CESifo *Working Paper Series*

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Working Paper No. 365

November 2000

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* We gratefully acknowledge comments from two anonymous referees, Gerard van den Berg, Per-Anders Edin, Anders Harkman and seminar participants at IFAU, Uppsala, and IIES, Stockholm University. We also thank Georgina Bermann for valuable contributions and acknowledge research assistance from Pontus Larsson and Staffan Magnusson. The research has in part been financed by HSFR.

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Abstract

In June 1995, the Swedish parliament decided to cut the replacement rate in unemployment insurance from 80 percent to 75 percent, a change that took effect on January 1, 1996. This paper examines how this change affected job finding rates among unemployed insured individuals. To identify the effect of the policy we exploit a quasi-experimental feature of the benefit cut: only a fraction of the unemployed was affected by the reduction in replacement rates. We compare the evolution of job finding rates before and after the reform among those affected and those not affected. Our estimates suggest that the reform caused an increase in the transition rate of roughly 10 percent. There is also evidence of anticipatory behavior among the unemployed; the effects of the reform seem to operate several months *before* its actual implementation in January 1996.

Keywords: Unemployment duration, unemployment benefits

JEL Classification: J64, J65

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1. Introduction

Sweden was hit by mass unemployment at a later stage than most other European countries. By 1990, the unemployment rate stood at 1.6 percent; by 1993 it had increased to 8.2 percent. The decline in employment-to-population rates was even more dramatic. 83.1 percent of the working age population was employed in 1990 but only 72.6 percent in 1993. Unemployment has in Sweden as elsewhere turned out to be persistent. The recovery from the shocks of the early 1990s has been relatively slow and shaky. Unemployment in 1999 stood at 5.6 percent and 72.9 percent of the working age population was employed.[1]

The slump in the Swedish economy in the early 1990s resulted in a huge government budget deficit that paved the way for a number of policy decisions to cut expenditure and increase revenues through higher taxes. Unemployment insurance (UI) emerged as one of the targets for expenditure cutting. Unemployment compensation in Sweden has by international standards been generous; in the early 1990s, the maximum replacement rate among workers eligible for UI amounted to 90 percent of previous earnings. The fiscal crisis induced a sequence of decisions to make the UI system less generous and less expensive. The replacement rate was reduced to 80 percent the 1st of July 1993 and was further reduced to 75 percent from the 1st of January 1996 (a decision taken already in June 1995). It is noteworthy that the main motivation for benefit cuts has been the need to exercise fiscal restraint. Concerns about possible adverse incentive effects have not played a major role in the Swedish political debate. Indeed, in the wake of fiscal consolidation in the late 1990s, a decision was taken to raise the UI replacement rate to 80 percent from September 1, 1997.

The main purpose of this paper is to examine how the cut in replacement rates from 80 to 75 percent in January 1996 affected the job finding rates among unemployed workers. We make use of data with information on the length of individual unemployment spells, as well as a host of characteristics pertaining to the individual, the household and the labor market. The key strategy to identify the effect of the benefit reform is to exploit a quasi-experimental feature of the 1996 policy: only a fraction – albeit a majority – of unemployed insured workers was affected by the cut in the replacement rates. We compare the conditional probability of escaping from unemployment to employment before and after the 1st of January 1996 for those affected by the cut – the "treatment group" – with the escape rate for those who were not affected – the "control group". Our results suggest that the benefit cut increased the escape rate by about 10 percent, which is a relatively strong effect compared to what has been found in other studies. We also find evidence of anticipatory behavior among the unemployed: the effect of the cut in replacement rates appears to operate already several months *before* its actual implementation in January 1996.[2]

We begin in the next section by describing the Swedish UI system and the changes that are of particular relevance for our study. Section 3 discusses some theoretical issues, section 4 presents the data and section 5 turns to the empirical analysis. Section 6 concludes.

2. Unemployment Compensation in Sweden

The Swedish UI system is based on voluntary membership in union affiliated UI funds. These funds are subject to various government regulations, including rules concerning benefits levels. The government also heavily subsidizes the funds; in the early 1990s, these subsidies covered around 95 percent of paid-out benefits in the UI funds. There has been a trend increase in the coverage of UI. In the early 1990s, over 80 percent of workers counted as unemployed according to the labor force surveys were members of UI funds. The fraction actually eligible for UI was lower, however, the main reason being the fact that some members do not fulfill the work requirement for eligibility. On average some 65 percent of the stock of unemployed registered at the employment exchange offices received UI during 1990-1995 (see SOU 1996:51, p. 51). The fraction of new spells of unemployment covered by UI was even lower. Carling et al (1996) report that only 43 percent of the inflow during 1991 of new unemployed aged 16-54 received regular UI

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[1] These numbers refer to the national definitions in the labor force surveys.

