offer the employed a regular employment or not.

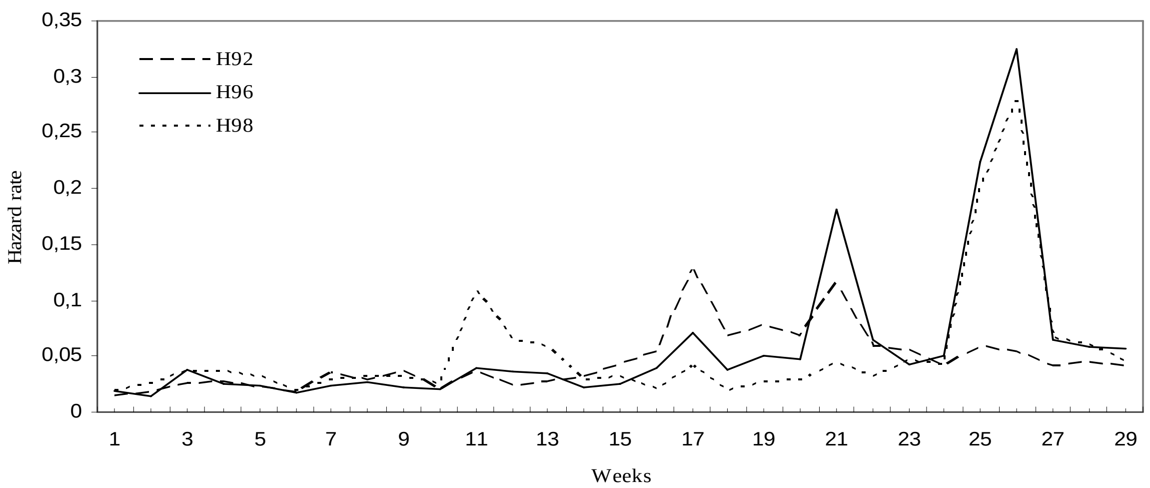
[20] The test performed is a log-rank test (Allison, 1995), the test statistic is distributed as $\chi^2(1)$ and takes a value of 605.

[21] Only one observation per individual is included in each sample. If a person has several different programme spells within the same year, the included observation is randomly selected. Multiple programme spells following each other are treated as one single observation. People not returning to unemployment after the spell are censored. See Table B1 in Appendix B for more details.

place-introduction programmes replaced the proportion in labour market training, which diminished.

Figure 4 plots the baseline hazards for labour market programmes. Comparing with job hazards, the patterns for 1992 and 1996 are rather similar from the 15th week and onward with spikes at 17, 21, and 25-26 weeks. The lower transition rates at earlier weeks correspond to reduced individual opportunity of variation in the duration in LMPs. The 1992 hazard grows slightly toward the 16th week and peaks at the 17th week due to exits from public temporary jobs and labour market training. The depicted 1996 hazard shows a similar pattern up to 17 weeks, but the largest departures occur at 21 and 26 weeks as a result of ended work-experience programmes. The large exit rates at 21 weeks in 1992 and 1996 show that LMPs in some cases are shorter than the regular 26 weeks, but that they still, with a few weeks margin, satisfy the ER. In 1998, when this no longer holds, the hazard is flat, which suggests an effect of the new UI rules. Apart from the large exits in computer/activity centre at 11-13 weeks, the 1998 hazard stays at a low level up to 26 weeks, which is around the latest ER.[22]

Figure 5: Baseline transition rates from LMPs to unemployment 1992, 1996 and 1998



[22] Note that by repeating participation in a computer/activity centre, a person could become eligible for benefits.

7.2 Job-to-unemployment transition rates using the model that includes covariates

Table 5 presents the estimates from the duration model for each year. The estimates give the effects of survival in employment. The results are generally rather intuitive. High education, big cities, previous well-paid jobs, on average, lead to longer working spells. Starting employment in January also increases the probability of relatively long spells. In contrast, these factors in general have a negative effect on job duration: age compared to the base group (25-34), certain job categories (manufacturing and mining, transport and communication, services, forestry, fishery and farming) and high local unemployment.

