Effects of Changes in the Unemployment Insurance Eligibility Requirements on Job Duration — Swedish Evidence*

By

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Abstract:

This paper investigates the impact of the unemployment insurance (UI) entrance requirement on employment duration in Sweden. I study employment spells in 1992, 1996, and 1998 to find behavioural adjustments in the timing of job separation. The results suggest that some adjustments have occurred. Comparisons between years with different UI requirements support the conclusion. By using predicted hazard rates for each week, I calculate an approximate 3-week extension in the average duration of employment spells between 1996 and 1998.

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 were 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 Glenday & Alam (1982), note that such a connection may exist on the employee side. Kesselman (1985) 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."[1] 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

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

weeks in 1990. Green & Riddell calculated a 1.5-week increase in the average duration of employment in regions 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 employment duration. I do so by examining adjustments in employment duration between years with different UI rules. Besides 1996 and 1998, 1992 is also analysed because it involves a third ER specification. I use a piece-wise constant exponential hazard model to establish differences in employment duration between the three years. From the database at the Swedish Labour Market Board (AMS), which contains longitudinal data, I have drawn samples of jobs started by unemployed workers.

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

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[2] Edebalk & Wadensjö (1978).

[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 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 estimates. The last section contains conclusions and final comments.

2. Unemployment benefit in Sweden

The Swedish unemployment benefit system contains two policies:

1. *Basic insurance,* whereby compensation is available for those who are *not* members of a UI fund and are age 20.[4]

2. *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.

In 1992, qualifying applicants received 90% of their daily earnings; in 1996, 75%; and in 1998, 80%.[5] 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.[6] 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.[7] For a second-time applicant, the re-qualifying condition applies. Participation in labour market training, vocational rehabilitation, education financed by training allowance and military service, also enable an applicant to qualify (besides working).

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.[8] 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-

[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] SFS 1989:331, SOU 1996:150 and SFS 1997:238 respectively.

[6] SFS 1987:226 for 1992 and 1996, SFS 1997:238 for 1998.

[7] SFS 1995:1636.

[8] SFS 1988:645.

month reference period.[9] 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.[10] 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.

3. Theoretical motivation

Christofides & McKenna (1996) present a model that includes the potential influence of the ER on workers' and firms' behaviours. The model presumes that quits and layoffs are behaviourally distinct. Both in Canada and Sweden, individuals who quit risk a waiting period before receiving benefits. So the UI system influences the pattern of quits and layoffs, and it is reasonable to suspect implicit contracts between workers and firms as the main source of ER effects.

In the following, I present a labour supply model introduced by Moffitt & Nicholson (1982) that outlines the effects of UI for workers. The model assumes that unemployment, or leisure, 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 is assumed to maximise utility, which is a function of total net income over the period and of leisure. I use a one-year time horizon in Figure 1 to put focus on seasonal unemployment, where work is concentrated to a limited period each year.

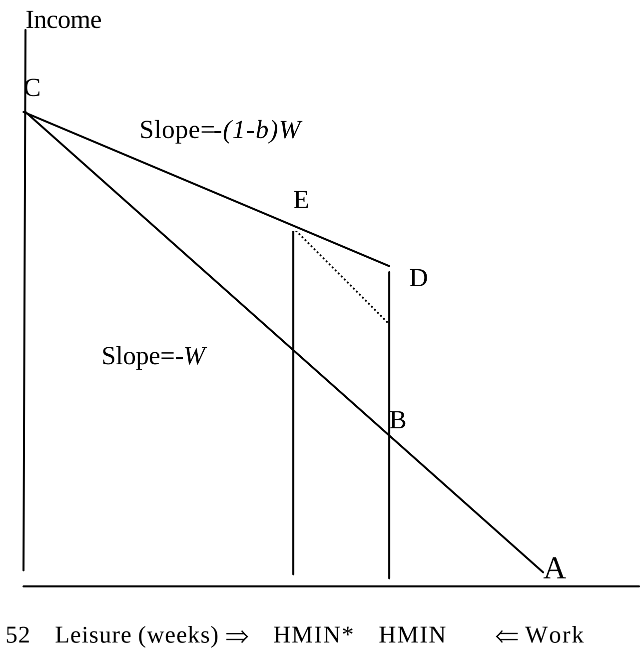
Figure 1 depicts the budget constraint for an unemployed person with UI benefits (*CDB*), and without (*CB*). $HMIN$ denotes the minimum number of weeks a person must work to become entitled for UI benefits. Two particular responses suggest job-termination clustering at $HMIN$:

[9] SFS 1994:1673, details about the temporary rule between July 1 1994 and January 1 1995 is not given here.

1. Many of those who, in the absence of UI, would work less than $HMIN$ and in the presence of UI, would want to work just enough to qualify for benefits. This primarily concerns those with spells a few weeks short of the ER and not people ending jobs well before. Seen from the right in Figure 1, reaching $HMIN$ involves an income effect that is equal to the distance between *B* and *D* due to the UI compensation.

2. In the presence of UI, people initially working beyond $HMIN$ will face 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 illustrated by the shift 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*0.8*W), where W denotes weekly wage and x 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 net outcome of the extension is hard to predict.

[10] SFS 1997:238.

4. The circular flow on the labour market

The purpose of this section is to focus on unemployment in Sweden between 1994-97, and to give the reader an idea of the magnitudes involved. I provide a measure of the circular flow on the labour market. Here, *circular flow* is defined as recurrent periods of unemployment and employment, or unemployment and labour market programmes (LMPs), during a short time period. The definition implies a close relationship to repeated seasonal unemployment. A high circular-flow rate suggests that the ER affects many workers. The extension encourages longer work spells in all types of jobs. But there is a possibility that the extension only has effects in industries and regions where repeated unemployment is common and where the stated minimum requirements affect people to a greater extent.

In Table 1, the first row shows 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 modest. But note that their unemployment spell, on average, is 6-8 weeks shorter compared to those receiving benefits in these years.

Table 1 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 3-5).[11] This is the work pattern that we would expect among seasonally unemployed, where jobs are concentrated to a certain period each year. Including repeated participation in LMPs, seasonal unemployment amounts to only 3.6-4.1% of aggregate unemployment. The relative strict definition of circular flow keeps the meas-

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

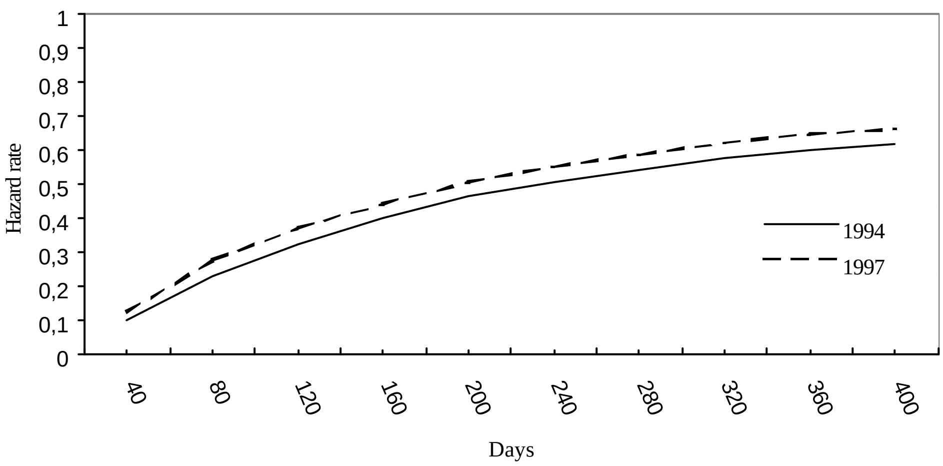
[11] Only start of the employment period is restricted to the particular calendar year. So the number of unemployment weeks in these rows only roughly refers to the particular calendar year.