[2] The reason for our focus on the 1996-reform is that this change is most suitable to analyze as a quasi-experiment. The 1993-change might also be analyzed as quasi-experiment if one is willing to regard workers without UI compensation as a control group, an approach adopted in Harkman (1997). However, this is problematic since the population eligible for UI face very different incentives than those not eligible. By focusing on insured workers we can thus compare treatment and control groups that are more similar than workers with and without UI compensation. Moreover, the data for this earlier period contain less information on personal characteristics. The data needed to analyze the policy change in 1997 have so far not been available.

compensation. The data set used for our present study – based on the inflow of unemployed during the mid-1990s – reveals that 41 percent of the new spells were covered by UI.

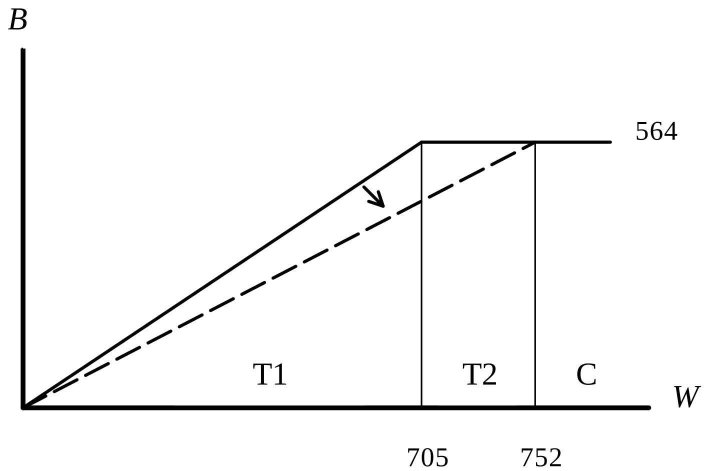
A ceiling on the benefit level – 75 percent of 16 500 SEK per month in 1996 – means that actual compensation rates can be much lower than the maximum rates. It has been estimated that 75 percent of all full-time *employees* had monthly earnings exceeding 16 500 SEK in 1996 (see SOU 1996:51). However, the distribution of actual replacement rates among the *unemployed* may differ substantially from those figures, as low earnings are correlated with higher risks of unemployment. Slightly more than 70 percent of the insured unemployed workers in our data set had a compensation rate of 80 percent before 1996; from 1996 and onwards, some 80 percent of the unemployed workers had a replacement rate at the new maximum of 75 percent.

Workers who are not members of UI funds may receive "cash assistance" (kontant arbetsmarknadsstöd, KAS). Compensation from KAS is much lower than UI benefits (40 percent of the maximum compensation from UI in 1996). KAS is paid out for a maximum of 30 weeks (150 working days), whereas UI benefits are paid for 60 weeks (300 working days) for workers under age 55 and for 90 weeks (450 working days) for workers who are 55 or older. The benefit reform of 1995/1996 also involved a cut in KAS from 245 SEK to 230 SEK per day.

Figure 1 illustrates how benefits (*B*) vary with earnings (*W*) for eligible workers in the mid-1990s. The maximum benefit level is 564 SEK per day, paid five days a week. With a replacement rate of 80 percent (before January 1996 – the solid line), this ceiling kicks in at a monthly pay of 15 510 SEK or 705 SEK per day (15 510/22). After the cut in the replacement rate from January 1996 and onwards (the dashed line), the ceiling kicks in at 16 544 SEK per month or 752 SEK per day. We can thus allocate the individuals into three groups, labeled T1, T2 and C in Figure 1. Group T1 includes people with replacement rates of exactly 80 percent before the change; group T2 consists of workers with pre-unemployment earnings in the interval 705 to 752 SEK; group C, finally, includes workers who were not affected by the cut in benefits. We will refer to T1 and T2 as "treatment groups" whereas C is the "control group".

In addition to these benefit cuts, some other changes were also introduced in January 1996. Workers who quit their job may be exposed to a benefit sanction, i.e., a temporary withdrawal of benefits. The period of benefit withdrawal for quitting "without good cause" was extended from 20 to 45 days from the 1st of January 1996. Workers who repeatedly rejected suitable job offers could be exposed to a withdrawal of benefits of up to 80 days (compared to 20 days before January 1996).

Figure 1. Unemployment Benefits in Sweden in the mid-1990s.



Note: The solid (dashed) line depicts the replacement rate before (after) January 1, 1996.

3. Theoretical Issues

The basic theory of how UI compensation affects job search is presented in Mortensen (1977). Other contributions include Burdett (1979), Mortensen (1990) and van den Berg (1990, 1994). The theory portrays an unemployed worker engaged in sequential search with the objective to maximize the present value of lifetime income (or utility). Mortensen allows for fixed duration of benefit payments and stochastic duration of employment spells. There is also an eligibility condition requiring a certain amount of work experience in order to qualify for UI. The wage offer distribution is taken as stationary and known by the unemployed searcher.