Table 5: Covariate effects using a piece-wise constant exponential specification. Estimated standard errors within parentheses

	1992		**1996**		**1998**	
Constant	6.613	***	7.059	***	7.089	***
	(0.070)		(0.073)		(0.078)	
Man	-0.136	***	-0.109	***	-0.087	***
	(0.017)		(0.018)		(0.021)	
Age						
25-34	-		-		-	
35-44	0.032	*	-0.080	***	-0.074	***
	(0.016)		(0.016)		(0.019)	
45-54	0.064	**	-0.116	***	-0.177	***
	(0.020)		(0.019)		(0.021)	
55-64	-0.041		-0.222	***	-0.276	***
	(0.031)		(0.027)		(0.029)	
County						
Big city[a]	-		-		-	
Local labour markets[b]	-0.146	***	-0.162	***	-0.138	***
	(0.023)		(0.019)		(0.021)	
Other	-0.003		-0.052	**	-0.003	***
	(0.017)		(0.017)		(0.019)	
Education						
<Upper secondary, 2 years	-		-		-	
Upper secondary, 2 years	-0.124	***	-0.088	***	0.003	
	(0.017)		(0.018)		(0.019)	
Upper secondary, 3-4 years	0.009		-0.002		0.099	***
	(0.025)		(0.025)		(0.027)	
University	0.172	***	0.191	***	0.306	***
	(0.026)		(0.028)		(0.032)	
Desired profession						
Technical, scientific, liberal arts, etc.	-		-		-	
Health and social work	0.123	***	0.025		0.213	***
	(0.030)		(0.030)		(0.034)	
Administrative work	-0.003		-0.017		0.163	***
	(0.031)		(0.032)		(0.035)	
Commercial work	0.007		-0.035		0.065	
	(0.034)		(0.036)		(0.040)	
Farming, forestry and fishery	-0.424	***	-0.383	***	-0.352	***
	(0.038)		(0.037)		(0.041)	
Manufacturing and mining	-0.412	***	-0.459	***	-0.388	***
	(0.027)		(0.029)		(0.032)	
Transport and communication	-0.237	***	-0.277	***	-0.214	***
	(0.034)		(0.037)		(0.042)	
Services	-0.216	***	-0.260	***	-0.103	**
	(0.032)		(0.034)		(0.038)	
Regional unemployment[c]	-0.002		-0.031	***	-0.043	***
	(0.009)		(0.005)		(0.006)	
Month in which spell began						
January	-		-		-	
February	-0.005		0.097	**	0.141	***
	(0.040)		(0.037)		(0.042)	

March	0.021		0.230	***	0.258	***
	(0.038)		(0.036)		(0.039)	
April	-0.078	*	0.186	***	0.115	**
	(0.036)		(0.032)		(0.035)	
May	-0.417	***	-0.269	***	-0.245	***
	(0.033)		(0.030)		(0.033)	
June	-0.716	***	-0.793	***	-0.724	***
	(0.032)		(0.028)		(0.031)	
July	-0.599	***	-0.716	***	-0.650	***
	(0.038)		(0.031)		(0.036)	
August	-0.168	***	0.073	*	0.173	***
	(0.034)		(0.031)		(0.032)	
September	-0.295	***	-0.206	***	-0.178	***
	(0.035)		(0.033)		(0.036)	
October	-0.318	***	-0.336	***	-0.285	***
	(0.037)		(0.036)		(0.040)	
November	-0.457	***	-0.429	***	-0.418	***
	(0.037)		(0.038)		(0.041)	
December	-0.366	***	-0.376	***	-0.306	***
	(0.043)		(0.046)		(0.055)	
Unemployment duration[d]	-2.4E-04	***	8.2E-05		1.7E-04	*
(previous to this spell)	(5.7E-		(5.7E-		(7.5E-	
Past earnings	~		1.0E-04	**	6.1E-05	
			(3.8E-		(3.8E-	
Experience						
No experience	-		-		-	
Some experience	-0.036		-0.032		-0.067	*
	(0.026)		(0.028)		(0.031)	
Long experience	0.092	***	0.054	*	0.030	
	(0.025)		(0.026)		(0.029)	
Log likelihood value	-130,269		-119,288		-99,029	
Number of observations	51,632		49,102		46,281	

Significance levels: *<0.05, **<0.01, ***<0.001. Notes, (~): No available information. [a]: Refers to the counties of Stockholm, Göteborg and Bohus (later Västra Götaland), and Malmöhus (later Skåne). [b]: Refers to the counties of Värmland, Kopparberg, Gävleborg, Västernorrland, Jämtland, Västerbotten, and Norrbotten. [c]: County-specific yearly averages of the unemployment rate. [d]: Refers to periods of open unemployment before job start.