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.[12]	3,417 (15.4%)	2,462 (11.2%)	1,756 (8.5%)	1,421 (7.6%)
2a) receiving UI compensation	2,081 (9.4%)	1,385 (6.3%)	952 (4.6%)	571 (3.0%)
- mean duration of unemployment spells (weeks).	21.7	20.6	18.5	17.1
2b) not receiving UI compensation	1,082 (4.9%)	934 (4.2%)	707 (3.4%)	826 (4.4%)
-mean duration of unemployment spells (weeks).	14.8	14.1	10.3	9.0
3) 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%)
4) 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%)
5) Total number of weeks of circular flow (3+4).	833 (3.8%)	900 (4.1%)	827 (4.0%)	665 (3.6%)
6) 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%)
-mean duration of unemployment spells (weeks).	14.3	13.6	12.5	10.9
7) 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%)
-mean duration of unemployment spells (weeks).	21.5	18.6	15.1	11.8

Source: Longitudinal data from the Swedish Labour Market Board. **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.

ures down. The low numbers also point to the fact that this could be a relatively small problem on aggregate. Rows 6 and 7 show the number of unemployment weeks derived from people entering jobs and LMP, respectively. The decrease in row 7 is mainly due to a 10-week drop in the average unemployment period preceding the programme start between 1994 and 1997. This, in turn, affects the corresponding spell of unemployment before entering a job (row 6), 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 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.[13]

Figure 2: Duration of employment before returning to unemployment in 1994 and 1997.



Source: Longitudinal data from The Swedish Labour Market Board (AMS). **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 18-65.

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

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

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

higher circular-flow level. [14] 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.

5. The data

I use data from the Swedish Labour Market Board (AMS) that consist of continuous information about every unemployment and programme spell of all people registered at the employment offices from August 1991 and onward. The database includes individual characteristics such as gender, age, education, sought-after occupation, training and experience in sought-after occupation, citizenship, community, and disability. Because data are updated once a day, the register contains information about the length of unemployment and programme spells. The cause of separation 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 1992, 1996, and 1998.

A person who gets a regular job is de-registered, and information regarding activities before a new unemployment period is not available. In this study, the central assumption is that the intermediate spell for those unemployed who are de-registered as getting a job, consists of a continuous work period. This need not be the case. In practice, such a de-registered person can initiate an education spell after a few weeks of work and consequently create an upward bias in the calculated days of work. So the important distinction to make is that data contain no information about the exact length of the actual employment spells but rather the duration between the end of one unemployment period and the start of another. On the opposite end, those who get a temporary job are not de-registered. So for such work, a more reliable meas-

[14] Local labour market refers to the forest counties: Värmland, Kopparberg, Gävleborg, Västernorrland, Jämtland, Västerbotten, and Norrbotten.

ure of work is available. To reduce bias associated with deregistration, I exclude the youngest age group (18-24), where students are over-represented. Another reason for not including this group is that jobs among students tend to be short jobs in the summer, which is of less interest in this study.

The precision of the measure of duration obviously depends on how accurate the entered data are. There is always a risk for errors in the reported dates. I consider the issue in Section 7.

In contrast to Green & Sargent (1998), I have no explicit information about seasonal and non-seasonal jobs. So I choose to not distinguish between different job types because spells of various duration could appear in regular and in temporary jobs. Spells that did not end before May 31, 1999 are censored.

As stated earlier, the ER can be satisfied through one, single, work period and several shorter periods. This study is limited to single composite periods of work. Hence, only one observation per individual is included in each sample. Two arguments work in favour of this restriction. First, besides working, participating in LMPs also entitles applicants for 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) conclude that adjustment to the ER almost exclusively occurs in seasonal industries. This suggests that workers, who take advantage of the system, work the exact number of weeks in one, single spell — rather than in several shorter periods across the entire reference period. [15]

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 initi-

[15] When the reference period is determined, one does not include 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. So the reference period is generally longer than 12 calendar months.

ated in the summer are shorter than jobs starting in other months. I exclude people with no UI during the unemployment period that precedes the job spell. This restriction is necessary because of differing working requirements for basic insurance and income-loss insurance recipients in 1996. This further accentuates the focus on people who have earlier working experiences and the habit of "playing the system". For this reason, I also restrict to persons with Swedish citizenship. Finally, the choice of using inflow of new employment spells excludes the problems involving left censoring.

Identifying the initial week of eligibility

We must find out whether or not a person has worked long enough to fulfil the UI requirement. This information is central in order to determine the first week of eligibility. 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 3 illustrates this. Accurate 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.

Furthermore, people enter the employment spells with different numbers of insured weeks. A person starting a 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.

Table 3: 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

Sample characteristics

The original samples represent 40% 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 4 by comparing aggregate unemployment with the share of censored spells in each year. In 1992 and 1998, more than 50% of the spells are censored. In 1998, the stop date, to a greater extent, is the reason for censoring.

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.

Table 4: 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	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: 1992, 1996 and 1998 longitudinal data from the Swedish Labour Market Board (AMS). **Notes**: (1) Standard errors are in parentheses. (2) Aggregate unemployment was 4.8%, 8.1%, and 6.5% in 1992, 1996, and 1998, respectively.

6. Empirical framework

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

$$\log q(t) = x'g + \sum_{m=1}^{M} b_m d_m(t) \qquad (1)$$

where $q(t)$ is the employment hazard, x is a vector of explanatory variables with corresponding coefficient vector g, $\{d_m\}$ 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. b_m is a vector of coefficients.

As x variables, I use gender, age, educational level, sought-after occupation, experience in sought-after occupation, county, month in which the spell begins, local unemployment, and past earnings (from job previous to this). 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 (b_{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 3) makes it possible to distinguish between the general effect of the 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:

$$\log q(t) = x'g + z(t)'l + \sum_{m=1}^{M} b_m d_m(t) \qquad (2)$$

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 in separating these two types of duration dependence.

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, individuals that initiate spells with insured weeks become eligible 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 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 $HMIN$.

[16] Lancaster (1990).

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, this implies reconstructing the step function using aggregate dummies for the weeks 11-13 (b_{11}), 14-15 (b_{14}), 17-18 (b_{17}), and 19-21 (b_{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, b_{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.[17]

Table 5: Tests of the transition rates from job to unemployment between weeks in 1992, 1996 and 1998.

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

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

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 $HMIN$.

So far, I have described a procedure that relies on information within a particular year. But studying one particular year may not be enough to conclude an ER effect. This becomes apparent if an ER effect is dispersed across the weeks surrounding $HMIN$. Comparing transition rates from employment to unemployment between years with different rules then becomes useful. For instance, large departures from jobs 1-2 weeks before (or after) the $HMIN$ in 1992, not found in 1996, is evidence of an ER effect in 1992. I perform comparisons between years by applying the unique model specification of one year — on the data from a different year. To give an example, comparing 1992 and 1996 at the time of the ER according to the rules in 1992 means imposing the 1992 model specification on the 1996 data. To evaluate the difference in exit rates in the 16th week between the years, I perform a pooled regression — pooling the data from 1992 and 1996. This is necessary because the estimate of b_{16} only captures the general flow out from jobs this week. If the hazards (due, for example, to different labour market situations) differ in the overall exit levels, a between-year test becomes misleading. Pooling the data, a year dummy captures the difference in labour market situations between the years.