The most important implications derived from this model are the following: First, the worker's reservation wage declines as he approaches the date at which benefits expire; hence the exit rate increases over the spell of (insured) unemployment. Second, an increase in the benefit level makes it more attractive for presently not eligible workers to accept jobs and thereby become qualified for benefits in the future; higher benefits thus result in an *increase* in the exit rate from unemployment to employment for workers who are not qualified for benefits, a response known as the "entitlement effect". Third, a rise in the benefit level will cause a newly unemployed and insured worker to increase his reservation wage but induce an insured worker close to benefit exhaustion to *reduce* his reservation wage. The exit rate thus declines for newly unemployed insured workers but increases for workers who have come close to benefit exhaustion. The last property follows from the fact that a higher benefit level increases both the value of continued search as unemployed and the value of accepting an offer. The immediate value of higher benefits is small for workers close to benefit exhaustion, as they are almost in the same situation as workers not qualified for UI.

The intriguing third prediction of this theory – that workers close to benefit exhaustion will respond to higher benefits by *lowering* the reservation wage – has rarely been tested in empirical research.[3] It has been common to include measures of benefits or replacement rates without allowing for different effects between those who have just entered the unemployment pool and those who are close to benefit exhaustion. If the theory is correct, however, the estimates of benefit effects are likely to be sensitive to the duration composition of the samples at hand.

The Swedish institutional setting raises some new issues. First, there is a question whether benefits have a fixed duration or if they in practice have unlimited duration. Active labor market programs have provided important escape routes from "open" unemployment. Since participation in these programs qualify for future benefit periods – and programs are targeted at the long term unemployed at risk of losing benefits – one might argue that benefit periods are in fact of unlimited length and there is then little reason to expect an increasing exit rate as benefit exhaustion is approached.[4]

A second issue is the possibility of anticipatory behavior when the policy change is known long in advance of its actual implementation. The decision to cut replacement rates from the 1st of January 1996 was taken already in June 1995. Workers who were

[3] The study by Katz and Meyer (1990) on U.S. data is an exception. The study does not find significant support for the prediction, however.

[4] The estimates in Carling et al (1996) on Swedish data lend some support for the hypothesis that the exit rate to employment does increase as insured workers approach benefit exhaustion, a result consistent with the basic theoretical prediction.

unemployed during the second half of 1995 were presumably aware of the fact that a new benefit regime was to be implemented in January 1996.

How would, then, an anticipated cut in *future* benefits affect an unemployed worker's search behavior? Consider an insured worker who has just entered unemployment and assume for simplicity that benefits have unlimited duration. A future cut in benefits would be like introducing a two-tiered benefit system with an initial relatively high level followed by a subsequent lower level. The optimal response to such a known future benefit cut would be to choose a declining reservation wage path prior to the change and a constant reservation wage thereafter (absent other changes in the worker's environment). The exit rate would thus be increasing as the worker approaches the date at which the benefit level is cut. It is more complicated to characterize behavior if the benefit period is fixed. The effect of a future reduction in benefits may in general depend both on the time to the benefit cut and the time to benefit exhaustion.

These examples suggest that we should, in general, expect that the reforms that were implemented in January 1996 might have affected search behavior already during the second half of 1995. We will in our empirical analysis investigate whether there is any evidence of such anticipatory behavior among the unemployed.

4. The Data

We have combined a number of different data sources for the empirical analysis. The data are part of the so-called LINDA database, a register-based longitudinal database for Sweden.[5] We use data on benefits from the UI funds and information on the length of spells on unemployment from the employment agencies. Survey evidence indicates that some 90 percent of those who are unemployed according the labor force surveys also register at the public employment offices (Statistics Sweden, 1993). Moreover, we focus our analysis on those who are entitled to UI, a category for which registration at the employment offices is compulsory. The different data sets are merged as described briefly in Appendix B and in detail in an appendix available on request. We have also added data on local unemployment rates.

[5] For more information on LINDA, see the web-page http://www.nek.uu.se/Linda/.

Our sample is drawn from the inflow to the unemployment registers during 24 months during three years: 1994 (the last six months), 1995 (all twelve months) and 1996 (the first six months). We follow the individuals until they escape unemployment or, at the most, until July 1997. The sampling procedure resulted in 45 125 individuals. 22 265 of those had neither UI compensation nor KAS, 2 384 received KAS and 20 476 received regular UI compensation. We decided to focus the analysis on those entitled to regular UI, thereby avoiding the need to address selectivity issues with respect to the choice of becoming insured. A further limitation was to set the upper age limit to 54 and to exclude workers with reported health problems. The reason for the age limit is that older workers (aged 55 or older) were entitled to 450 days of unemployment compensation (compared to 300 days for those aged 54 or less). Differences in the maximum duration of benefit payments may have consequences for search behavior over the spell of unemployment and hence for the evolution of the escape rate to employment.