7.3 Within-year estimations of ER effects

Tables 6 presents the weekly hazard estimates at, and around, the weeks of fulfilling the ER in 1992, 1996 and 1998.[23] Remember that the estimates surrounding the 1992 ER captures the general transition from employment to unemployment represented by the baseline hazard.[24]

[23] I only report the estimates of the UI-related parameters in the following. Please contact the author for the results of the full model estimations.

[24] The flow from employment to unemployment during the first week of employment constitutes comparison in this analysis.

Again, due to the variation, the estimates around the 1996 and 1998 ER extracts from other forms of duration dependence and thus more explicitly focuses on ER effects.[25]

To illustrate the estimated ER effects, I plot the hazards suggested by applying the estimates to a flat baseline of 0.020 for 1992, 0.024 for 1996, and 0.019 for 1998. These are the calculated hazard averages for the first 30 weeks in each year. In Figure 6a, studying the hazard around the ER in 1992, the hazard decreases toward the 16th week and increases significantly the following weeks. This suggests a late ER effect due to difficulty in timing job separation to a certain week.

Turning to the ER in 1996, Figure 6b depicts a small upward trend toward the weeks of UI fulfilment in 1996 — based on the UI-related effects from Table 6. Although the $HMIN_{y=96}^{r=96}$ estimate is significant, the rise is not significant compared to the preceding period. The significantly positive exit rates in the weeks leading up to the ER could have been caused by individuals entering the employment spell with insured weeks. However, the lack of spikes creates doubts as to whether or not an ER effect exists.

The hazard surrounding the weeks of the 1998 ER in 1998 drastically increases in the two-week period before satisfying the ER, and drops 3-5 weeks after. This suggests some adjustments in the timing of job exits due to the ER.

Overall, the within-year estimations are ambiguous as to the effect of the ER. Clear-cut spikes or drops in the weeks surrounding fulfilment are difficult to find. This could be due to the result of the ER effects being dispersed over several weeks due to problems identifying the initial week of benefit entitlement, and difficulties in timing job exits to a certain week.

[25] However, the model specification opens for a possible multicollinearity problem between the time-varying and the step function variables. Through larger standard errors, this could affect inferences of tests including these estimates.

Table 6: Baseline estimates around the 1992 ER, in 1992 and 1996

Variable	**1992**	**Wald´s test**	**1996**	**Wald´s test**	**1998**	**Wald´s test**
HMIN –(3-5) weeks	0.147*** (0.039)		0.353*** (0.068)		-0.376*** (0.065)	
		0.24		0.26		51.71***
HMIN –(1-2) weeks	0.130*** (0.043)		0.390*** (0.090)		0.141 (0.088)	
		2.72		0.78		1.72
$HMIN^{r=92}$ (β_{16})	0.050 (0.052)		0.453*** (0.098)		0.025 (0.110)	
		32.38***		1.43		0.21
HMIN +(1-2) weeks	0.321*** (0.042)		0.370*** (0.096)		0.066 (0.096)	
		23.76***		1.92		43.44***
HMIN +(3-5) weeks	0.149*** (0.041)		0.264*** (0.079)		-0.429 (0.085)	

Notes: No. of observations, 1992: 51,635; 1996: 49,102. (1) Base controls include the covariates in Table 5 and a step function in duration. (2) Standard errors are in parentheses. (3) Significant levels: *<0.10, **<0.05, ***<0.01. (4): Wald´s test is specified in note 22.