In the pooled regression, I use information from these estimates:

- $b_{16} * d_{16}$ where d_{16} is a dummy variable that takes the value 1 the 16th week.
- $g * Year92$ where $Year92$ is a dummy variable that takes the value 1(0) if a person initiates a job in 1992 (1996).
- $d * (d_{16} * Year92)$ is an interaction term that takes the value 1 in week 16 in 1992.

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

Comparing the exit rates at the time of the ER in 1996 correspondingly implies involvement of the 1996 model in the 1992 data. Because $HMIN_{y=92}^{r=96}$ and $HMIN_{y=96}^{r=96}$ are separated from the step function in each year, the estimates are comparable, and a pooled regression is unnecessary. I thus test $coeff\ (HMIN_{y=92}^{r=96}) \geq coeff\ (HMIN_{y=96}^{r=96})$.[18]

I proceed with the empirical analysis in two steps. 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. They do not consider any individual, job, or labour market differences and thus provide only a benchmark for the completely specified model. Thereafter, the fully specified duration model for jobs is estimated for each year. It takes into account individual characteristics, and also the separate set of dummy variables that represent UI fulfilment. Behavioural effects derived from the ER each particular year are studied as are adjustments between years due to the changed requirements. First I compare 1992 to 1996 and then 1996 to 1998.

7. Results

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.

Baseline hazards from job to unemployment and from LMP to unemployment

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

[18] I use Wald´s test that is distributed as $c^2(1)$.

hazard and the 21-week hazard is 0.82 for 1996 and 1.1 for 1998. An adjustment due to the latest change in the ER suggests a higher

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

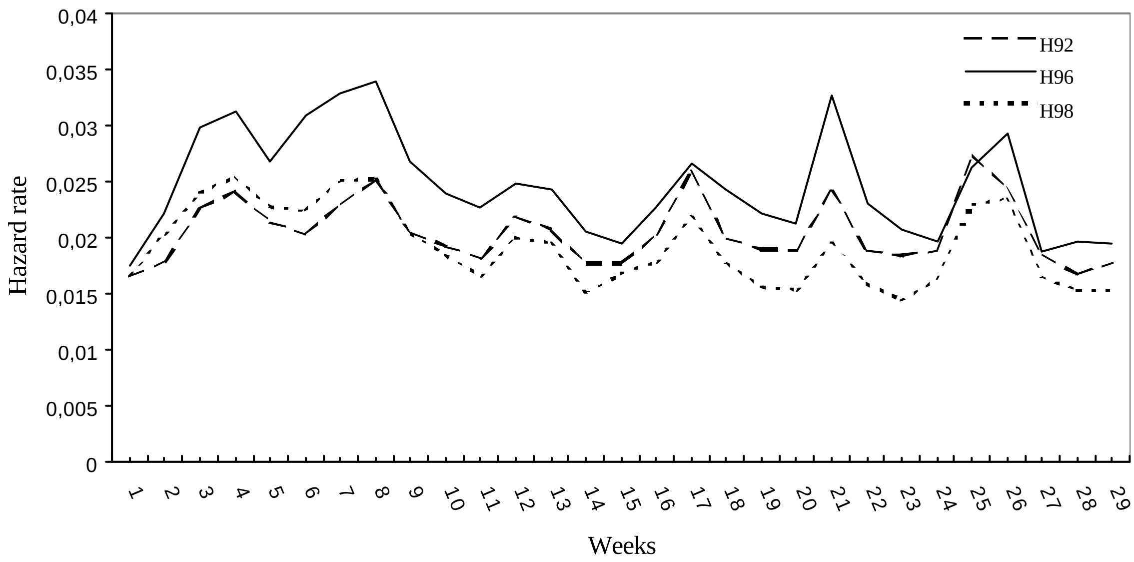
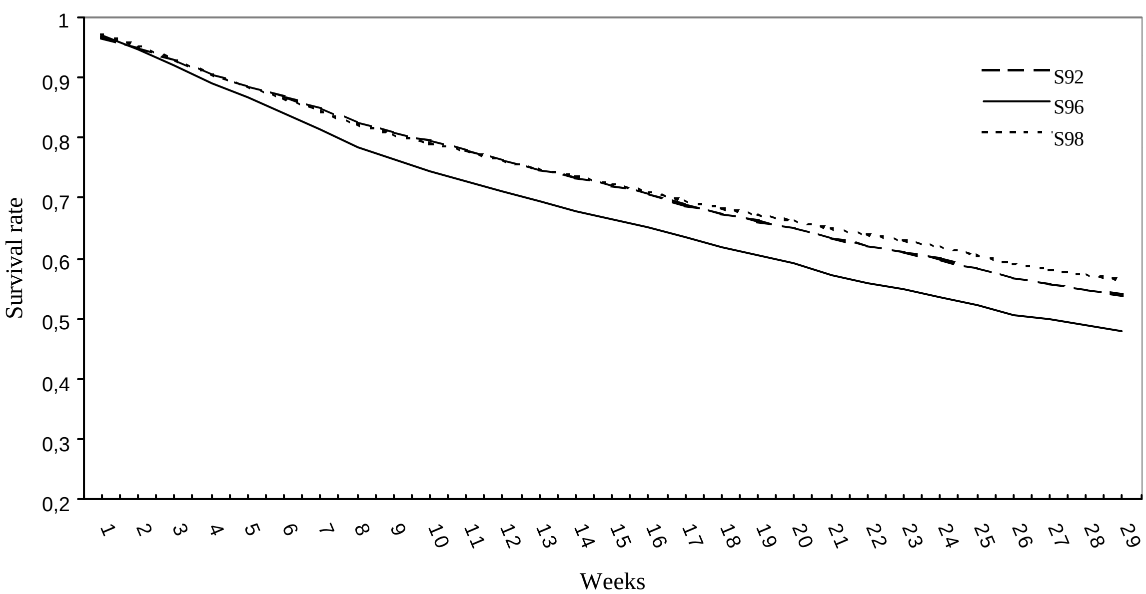


Figure 4: Survival rates of employment before returning to unemployment in 1992, 1996 and 1998.



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 is probably the result of a 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 workplace-introduction programmes replaced the proportion in labour market training, which diminished.