The resulting sample contained 18 429 individuals. Table 1 gives descriptive statistics on a variety of characteristics for individuals in this sample, whereas Table 2 describes the distribution of replacement rates. Spell characteristics are displayed in Table 3. Table 1 is largely self-explanatory. The individuals in the control group are on average older, better educated and have higher wages as well as higher non-labor income than people in the treatment groups. It is notable that the fraction of women is 66 percent of the T1 group, i.e., the group with earnings below the 1995 ceiling of 705 SEK per day. The control group, by contrast, includes only 16 percent women.

The distribution of replacement rates is highly compressed in this sample, as shown in Table 2. Before 1996, 72 percent received the statutory maximum of 80 percent, and 16 percent received a replacement rate in the interval 70-80 percent. Only 12 percent received less than 70 percent. After the 1996-reform, 80 percent received the maximum of 75 percent. The duration pattern of the spells, shown in Table 3, reveals that almost 60 percent of the spells end within three months. Only 10 percent of the spells last for more than a year. Almost 50 percent of the spells are escaped through transitions to regular jobs. Most of the remaining spells end through exits to non-participation and labor

Table 1. Sample characteristics (means).

	T1	T2	C
Demographic characteristics			
Age	32.4	36.1	37.5
Female	0.664	0.268	0.165
Foreign citizen: Nordic	0.026	0.023	0.021
Foreign citizen: Non-Nordic	0.045	0.021	0.012
Cohabitant	0.115	0.154	0.131
Married	0.459	0.522	0.581
Children, 15 yrs old or less	0.636	0.521	0.520
Children, 16 yrs old or more	0.066	0.077	0.093
Education and work experience			
9 yrs or less	0.220	0.219	0.199
High school, 2 yrs	0.417	0.479	0.489
High school, 3 yrs	0.144	0.098	0.104
University, 1-3 yrs	0.149	0.112	0.113
University, 4 yrs or more	0.062	0.087	0.092
No work experience	0.156	0.085	0.047
Some work experience	0.312	0.198	0.121
Long work experience	0.501	0.707	0.821
Previous wage and non-labor income	560.2 (12 324)	729.3 (16 045)	854.5 (18 799)
Wage per day (month), SEK			
Income of spouse (SEK per month)	5514	6022	6241
Income from capital (SEK per month)	77	111	153
# individuals	13 330	1 396	3 703

Notes: The sample is restricted to workers with regular unemployment compensation who are less than 55 years old. Experience refers to work experience in the occupation within which the person searches for a job. All variables are dummies except age, previous wage and income.

Table 2. The distribution of replacement rates before 1996.

Replacement rate	%
0.80	72.33
[0.775, 0.80)	3.10
[0.75, 0.775)	5.34
[0.725, 0.75)	3.83
[0.70, 0.725)	3.75
[0.65, 0.70)	5.71
[0.60, 0.65)	3.05
[0.50, 0.60)	2.17
[0.40, 0.50)	0.62
< 0.40	0.076

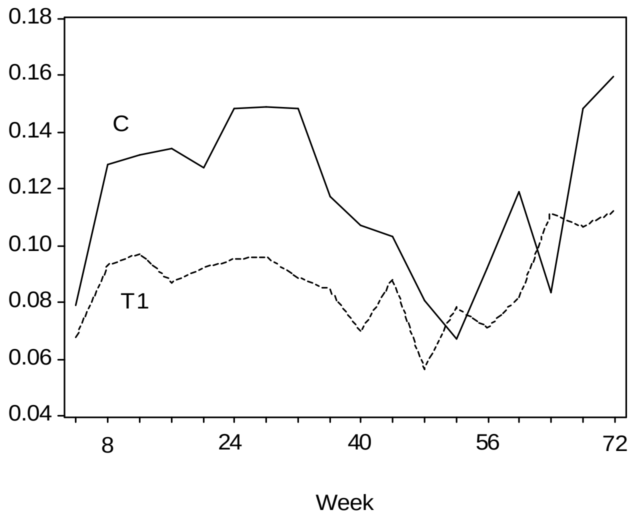
Table 3. Spell characteristics.