Figure 6a: Fitted hazard around the 1992 ER, in 1992

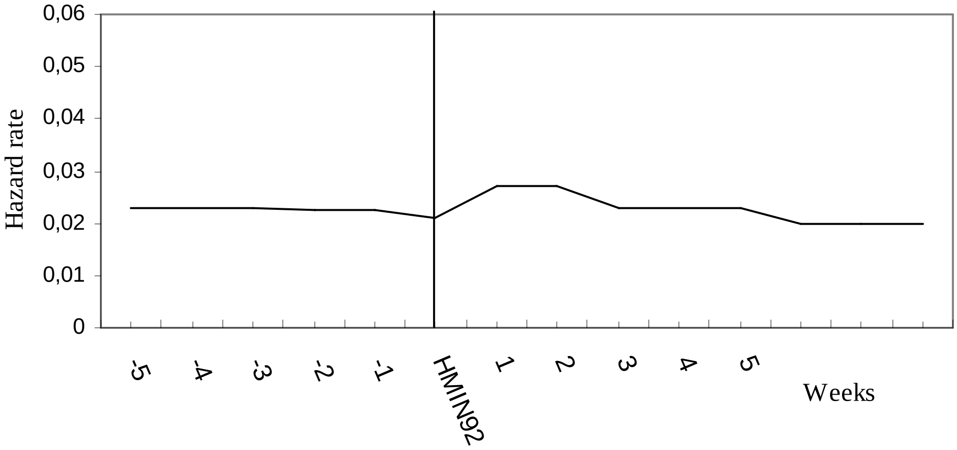


Figure 6b: Fitted hazard around the 1996 ER, in 1996

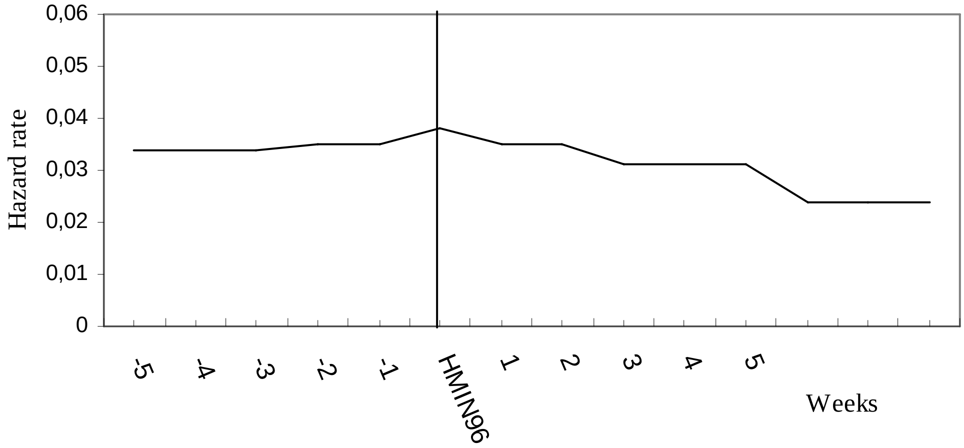
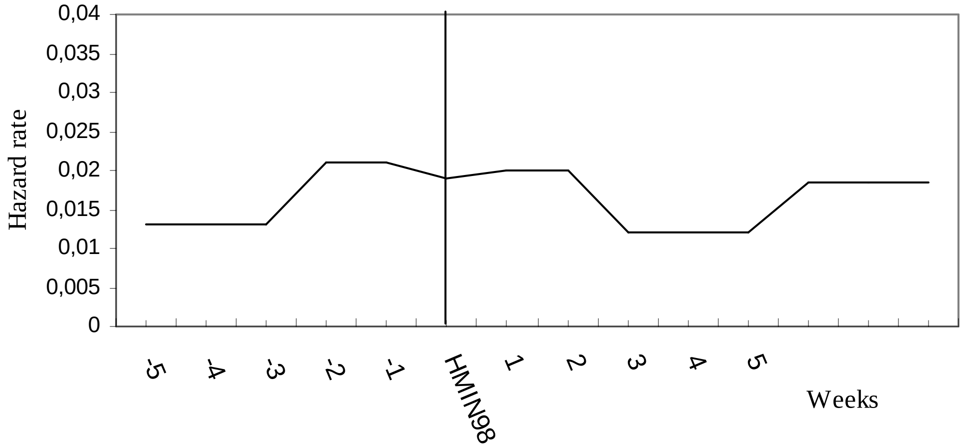