Figure 5 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

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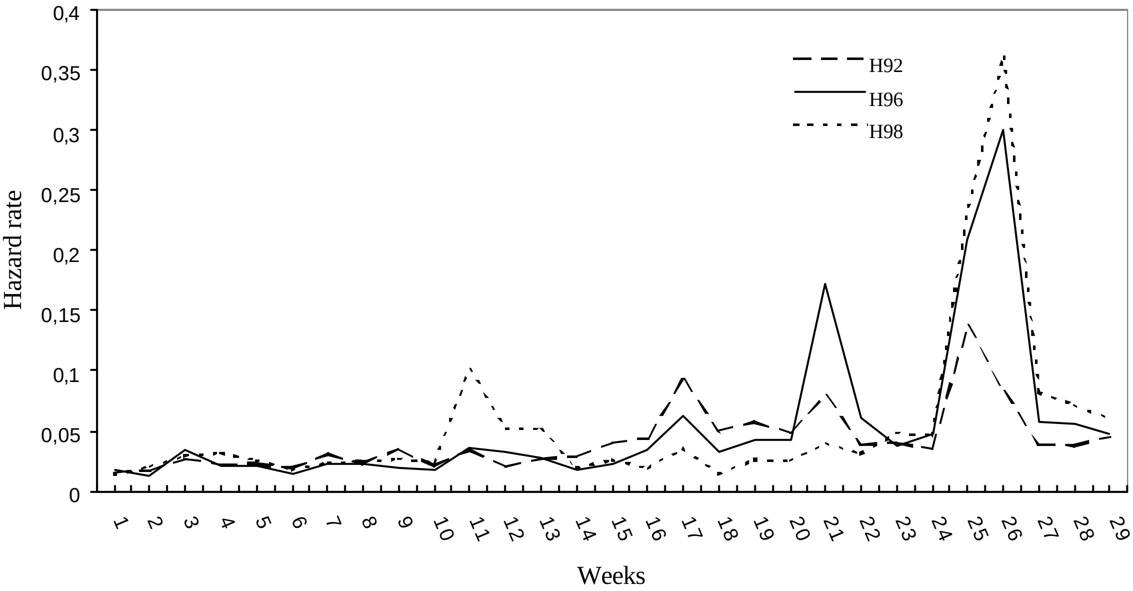
[19] A trial employment is an employment where the firm after six months must decide whether to offer the employed a regular employment or not.

[20] The test performed is a log-rank test (Allison), the test statistic is distributed as $c^2(1)$ 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.

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.



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

Table 6 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.

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

Table 6: 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	0		0		0	
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[23]	0		0		0	
Local labour markets	-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	0		0		0	
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.	0		0		0	
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	**

[23] Big cities: Stockholm, Göteborg and Malmö.

	(0.032)		(0.034)		(0.038)	
Regional unemployment	-0.002		-0.031	***	-0.043	***
	(0.009)		(0.005)		(0.006)	
Month in which spell began						
January	0		0		0	
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	-2.4E-04	***	8.2E-05		1.7E-04	*
(previous to this spell)	(5.7E-05)		(5.7E-05)		(7.5E-05)	
Past earnings	~		1.0E-04	**	6.1E-05	
			(3.8E-05)		(3.8E-05)	
Experience						
No experience	0		0		0	
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. Note (~): No available information.**

Effects of the ER on job duration ¾ a comparison of 1992 to 1996

Tables 7a and 7b present the estimates for the $HMIN:s$ in 1992 and 1996 and the weeks surrounding them in each year.[24] Remember that the estimates surrounding the 1992 ER in Table 7a, only captures the general outflow from employment represented by the baseline hazard. Due to the variation in 1996, the estimates around the 1996 ER in Table 7b extracts from other forms of duration dependence and thus focuses on the potential ER effects.[25]

To illustrate the estimated ER effects, I plot the hazard rate functions suggested by applying the estimates to a flat baseline of 0.020 for 1992 and 0.024 for 1996. 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 (Table 7a), 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. But the hazard corresponding to the same weeks in 1996 (Figure 6b) shows the same pattern, which creates doubts about the reason for this increase. Turning to the ER in 1996, Figure 7b depicts a small upward trend toward the weeks of UI fulfilment in 1996 — based on the UI-related effects from Table 7b. Although the $HMIN_{y=96}^{r=96}$ estimate is significant, the rise is not significant compared to the preceding period. The lack of spikes in the 1996 hazard implies that no ER effect is present. But the high exit rates before the ER could have been caused by individuals entering the employment spell with insured weeks. In 1992 (Figure 7a), the exit rate is almost constant. The small difference in the ER rules between 1992 and 1996 helps explain the similar hazard patterns between the years.

Across-year comparisons are necessary for more reliable inference. They can help in establishing ER effects in cases when the results from within-year studies are ambiguous. Performing a pooled regression, the H_0 hypothesis $coeff\,(HMIN_{y=92}^{r=92}) \leq coeff\,(HMIN_{y=96}^{r=92})$ is not

[24] The model estimates originally signalled the effects of survival in employment. A negative estimate then implied shorter duration and thus a higher exit rate. In the following I reverse the signs. The estimates now indicate the effects on the hazard rate.

[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 infereces of tests including these estimates.

rejected.[26] So there is no evidence of any behavioural adjustment at the 16th week due to the change between 1992 and 1996. And since the within year results are ambiguous, there is no evidence of an ER effect in 1992

In Figure 8, I have depicted the hazard rate patterns from Figures 7a-b, the weeks surrounding the 1996 ER, by applying the flat baseline from 1996 of 0.024. I do so to simplify a direct comparison between the estimates across years. The 1996 hazard is above that of 1992, and the test $coeff\,(HMIN_{y=92}^{r=96}) \geq coeff\,(HMIN_{y=96}^{r=96})$ is rejected at the 5% level of significance, which suggests an adjustment to the 1996 rules.[27]

To conclude, the results do not support an ER effect in 1992. But the difference in exit rates between 1992 and 1996 in the weeks surrounding the 1996 ER implies an ER effect in 1996.

Table 7a: Baseline estimates around the 1992 ER, in 1992 and 1996.

Variable	**1992**	**Wald´s test**	**1996**	**Wald´s test**
HMIN –(3-5) weeks	0.147***		0.603***	
	(0.039)	0.24	(0.043)	1.59
HMIN –(1-2) weeks	0.130***		0.560***	
	(0.043)	2.72	(0.046)	7.11**
$HMIN^{r=92}$ (b_{16})	0.050		0.428***	
	(0.052)	32.38***	(0.056)	23.49***
HMIN +(1-2) weeks	0.321***		0.668***	
	(0.042)	23.76***	(0.046)	4.65*
HMIN +(3-5) weeks	0.149***		0.589***	
	(0.041)		(0.045)	

Source: 1992 and 1996 longitudinal data from the Swedish Labour Market Board (AMS). **Notes**: (1) Base controls include a step function in duration, gender, age, education, sought-after occupation, experience in sought-after occupation, unemployment duration, provincial UR, provincial type 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 17.

Table 7b: Estimates of UI-related effects around the 1996 ER, in 1992 and 1996.

[26] The test statistic is distributed as $c^2(1)$ and takes the value of 0.02.

[27] The test statistic is distributed as $c^2(1)$ and takes the value of 5.85.

Variable	1992	Wald´s test	1996	Wald´s test
HMIN –(3-5) weeks	0.100		0.353***	
	(0.069)	0.09	(0.068)	0.26
HMIN –(1-2) weeks	0.079		0.390***	
	(0.089)	0.39	(0.090)	0.78
$HMIN^{r=96}$	0.123		0.453***	
	(0.095)	0.05	(0.098)	1.43
HMIN +(1-2) weeks	0.109		0.370***	
	(0.092)	0.00	(0.096)	1.92
HMIN +(3-5) weeks	0.107		0.264***	
	(0.076)		(0.079)	

Source: 1992 and 1996 longitudinal data from the Swedish Labour Market Board (AMS). **Notes**: (1) Base controls include a step function in duration, gender, age, education, sought-after occupation, experience in sought-after occupation, unemployment duration, provincial UR, provincial type 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 17.