Mean duration (months)	5.4
Proportion of spells lasting more than:	
30 days (1 month)	90.6
60 days (2 months)	76.6
120 days (3 months)	43.4
180 days (6 months)	28.4
360 days (12 months)	6.2
420 days (14 months)	3.7
Proportion of spells ending in:	
regular employment	46.8
labor market program	23.8
labor force exit	24.3
lost contact	4.4
Censored	0.8
# spells	18 429

market programs.[6] The category "lost contact", consists of workers with uncertain destination state as the employment office has lost contact with them. An earlier follow-up study of "lost contact" individuals by Bring and Carling (1994) found that roughly 50 percent of them actually were employed. We assume in the empirical analysis that these workers found a job at the attrition date.

The evolution of the empirical job finding rates for the C and T1 groups are shown in Figure 2.[7] The rates are computed for time intervals of four weeks. There is a phase with increasing exit rates during the first months of unemployment, followed by a phase of declining rates. The exit rates start to increase after around 50 weeks of elapsed unemployment. It is tempting to interpret the rising hazard after 50 weeks as being driven by the risk of benefit exhaustion. However, this can be no more than a speculation absent a control group that is *not* exposed to benefit exhaustion after 60 weeks. Note that the figure is based on data for individual spells that are pooled irrespective of when the spells started. It thus shows the "cross-sectional" differences between hazard rates for control and treatment groups but contains no information on the pre- and post-reform evolution of these rates. We will provide a before-after comparison of hazard rates as we proceed.

Figure 2. Job finding rates for control (C) and treatment (T1) groups.



[6] The distinction between labor market programs and labor force exits is not quite conventional. According to the labor force surveys, participation in labor market programs usually means that the person is classified as being outside the labor force.
[7] The hazard rate for the T2 group has basically the same pattern as the rates shown in Figure 2. We show only two rates to avoid a cluttered figure.

5. Empirical Analysis

5.1 Empirical Strategy

Many studies concerned with the effects of unemployment benefits on unemployment duration have made use of data on unemployment spells with cross sectional variations in benefit receipt.[8] This approach is susceptible to the criticism that the estimates may be biased due to unobserved characteristics that are correlated with the amount of benefit receipt. We therefore proceed by exploiting a feature of the 1996-reform that is close to a natural experiment.

Recall that the cut in replacement rates did not affect all unemployed workers. Referring to Figure 1, there are two "treatment groups". The first one (T1) is the group with replacement rates of exactly 80 percent before the change, whereas the second (T2) consists of workers with $W \in (705{,}752)$. Both groups experienced cuts in the replacement rates, but the cuts in the rates were smaller for T2 than for T1. The "control group", finally, consists of workers who were not affected by the cut, i.e., those with earnings equal to or exceeding the new ceiling of 752 SEK. As shown by Table 1, there are 13 330 persons in T1, 1396 in T2 and 3703 in C.

The general strategy for estimating the effect of the benefit reform is to examine the evolution of the hazard rates for the treatment groups and the control group before and after the policy change.[9] If the hazard rate for a treatment group increases more (declines less) than the hazard rate for the control group around the 1st of January 1996, then we conclude that the reform increased the hazard rate.

We begin by a simple comparison of the job finding rates before and after the policy change in January 1996. We compare the empirical hazard rates for the control group and the largest treatment group (T1), separating pre-reform spells (beginning before the policy change) and post-reform spells (beginning after the change). Specifically, we compare spells that started between January and mid-May 1995 (pre-reform spells) with spells that started between January and mid-May 1996 (post-reform spells). The time intervals are

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[8] There are a large number of studies in this area. The paper by Lancaster and Nickell (1980) is an early and representative example. The available surveys include the book by Devine and Kiefer (1991) and the papers by Atkinson and Micklewright (1991), Pedersen and Westergård Nielsen (1993) and Holmlund (1998).

[9] Meyer (1989), Hunt (1995) and Steiner (1997) have adopted a similar methodology in studies of changes of UI policies.

chosen so as to avoid distortions due to seasonal effects. The hazard rates are calculated for the first 36 weeks.

Figure 3 shows the results of the calculations.[10] The policy change is associated with a substantial decline in the C-hazard, whereas the T1-hazard increases slightly during

Figure 3. Job finding rates before and after the benefit cut for the control and treatment groups.