Figure 6c: Fitted hazard around the 1998 ER, in 1998



7.4 Across-year estimations of ER effects

The ambiguous results in the within-year analyses motivate further investigation of ER effects instead focusing on across-year comparisons of job exits for more reliable inference. I perform across-year analyses of all possible pair combinations of ERs (rows) and comparison years (columns), for both job turnovers at the exact week of satisfying the ER, and the following two weeks. The ER effect estimates are reported in Table 8. A positive impact estimate suggests a positive effect of the ER in the particular combination of years investigated. Note however that effects dispersed over the weeks surrounding the ER could have some implications comparing two years where the ER has been extended. For example, if the ER affects the job exits in the subsequent weeks, the effect of the most recent change among the two years compared is downward biased. With late adjustments, effects corresponding to the oldest ER should be easier to detect.

The results generally support the delayed effect mechanism. Studying the 1992 ER, both the 1996 and 1998 comparisons suggest late significant shifts away from the old rules. Examining the 1996 ER, no evidence of an ER effect is found comparing with the job exits in 1992. The point estimates even suggest a lower (!) job turnover rate in 1996. Note however that the ER in 1992 and 1996 only differ by 1-2 weeks. Instead comparing with the exits in 1998, a significant positive impact estimate is discovered, suggesting a 16 per cent higher exit rate in the weeks of the 1996 ER.

Table 8: Across-year estimations of ER effects at the week of ER fulfilment, and in the two week period following the week of ER fulfilment

ER rule ↓	1992		1996		1998	
	$HMIN_{y=92}$	$HMIN_{y=92}$ (1-2)	$HMIN_{y=96}$	$HMIN_{y=96}$ (1-2)	$HMIN_{y=98}$	$HMIN_{y=98}$ (1-2)
1992	-	-	0.082 (0.059)	0.114*** (0.038)	0.003 (0.061)	0.104** (0.041)
1996	-0.086 (0.052)	-0.030 (0.040)	-	-	0.161*** (0.059)	0.040 (0.043)
1998	0.081 (0.069)	0.187*** (0.048)	-0.155** (0.067)	0.074 (0.050)	-	-

Notes: No. of observations, 1992/96: 100,734; 1992/98: 97,913; 1996/98: 95,383. (1) Base controls include the covariates in Table 5 and a step function in duration. (2) Standard errors are in parentheses. (3) Significant levels: *<0.10, **<0.05, ***<0.01. (4): Wald´s test is specified in note 22.

Finally, the 1998 ER seems to have caused an adjustment of the timing of job exits compared to 1992, at least studying the two weeks following ER fulfilment. The results from the 1996 comparison are more ambiguous. The significantly negative effect at the first week of UI eligibility suggests that the impact estimate captures a delayed effect of the 1996 UI rule. The recovery in the subsequent two-week period provides some indications of a late ER adjustment corresponding to the new rules. The lack of a more pronounced effect is possibly the result of the design of the 1998 rules, earlier discussed in Section 5.1. The rule involves a higher degree of uncertainty establishing the first week of UI entitlement.

Summing up the across-year comparisons, we find evidence of an enhanced unemployment risk at the time of meeting the working requirement for all of the three UI regimes investigated. While the 1996 ER generates an increased hazard in the first week of UI fulfilment, the 1992 and 1998 rules involve a higher job exit rate in the subsequent two-week period.

7.5 Analysing the ER effects in one sector and in one region

Green & Sargent (1998) discovered substantial UI-related impacts on the job hazard for seasonal jobs. In the following, I narrow the analysis to one occupational group (farmers) and one local labour market (Norrbotten), both characterised by a relatively high degree of recurrent unemployment in the labour force according to Section 4.[26] I restrict the analysis to a comparison between 1996 and 1998. The small samples inevitably produce effect estimates with large standard errors.