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

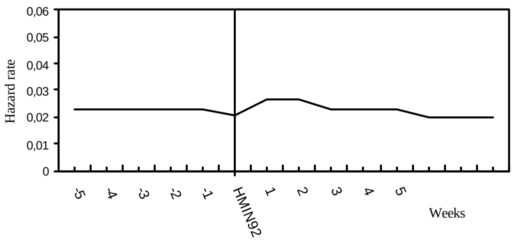


Figure 6b: Fitted hazard around the 1992 ER, in 1996.

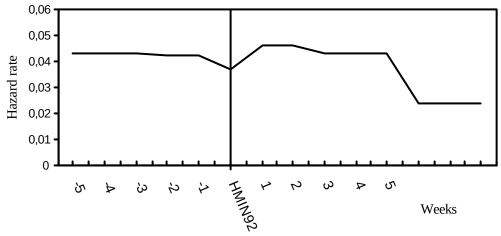


Figure 7a: Fitted hazard around the 1996 ER, in 1992.

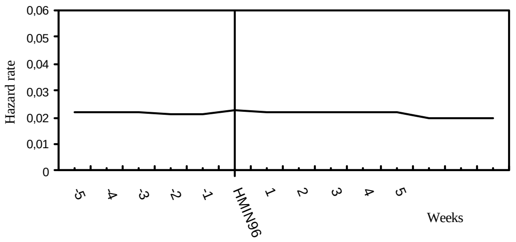


Figure 7b: Fitted hazard around the 1996 ER, in 1996.

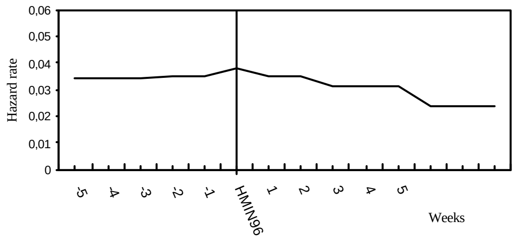
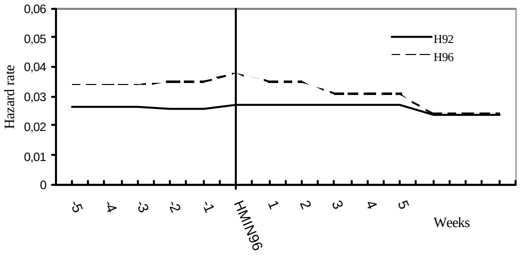


Figure 8: Fitted hazards around the 1996 ER, in 1992 and 1996.



Effects of the ER on job duration ¾ a comparison of 1996 to 1998

Tables 8a and 8b give the estimates of the time-varying variables for the weeks surrounding the ER in 1996 and 1998, respectively. I repeat the estimates around the 1996 ER from Table 7b to make comparisons easier. Figures 9a-b and 10a-b depict the hazard rates using horizontal baselines at 0.024 and 0.019. These are the calculated hazard averages for the first 30 weeks in 1996 and 1998 respectively. Figure 9b shows that the 1998 hazard decreases toward the 1996 ER in contrast to the 1996 hazard (Figure 9a). For the weeks surrounding the 1998 ER, portrayed in Figures 10a and 10b, the patterns are somewhat difficult to interpret. The significant increase in the hazard 1-2 weeks before the ER in 1998 (Figure 10b) is also present in the 1996 hazard (Figure 10a). One explanation could be that these weeks coincides with the weeks following the 1996 ER in 1996. The difference in patterns 1-2 weeks following the 1998 ER is interesting. While the 1996 hazard drops significantly, the 1998 hazard grows slightly, perhaps as an effect of the new ER. Once again, studying each year separately, there is no evidence of an ER effect.

In Figures 11a-b, I plot the hazard rate patterns from Figures 9a-b and 10a-b by applying the baseline average of 0.024 from 1996 and 0.019 from 1998 respectively. In 11a, the across-year difference at the 1996 ER indicates an adjustment due to the ER extension. The test $coeff\,(HMIN^{r=96}_{y=96}) \leq coeff\,(HMIN^{r=96}_{y=98})$ is rejected consistently.[28]

[28] The test statistic is distributed as $c^2(1)$ and takes the value of 3.92.

Table 8a: Estimates of UI-related effects around the 1996 ER, in 1996 and 1998

Variable	1996	Wald´s test	1998	Wald´s test
HMIN –(3-5) weeks	0.353***		0.336***	
	(0.068)	0.26	(0.081)	0.39
HMIN –(1-2) weeks	0.390***		0.278**	
	(0.090)	0.78	(0.113)	2.30
$HMIN^{r=96}$	0.453***		0.137	
	(0.098)	1.43	(0.126)	26.07***
HMIN +(1-2) weeks	0.370***		0.541***	
	(0.096)	1.92	(0.128)	1.37
HMIN +(3-5) weeks	0.264***		0.660***	
	(0.079)		(0.118)	

Source: 1992 and 1996 longitudinal data from the Swedish Labour Market Board (AMS). **Notes**: (1) Base controls include a step function in duration, gender, age, education, past earnings, sought-after occupation, experience in sought-after occupation, unemployment duration, provincial UR, provincial type 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 17.

Table 8b: Estimates of UI-related effects around the 1998 ER, in 1996 and 1998

Variable	1996	Wald´s test	1998	Wald´s test
HMIN –(3-5) weeks	-0.197***		-0.376***	
	(0.050)	29.56***	(0.065)	51.71***
HMIN –(1-2) weeks	0.080		0.141	
	(0.064)	22.75***	(0.088)	1.72
$HMIN^{r=98}$	-0.210***		0.025	
	(0.080)	7.96***	(0.110)	0.21
HMIN +(1-2) weeks	-0.398***		0.066	
	(0.071)	33.00***	(0.096)	43.44***
MIN +(3-5) weeks	-0.061		-0.429	
	(0.060)		(0.085)	

Source: 1992 and 1996 longitudinal data from the Swedish Labour Market Board (AMS). **Notes**: (1) Base controls include a step function in duration, gender, age, education, past earnings, sought-after occupation, experience in sought-after occupation, unemployment duration, provincial UR, provincial type 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 17.

In Figure 11b, the hazards surrounding the 1998 ER are portrayed. The already high hazard rate two weeks before the 1998 ER for 1996 is even more pronounced for 1998. Again, this could be the result of earlier insured weeks, but also remember that the 1998 ER was specified in hours, which make the actual variation in the ER even greater than in 1996. This, in turn, influences the accuracy in the first week of entitlement specified in Table 3. Comparing the job exits between 1996 and 1998 around the 1998 ER suggests a rejecting outcome at the 10% significance level.[29]

The shift away from the 1996 ER at 17-18 weeks confirms an ER effect in 1996. Also, the adjustment to the new rule in 1998 supports an effect in 1998.

Figure 9a: Fitted hazard around the 1996 ER, in 1996 (also Figure 6b).

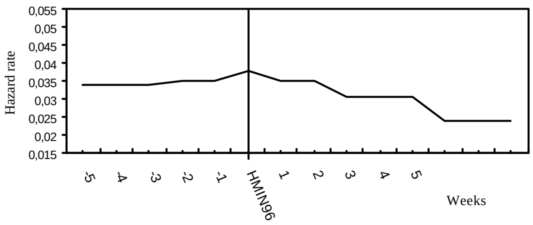


Figure 9b: Fitted hazard around the 1996 ER, in 1998.