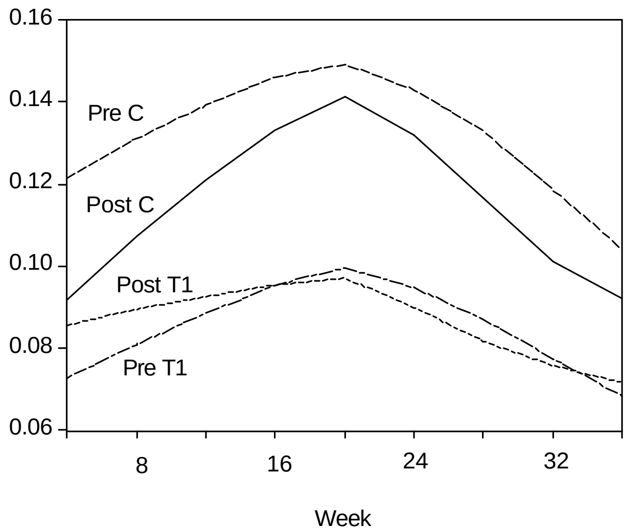
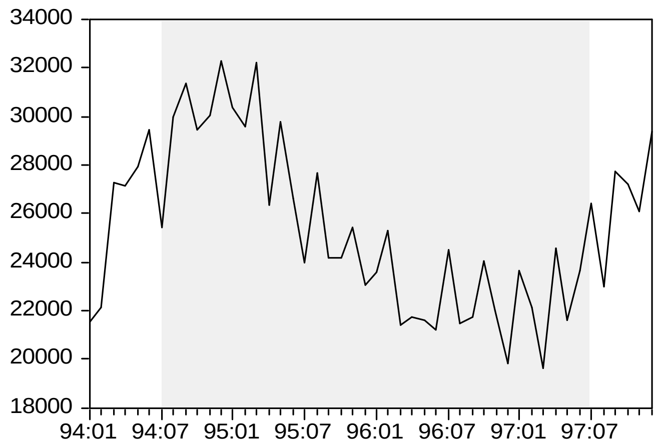


Figure 4. The monthly flow of new vacancies, 1994.01 – 1997.12.



Note: The shaded area corresponds to the "sample window" for our unemployment spells.
Source: The National Labour Market Board.

[10] The empirical hazard functions are quite rough and have therefore been smoothed by a scatterplot smoother (see Cleveland, 1979).

the first quarter. A crude "difference-in-difference" estimate of the effect of the policy change suggests an increase in the hazard rate of approximately 0.018, or 18 percent if applied to a job finding rate of 0.10 per four-week period.[11] When interpreting this figure, it is important to recognize that we compare two time periods with somewhat different labor market conditions. The period 1995–1996 is characterized by a weakening of overall economic activity, resulting in a substantial fall in the number of new vacancies notified to the employment offices (see Figure 4). With this evolution of labor demand conditions, some decline in job finding rates should be expected. The surprising part of Figure 3 is the absence of a decline in job finding rates among workers in treatment group T1.[12] All in all, the evidence from this crude before-after comparison suggests that the policy change may have caused an increase in job finding rates.

We proceed to discuss the empirical specification. The difference-in-difference procedure can be described as follows, assuming for the moment that there is only one treatment group. Let $h(t)$ denote the hazard rate and consider the equation:

$$(1) \qquad h(t) = h_0(t)\exp\left(m(x, z(t); \Omega) + d \cdot D_t^{96} + g \cdot D^T + l \cdot D^T \cdot D_t^{96}\right).$$

The baseline hazard, $h_0(t)$, is taken to be identical for the treatment and control groups. $m(\cdot)$ is a function that links time-constant covariates, x, and time-varying covariates, $z(t)$, to the hazard rate, and Ω is a vector of parameters corresponding to the covariates. D_t^{96} is a time-varying dummy, where $D_t^{96} = 0$ prior to January 1996 and $D_t^{96} = 1$ thereafter. D^T is a dummy for the treatment group. The effect of the cut in the replacement rate is obtained by comparing the hazard rates for the treatment and the control groups before and after the 1st of January 1996. The effect of the policy change is given by the coefficient on the interaction variable, i.e., l .

This difference-in-difference approach is not without pitfalls. Suppose, for example, that labor market opportunities develop differently for the two groups around the time of

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[11] The mean difference between the post- and pre-hazard for the C-group is –0.0166, whereas the analogous mean difference for the T1-group is 0.0016.

[12] There appear to be relatively small differences between the pre- and post-categories in terms of their human capital characteristics. The real wage increases by 1.6 percent for the T1-group whereas it falls by 0.2 percent for the C-group.

the policy change, thus causing an upward shift in the hazard for one group and a downward shift for the other group. A negative bias in the estimated effect is obtained if the demand for skilled labor – typically at the benefit ceiling and therefore in the control group – increases relative to the demand for less skilled workers (typically in the treatment group). A positive bias is obtained if the opposite development of relative labor market opportunities occurs. It thus becomes important to assess the extent to which such divergent changes in labor market conditions have taken place during this period.