Table 10: Across-year estimations of ER effects at the week of ER fulfilment in one sector (farmers), and one region (Norrbotten)

	Farmers				**Norrbotten**			
	1996		1998		1996		1998	
ER rule ↓	$HMIN_{y=96}$	$HMIN_{y=96}$ (1-2)	$HMIN_{y=98}$	$HMIN_{y=98}$ (1-2)	$HMIN_{y=96}$	$HMIN_{y=96}$ (1-2)	$HMIN_{y=98}$	$HMIN_{y=98}$ (1-2)
1996	-	-	0.234 (0.431)	0.730* (0.420)	-	-	0.855*** (0.294)	0.214 (0.177)
1998	0.105 (0.552)	0.200 (0.413)	-	-	-0.043 (0.241)	0.053 (0.205)	-	-

Notes: No. of observations, Farmers: 1,411; Norrbotten: 4,500. (1) Base controls include a step function in duration, gender, age, education, desired profession (only in the Norrbotten analysis), experience in desired profession, previous unemployment, county-specific UR (only in the farmer analysis), county type (only in the farmer analysis), and month of employment. (2) Standard errors are in parentheses. (3) Significant levels: *<0.10, **<0.05, ***<0.01. (4): Wald´s test is specified in note 22.

The results in Table 10 further underline the results of the main analysis. Distinct mass point shifts away from the 1996 ER in 1998 are identified for both farmers and UI receivers in Norrbotten. Adjustments in accordance to the new working requirement in 1998 are, similar to when analysing the full samples, difficult to establish. Although the small samples and large confidence intervals suggest a careful interpretation of the point estimates, it is interesting to note that the 1996 ER effects in the sub samples indicate impacts 7-8 times as large as the average impact. The results thus support the idea that adjustments of the ER primarily affect sectors where repeated unemployment is relatively common.

7.6 Measure of the size of the observed effects

To provide a measure of the size of the observed effects of the 1997 extension of the ER, I use a formula from Green & Riddell (1997) to estimate average employment duration using baseline and covariate estimates from the duration model where all covariates are set to their average values in each year. Average employment duration is calculated as,

$$E(emp) = \sum_{H=1}^{29} Hf(H) + \left[\prod_{H=1}^{29} (1 - h(H) \right] \left(29 + \frac{1}{h_{30}} \right), \qquad (3)$$

where, $f(H)$ is the density function for employment duration based on the fitted hazard estimates, H is week and h_{30} is the hazard rate for the 30th week in 1998. For weeks beyond 30, I assume a constant hazard equal to the hazard rate for this particular week.

Assuming a decreasing hazard, this may underestimate the actual average employment duration. To predict hazard values for each week, I also include the estimates of the UI-related variables. We already know that employment spells in general were longer in 1998 compared to 1996 from Figure 4. Using this specification, the average duration increased from 60.0 to 63.8 weeks. In evaluating the effects from the extension, we wish to control for across-year differences in baseline hazards and individual characteristics. I could then restrict to the immediate effects of the ER. To accomplish this, I replace the 1998 UI parameters, i.e., the parameters capturing fulfilment of the ER and the weeks surrounding ER fulfilment, by the 1996 UI parameters in the fitted hazard of 1998. The expected duration then drops from 63.8 to 60.9 weeks, creating a 2.9-week extension as a result of the altered ER. In the calculated extension, I

[26] Farmers are defined as individuals belonging to the farmers UI fund.

make a reservation for the difficulty in confirming the initial week of eligibility, especially for the weeks surrounding the 1998 ER.

8. Conclusions

I investigate the effect of the UI entrance requirement on employment duration on the Swedish labour market in 1992, 1996, and 1998. I do so by studying the behavioural adjustments in the timing of job exits due to the changes in the ER in 1994 and 1997. The study restricts to UI receivers, i.e., unemployed who have satisfied the ER at least once, and thus focuses on people with some working experience. It is important to be aware that an extension of the entrance requirement also has consequences on people who have not yet fulfilled the UI requirements a first time. By making the entry to the UI system more difficult, it is quite possible that expenditures in the social assistance system increase.

Studying each year separately, I find no evidence of adjustments due to the entrance requirement in terms of distinct mass points of job terminations at, or around, the week of fulfilment. Several possible explanations have been introduced here; the lack of exact data on employment duration, the concentration on single spells in the analysis, and the problems in timing the job exit to one particular week. However, instead analysing across-year differences in job turnovers, I find evidence of adjustment to the entrance requirement in all three years. By using predicted hazard rates for each week, I calculate an approximate 2.9-week extension in average employment duration between 1996 and 1998 due to the 5-week prolonging of the entrance requirement in 1997. Analysing the effects in one industry (farmers), and one region (Norrbotten), suggests that the ER extension primarily affected sectors where repeated unemployment, indicating seasonality in the production process, was relatively common.