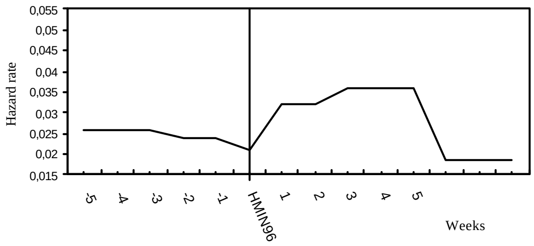


Figure 10a: Fitted hazard around the 1998 ER, in 1996.

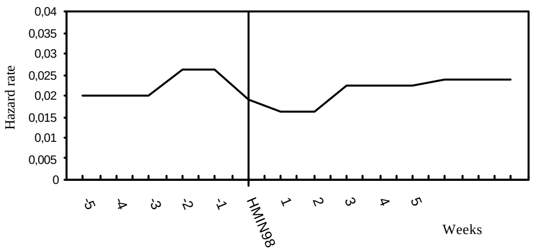


Figure 10b: Fitted hazard around the 1998 ER, in 1998.

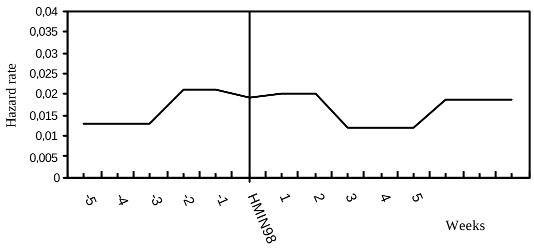


Figure 11a: Fitted hazards around the 1996 ER, in 1996 and 1998.

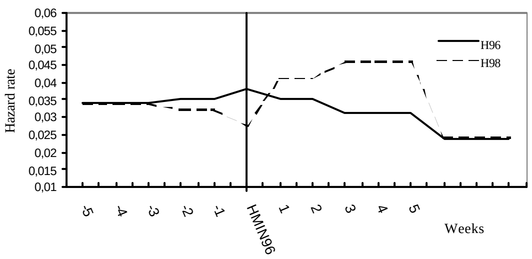
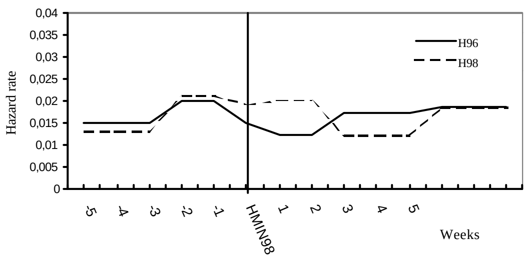


Figure 11b: Fitted hazards around the 1998 ER, in 1996 and 1998.



Green & Sargent (1998) discovered substantial UI-related impacts on the job hazard for seasonal jobs. In the following, I focus on one occupational group (farmers) and one local

[29] The test statistic is distributed as $\chi^2(1)$ and takes the value of 2.99.

labour market (Norrbotten). In Section 4, both are identified as sectors with a high degree of circular flow. I restrict the analysis to a comparison between 1996 and 1998. Estimates (within and between year tests) are in Tables B2a-b and B3a-b in Appendix B.

In Figures 12a-b, I plot the job spell hazards for individuals belonging to the UI farmers' fund in 1996 and 1998, respectively. In 12a, which portrays the behaviour around the 1996 ER, there is a large increase 1-2 weeks preceding the ER in 1996. Because the 1998 hazard drops, an adjustment is obvious and despite relatively few observations, statistical significance is confirmed. In Figure 12b, the 1998 hazard grows continuously between $HMIN^{r=98}_{y=98} - (1-2)$ and $HMIN^{r=98}_{y=98} + (1-2)$ while the exit rate in 1996 is constant. Although no statistical significance is established, the results indicates a possible behavioural adjustment due to the new ER rule.

Figure 12a: Fitted hazards around the 1996 ER, in 1996 and 1998 for farmers.

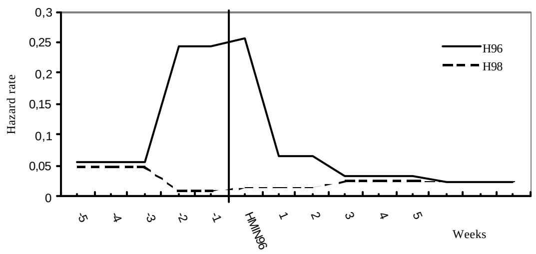
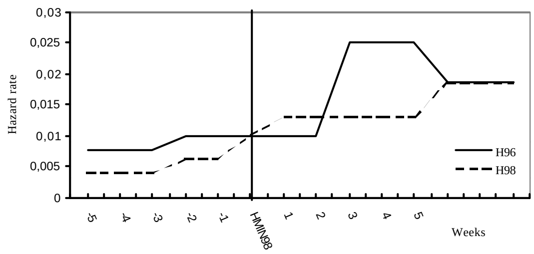


Figure 12b: Fitted hazards around the 1998 ER, in 1996 and 1998 for farmers.



In Figures 13a-b, I make a similar analysis based on a local labour market in the northern part of Sweden (Norrbotten). The plotted hazards are based on the estimates from Tables B3a-b. Again the hazard rate for 1996 is higher than the 1998 hazard around the 1996 ER. The hypothesis $coeff\,(HMIN^{r=96}_{y=96}) \leq coeff\,(HMIN^{r=96}_{y=98})$ is rejected. In 13b, the 1998 hazard increases towards the 1998 ER and decreases thereafter. In contrast, the 1996 hazard is generally low. Once again, statistical significance is confirmed.

Figure 13a: Fitted hazards around the 1996 ER, in 1996 and 1998 in Norrbotten.

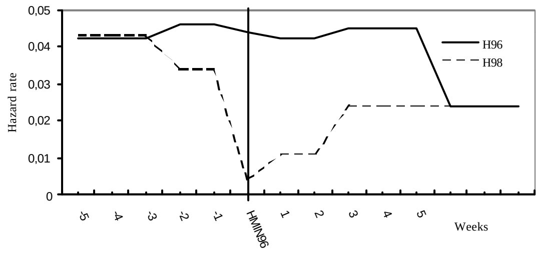
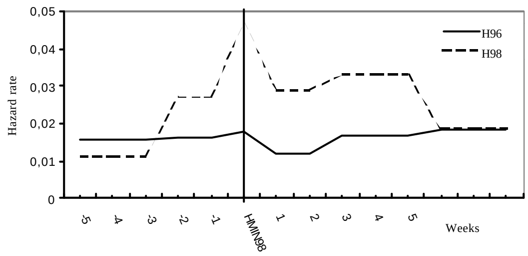


Figure 13b: Fitted hazards around the 1998 ER, in 1996 and 1998 in Norrbotten.



Figures 12a-b and 13a-b support the results from the earlier analysis. Generally, the within-year ER effects are not clear-cut but an adjustment due to the change in 1997 seems apparent, particularly the shift away from the 1996 ER at 17-18 weeks. Rather than UI-related within-year spikes, the adjustments between the years suggest within-year effects.