We have information on two treatment groups and will exploit information on both, recognizing that workers in group T2 experienced cuts in replacement rates that were smaller than those experienced by workers in T1. Let R denote the replacement rate prior to the benefit reform and consider the following specification:

$$(2) \qquad h(t) = h_0(t)\exp\begin{pmatrix} m(x, z(t);\Omega) + d \cdot D_t^{96} + g_1 \cdot D^{T_1} + g_2 \cdot D^{T_2} \\ \\ + b \cdot \left[D^{T_1} \cdot D_t^{96} + \{(R-0.75)/0.05\} \cdot D^{T_2} \cdot D_t^{96}\right] \end{pmatrix}.$$

We control for time effects by the time dummy and for group differences by means of the dummies for the two treatment groups, i.e., D^{T_1} and D^{T_2}. The specification presupposes that it is changes in replacement rates that matter for behavior. The effect for those around W=705, and hence $R \approx 0.80$, should be the same irrespective of whether they are just above or just below the initial ceiling. Analogously, the effect for those around $W = 752$, and hence $R \approx 0.75$, should be the same irrespective of whether they are located to the left or to the right of the 1996-ceiling. The variable $(R - 0.75)/0.05$ is thus located in the interval (0,1). We let $DPOL$ denote the interaction terms capturing the policy change, i.e.,

$$(3) \qquad DPOL_t \equiv D^{T_1} \cdot D_t^{96} + \{(R-0.75)/0.05\} \cdot D^{T_2} \cdot D_t^{96}$$

The effect of the benefit reform is given by the coefficient in front of the interaction terms in (2), i.e., β. Note that the policy change involved a cut in the replacement rate of 5 percentage points.

5.2 Empirical Results

The results of the estimations are given in Table 4. The baseline hazard is estimated non-parametrically for each four-week interval. Appendix A presents the statistical model and the estimates of the baseline hazard (for the model in the fourth column) are given in Appendix C. As mentioned, individuals in the "lost contact" category are treated as if they entered employment at the attrition date. Our results are basically the same if we alternatively treat these subjects as right-censored.

The variable of main interest is *DPOL* in the fourth line of the table. The estimates of its coefficient, β, vary between .095 and .117; the estimated effect of the benefit cut on the job finding rate is thus roughly 10 percent. The specifications in column (3) and (4) clearly outperform the more restrictive specifications in the first two columns; most of the demographic characteristics and some education and experience variables are significant. The *t*-values for the estimated effects are 2.1 and 2.0, respectively, in the two right-most columns. Our conclusion, then, is that the benefit cut appears to have increased the transition rate to employment.[13]

Among the other variables, we note that the coefficient on the dummy for treatment group T1 is significantly negative, which confirms the picture given already by the raw hazards in Figure 2. The coefficient on T2 is insignificantly different from zero. The time dummy is significantly negative, as should be expected given the evolution of labor demand conditions over the period. The local unemployment rate has a negative effect, although only marginally significant. The demographic variables have in general significant coefficients. The job finding rate is decreasing in age, with an increase in age of 10 year being associated with a fall in the hazard of about 10 percent. Women have substantially lower escape rates than men; the difference is over 20 percent. This pattern is very different from the results in Carling et al (1996), where the escape rate to employment were estimated to be higher for women than for men. The precise reasons for these differences are unclear, but may reflect different labor market conditions associated with the sample periods, i.e., the early 1990s in the previous study as opposed to the

[13] We have tested whether pooling of the two treatment groups is valid by adding an additional free parameter for the T2-group, appropriately scaled by the change in the replacement rate as in eq. (3). The estimate is –0.052 with a standard error or 0.043 (not significant).

Table 4. Estimation results, exits to employment. Asymptotic standard errors in italics.

	(1)	(2)	(3)	(4)
D_t^{96}	-.214 *.046*	-.178 *.047*	-.179 *.048*	-.181 *.048*
D^{T_1}	-.330 *.028*	-.325 *.028*	-.152 *.031*	-.126 *.041*
D^{T_2}	-.003 *.040*	-.004 *.040*	-.004 *.040*	+.006 *.041*
DPOL	**.095** ***.056***	**.095** ***.057***	**.117** ***.057***	**.116** ***.057***
Regional dummies	No	Yes	Yes	Yes
Dummies for the quarter of inflow	No	Yes	Yes	Yes
Local unemployment rate		-.779 *.397*	-.694 *.399*	-.684 *.399*
Demographic characteristics				
Age			-.010 *.002*	-.010 *.002*
Female			-.224 *.025*	-.221 *.025*
Foreign citizen: Nordic			-.067 *.069*	-.067 *.070*
Foreign citizen: Non-Nordic			-.482 *.068*	-.478 *.068*
Cohabitant			.034 *.034*	.046 *.050*
Married			.116 *.027*	.128 *.043*
Children, 15 yrs old or less			-.195 *.025*	-.192 *.026*
Children, 16 yrs old or more			.151 *.040*	.153 *.040*
Education and work experience				
Less than 9 yrs			-.039 *.152*	-.041 *.152*
9 yrs			-.074 *.148*	-.077 *.148*
High school, 2 yrs			.049 *.147*	.045 *.147*
High school, 3 yrs			-.002 *.149*	-.007 *.149*
University, 1-3 yrs			.055 *.149*	.048 *.149*
University, 4 yrs			.286 *.150*	.278 *.150*
No work experience			.414 *.098*	.413 *.098*
Some work experience			.606 *.094*	.605 *.094*
Long work experience			.701 *.093*	.699 *.093*
Previous wage and non-labor income				
ln wage				.067 *.070*
ln (1 + income from capital)				.002 *.005*
ln (1+ income of spouse)				-.002 *.005*
ln L	29 886.1	29 737.9	29 499.2	29 498.5