In comparison with the Canadian studies, Green & Riddell (1997) concluded a 1.5-week extension between 1989 and 1990 due to a 4-week prolonging of the ER in high unemployment regions. Green & Sargent (1998) observed a small decrease in employment duration among seasonal jobs between 1989 and 1994 in regions of high unemployment. The decrease comes from a greater portion of very short jobs. According to theory, the ER has little effect on the choices to end jobs well before the minimum requirement. Because an extension implies more weeks unaffected by the ER, the increase of jobs of short duration may offset the potential mass-point extension at higher weeks. Their result is in contrast to the predictions in this study.

But similar to Green & Riddell, I examine only a short-term reaction. When people have fully adjusted to the new ER, the result may be different.

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Appendix A

Figure A1: Share of unemployment weeks for people who, at least twice in the years 1994-97, worked for 3-9 months (composite time) and were unemployed the remaining days of a 360-day period, by county.

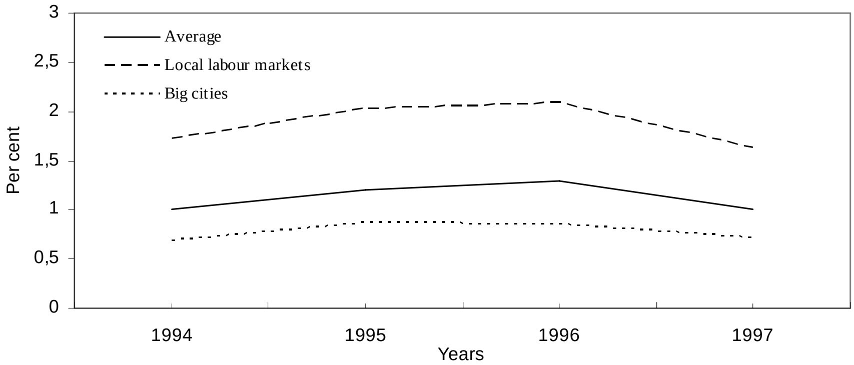
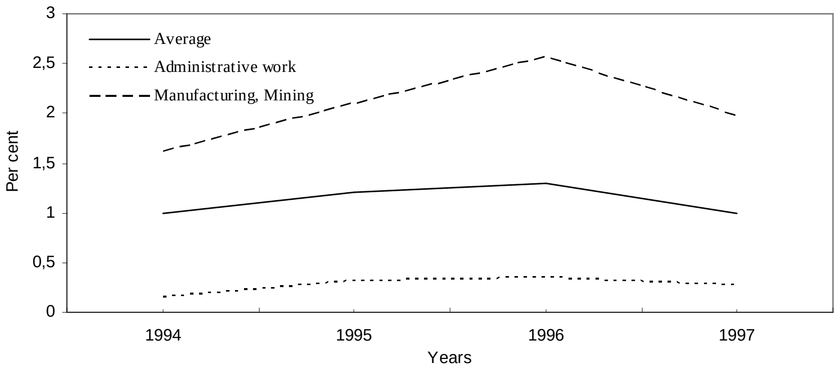


Figure A2: Share of unemployment weeks for people who, at least twice in the years 1994-97, worked for 3-9 months (composite time) and were unemployed the remaining days of a 360-day period, by job category.



Appendix B

Table B1: Types of LMPs and their share 1992, 1996 and 1998

LMP (%)	1992	1996	1998
Recruitment subsidy	*	5.2	0.0
Youth traineeship	*	0.0	7.5
Start your own business	*	4.7	1.0
Public temporary work	27.8	4.8	0.0
Work experience programme	1.1	40.3	39.5
Trainee in temporary replacement programme	6.2	4.1	0.0
Immigrant programme	#	#	1.6
Workplace introduction	*	13.1	11.6
Computer/activity centre	*	3.5	12.1
Labour market training	64.8	24.3	26.7

Source: 1992, 1996, and 1998 longitudinal data from the Swedish Labour Market Board. The samples include individuals registered as Swedish citizens that are between ages 25-65. The samples represent about 30% of the programme spells in 1992, 1996, and 1998. **Notes**: (*) From 1995, (#) The Workplace Introduction programme replaced the Immigrant programme in 1995.