Measure of the size of the observed effects

To provide a measure of the size of the observed effects of the extension of the ER between 1996 and 1998, I use a formula from Green & Riddell (1997) to calculate average employment duration using baseline and covariate estimates from the duration model where all covariates are set to their average values in each year:

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

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

Possible effects of measurement errors

A few potential explanations for the lack of distinct spikes have been brought forward, for example, difficulty in timing job separation and earlier insured weeks. I now focus on the issue of measurement errors.

From the database, I use the day in which the employment officer registers a person as employed as the first day from which the required days, specified in the ER, are subtracted. If the employment officer, for some reason, waits a few days with registration of this new information, the employment spells are biased downward. In Tables B4a-b (Appendix B), I present the estimates of the UI-related effects instead using information of last week of UI benefit preceding the registered job spell. If the last week in which a person gets UI benefits differ from the week of registered job start, I have added the difference to the aggregate spell.

I assume that people initiate their employment spells the day following their last day of benefits. I also use information about the number of insured days during their last week of UI. This is crucial. If a person starts a job spell on a Monday, a one-week deviation in the two specifications should exist. Studying 1996 and 1998, 8,673 (17.7%) and 7,203 (15.6%) spells were prolonged by at least one week in each year.

Figures 14a-b plot the fitted hazards around the 1996 and the 1998 ER respectively and compare last day of UI benefits and database information as indicators of job spell start. In 14a, the patterns differ slightly. The increase in the exit rate 1-2 weeks before the ER (according to the 1996 rules) is replaced by an almost horizontal hazard using the UI benefit measure. The $HMIN_{y=96}^{r=96}$ estimate is significant (Table B4a) which corresponds to the results when using register data. However, the between-year test $coeff\,(HMIN_{y=96}^{r=96}) \leq coeff\,(HMIN_{y=98}^{r=96})$

is no longer rejected, creating some doubts about the earlier suggested shift away from the 1996 ER.[30]

Figure 14a: Fitted hazards around the 1996 ER in 1996, using two measures of employment duration.

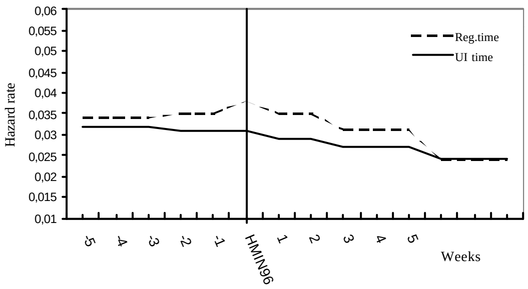
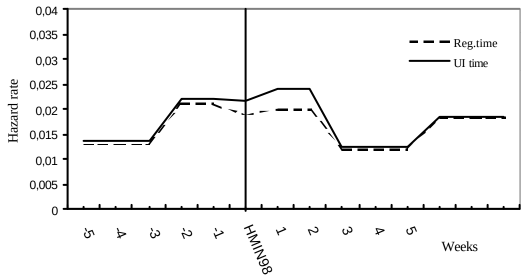


Figure 14b: Fitted hazard around the 1998 ER in 1998, using two measures of employment duration.



Turning to the ER in 1998, the spike 1-2 weeks before the ER is still present using the UI benefit measure. The estimates in Table B4b are very similar to those in Table 8b, which are also illustrated in Figure 14b, where both time specifications are depicted. Although more positive, the $HMIN_{y=98}^{r=98}$ estimate is still not statistically significant. Applying the UI benefit measure, the test $coeff\,(HMIN_{y=96}^{r=98}) \geq coeff\,(HMIN_{y=98}^{r=98})$ is rejected, thus confirming a behavioural adjustment to the 1998 ER.[31]

Using this alternative measure of job duration, there is a small deviation from the original duration measure in the estimates related to the ERs. But the effects of UI fulfilment are still ambiguous when studying each year alone.

8. Conclusions

I investigate the effect of the ER 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 older than age 24 and thus focuses on people with some working experience. It is important to be

[30] The test statistic is distributed as $c^2(1)$ and takes the value of 1.41.

[31] The test statistic is distributed as $c^2(1)$ and takes the value of 12.43.

aware that an ER extension 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.

The effects of the ER found in this study are not clear-cut when studying each year alone. Several possible explanations are introduced here; the lack of exact data on employment spells and the concentration on single spells are two examples. Instead of distinct mass points exactly at the ERs, the exit levels surrounding the ERs imply behavioural effects. This becomes particularly obvious when comparing the exit patterns between years with different UI rules. I detect an adjustment between 1992 and 1996 at the ER according to the rules in 1996 but not at the 1992 ER. The difference in the number of required weeks between these years was only 1-2 weeks. Studying the larger extension between 1996 and 1998, I find evidence of an adjustment to the new ER in 1998. Using register data, I also conclude a shift away from the 1996 ER. However, this result is not confirmed instead using the last day of UI benefit measure. By using predicted hazard rates for each week, I calculate an approximate 2.9-week extension in average employment duration.

In comparison with the Canadian studies, Green & Riddell (1997) concluded a 1.5-week extension between 1989 and 1990 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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SOU 1996:150 *En allmän och sammanhållen arbetslöshetsförsäkring.*

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.

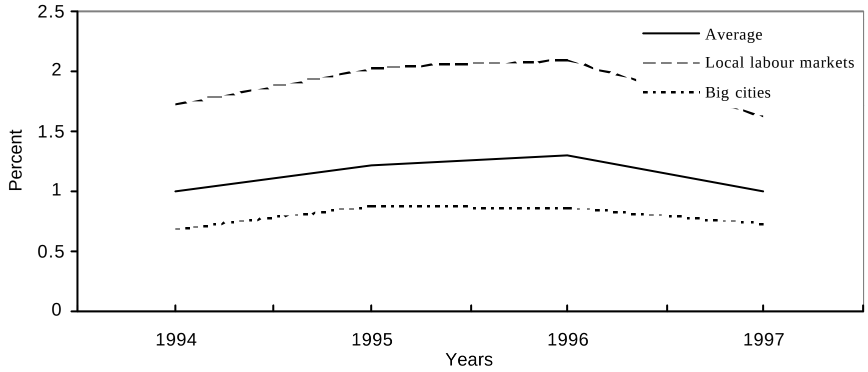
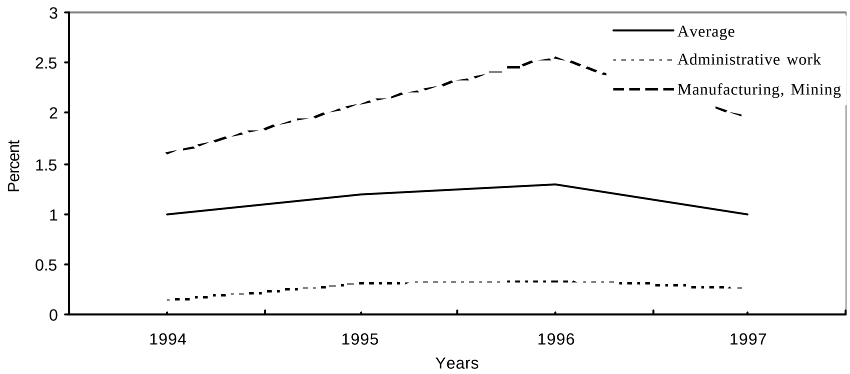


Figure A2: Share of unemployment weeks for people who, at least twice in the years 1994-97, worked for 3-9 months (composite time) and were unemployed the remaining days of a 360-day period, by job category.



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 (AMS). 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.

Table B2a: Estimates of UI-related effects around the 1996 ER, in 1996 and 1998 for farmers.

Variable	1996	Wald´s test	1998	Wald´s test
HMIN –(3-5) weeks	0.869		0.707	
	(0.622)	2.93	(0.743)	1.55
HMIN –(1-2) weeks	2.324**		-0.974	
	(0.950)	0.07	(1.277)	0.59
$HMIN^{r=96}$	2.365**		-0.503	
	(0.983)	3.01	(1.200)	0.00
HMIN +(1-2) weeks	1.018		-0.508	
	(0.976)	1.08	(1.067)	0.33
HMIN +(3-5) weeks	0.299		0.058	
	(0.871)		(0.805)	

Source: 1992 and 1996 longitudinal data from the Swedish Labour Market Board (AMS). **Notes**: (1) Base controls include a step function in duration, gender, age, education, past earnings, sought-after occupation, experience in sought-after occupation, unemployment duration, provincial UR, provincial type 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 17. (5): The sample sizes are 706 and 705 respectively.

Table B2b: Estimates of UI-related effects around the 1998 ER, in 1996 and 1998 for farmers.

Variable	1996	Wald´s test	1998	Wald´s test
HMIN –(3-5) weeks	-0.874**		-1.567***	
	(0.385)	0.37	(0.494)	0.69
HMIN –(1-2) weeks	-0.627		-1.112	
	(0.485)	0.20	(0.620)	0.83
$HMIN^{r=98}$	-0.409		-0.606	
	(0.600)	0.02	(0.692)	0.22
HMIN +(1-2) weeks	-0.482		-0.339	
	(0.500)	3.42	(0.550)	0.06
HMIN +(3-5) weeks	0.313		-0.375	
	(0.367)		(0.391)	

Source: 1992 and 1996 longitudinal data from the Swedish Labour Market Board (AMS). **Notes**: (1) Base controls include a step function in duration, gender, age, education, past earnings, sought-after occupation, experience in sought-after occupation, unemployment duration, provincial UR, provincial type 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 17. (5): The sample sizes are 706 and 705 respectively.

Wald´s test for across-year comparisons:

$$H_0 : coeff\ (HMIN^{r=96}_{y=96}) \leq coeff\ (HMIN^{r=96}_{y=98}) = \boldsymbol{c}_1^2 = 3.42 \Rightarrow \textit{rejected at the 10\% level of significance.}$$

$$H_0 : coeff\ (HMIN^{r=98}_{y=96}) \geq coeff\ (HMIN^{r=98}_{y=98}) = \boldsymbol{c}_1^2 = 0.05 \Rightarrow \textit{not rejected}$$

Table B3a: Estimates of UI-related effects around the 1996 ER, in 1996 and 1998 in Norrbotten.

Variable	1996	Wald´s test	1998	Wald´s test
HMIN –(3-5) weeks	0.568* (0.295)	0.09	0.583*	
			(0.330)	0.36
HMIN –(1-2) weeks	0.661*		0.349	
	(0.382)	0.02	(0.485)	12.15***
$HMIN^{r=96}$	0.615		-1.770***	
	(0.406)	0.05	(0.548)	9.46***
HMIN +(1-2) weeks	0.554		-0.781	
	(0.391)	0.10	(0.493)	5.70**
HMIN +(3-5) weeks	0.639		0.003	
	(0.345)		(0.406)	

Source: 1992 and 1996 longitudinal data from the Swedish Labour Market Board (AMS). **Notes**: (1) Base controls include a step function in duration, gender, age, education, past earnings, sought-after occupation, experience in sought-after occupation, unemployment duration, provincial UR, provincial type 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 17. The sample sizes are 2,272 and 2,228 respectively.

Table B3b: Estimates of UI-related effects around the 1998 ER, in 1996 and 1998 in Norrbotten.

Variable	1996	Wald´s test	1998	Wald´s test
HMIN –(3-5) weeks	-0.170		-0.480	
	(0.211)	0.07	(0.266)	10.45***
HMIN –(1-2) weeks	-0.116		0.369	
	(0.266)	0.05	(0.338)	3.99*
$HMIN^{r=98}$	-0.050		0.932	
	(0.316)	2.02	(0.399)	2.51
HMIN +(1-2) weeks	-0.415		0.454	
	(0.307)	1.92	(0.388)	0.14
HMIN +(3-5) weeks	-0.073		0.573	
	(0.284)		(0.356)	

Source: 1992 and 1996 longitudinal data from the Swedish Labour Market Board (AMS). **Notes**: (1) Base controls include a step function in duration, gender, age, education, past earnings, sought-after occupation, experience in sought-after occupation, unemployment duration, provincial UR, provincial type 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 17. The sample sizes are 2,272 and 2,228 respectively.

Wald´s test for across-year comparisons:

$$H_0 : coeff\,(HMIN^{r=96}_{y=96}) \leq coeff\,(HMIN^{r=96}_{y=98}) = c_1^2 = 12.23 \Rightarrow \textit{rejected at the 1\% level of significance.}$$

$$H_0 : coeff\,(HMIN^{r=98}_{y=96}) \geq coeff\,(HMIN^{r=98}_{y=98}) = c_1^2 = 3.72 \Rightarrow \textit{rejected at the 10\% level of significance.}$$

Table B4a: Estimates of UI-related effects around the 1996 ER in 1996 and 1998, using last day of UI benefit as indicator of job spell start.

Variable	1996	Wald´s test	1998	Wald´s test
HMIN –(3-5) weeks	0.284***		0.212***	
	(0.068)	0.16	(0.082)	0.19
HMIN –(1-2) weeks	0.255***		0.251**	
	(0.090)	0.00	(0.110)	4.63*
$HMIN^{r=96}$	0.255***		0.068	
	(0.098)	0.67	(0.123)	23.07***
HMIN +(1-2) weeks	0.197**		0.448***	
	(0.095)	1.32	(0.123)	0.02
HMIN +(3-5) weeks	0.110		0.460***	
	(0.077)		(0.110)	

Source: 1992 and 1996 longitudinal data from the Swedish Labour Market Board (AMS). **Notes**: (1) Base controls include a step function in duration, gender, age, education, past earnings, sought-after occupation, experience in sought-after occupation, unemployment duration, provincial UR, provincial type 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 17.

Table B4b: Estimates UI-related effects around the 1998 ER, in 1996 and 1998, using last day of UI benefit as indicator of job spell start.

Variable	1996	Wald´s test	1998	Wald´s test
HMIN –(3-5) weeks	-0.180***		-0.305***	
	(0.050)	19.60***	(0.065)	49.79***
HMIN –(1-2) weeks	0.040		0.195**	
	(0.063)	31.06***	(0.086)	0.12
$HMIN^{r=98}$	-0.304***		0.165	
	(0.079)	1.08	(0.107)	1.76
HMIN +(1-2) weeks	-0.373***		0.280***	
	(0.069)	37.17***	(0.092)	92.43***
HMIN +(3-5) weeks	-0.017		-0.399	
	(0.057)		(0.082)	

Source: 1992 and 1996 longitudinal data from the Swedish Labour Market Board (AMS). **Notes**: (1) Base controls include a step function in duration, gender, age, education, past earnings, sought-after occupation, experience in sought-after occupation, unemployment duration, provincial UR, provincial type 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 17.