Notes: There are 18 429 spells. The regional dummies are dummies for counties (län). The reference individual is a single male Swedish citizen in the control group. He has no children and has missing values on education and work experience.

mid-1990s in the present one.[14] Note also that this paper uses a sample that is restricted to insured workers, whereas all categories of unemployed were pooled in the previous study.

The fact that women appear to have much lower exit rates than men has motivated us to estimate separate models for men and women. The precision of these estimates (not reported) is generally lower, as should be expected. The benefit effects are not significantly different between the two groups[15], but the effects of children are. Having small children means a 30 percent lower exit rate for women but only 10 percent lower rate for men. The pattern is reversed for older children: having older children is associated with a 25 percent higher exit rate for women whereas the effect is only 5 percent for men.

Among other results reported in Table 4, we note that non-Nordic immigrants have job finding rates that are more than 40 percent lower than the exit rates for Swedish citizens. Better education is not uniformly associated with higher escape rates, although a long university education appears to make a significant difference. Improved work experience has the expected positive effects. Finally, we find no significant effects of the previous wage and non-labor income, where non-labor income includes the person's income from capital and the income of the spouse.[16]

We have also investigated whether there is any effect of the policy change on exit rates to labor market programs and to non-participation. A cut in benefits is likely to raise the exit rates to non-participation since the value of unemployment declines relative to the value of being outside the labor force. Under the assumption of independent risks, a competing risk model can be estimated by treating exits to states other than that of interest

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[14] The unemployment rate according to the labor force surveys was constant for males (8.4 percent) between 1995 and 1997, whereas it increased from 6.9 to 7.6 percent for females during the same period.

[15] In a model with interactions between *DPOL* and the female dummy we obtain an estimate of the main effect of 0.123 (with standard error 0.062). The estimated parameter for the interaction variable is –0.010 (with standard error 0.040). The implied estimates for males and females are thus 0.123 and 0.113, respectively, with a standard error of 0.081 for females. The large standard error for females appears to be due to the fact that there are relatively few women in the C-group.

[16] The lack of significance for the previous wage is perhaps not so surprising recognizing that we control for a number of personal characteristics of the individuals. Non-labor income would have no effect on workers' search behavior if they only cared about income, in which case the difference between the value of employment and the value of unemployment would be unaffected by changes in non-labor income. In general, however, we would expect that non-labor income reduces effective labor supply, i.e., reduces search intensity.

as censored observations at the relevant point in time.[17] Table 5 shows the results of the estimations. We find *no* significant effect of the policy change on exits to labor market programs and non-participation.

Other results are more in line with what we would expect. We note, for example, that workers in T1 are much more likely to leave the labor force than the other groups. Transition rates to non-participation are also higher among women and among young workers as well as among persons with small children. The local unemployment rate has a very strong positive effect on the exit rate to labor market programs and also a positive effect on exits to non-participation.

5.3 Discussion

Comparisons with other studies

How large is the estimated effect of the benefit cut compared to the results of earlier studies? Layard et al (1991) characterize the literature as follows (p. 255): "The basic result is that the elasticity of the expected duration with respect to benefits is generally in the range 0.2-0.9 depending on the state of the labour market and the country concerned, although estimates as low as 0 (Atkinson et al. 1984) and as high as 3.3 (Ridder and Gorter 1986) may be found". Our implied elasticity of the hazard rate with respect to benefits is about 1.6, which is on the high side compared to most of the results reported in previous research.[18]

Lancaster and Nickell (1980) reviewed some of the early empirical work in this field and concluded that the size of the effect of benefits on the exit rate from unemployment is "now a rather firmly established parameter". This conclusion was surely premature, as has been revealed by subsequent studies with rather diverse results. One can ask whether there is any systematic relationship between the adopted methodology and the magnitude

[17] Models that incorporate transitions from unemployment to non-participation are, for example, presented in Toikka (1976) and Flinn and Heckman (1982). One can think of non-participation as a state associated with a utility flow of non-market opportunities, subject to stochastic change. Changes in benefits affect the "non-market reservation utility", i.e., the value of non-market time that makes the unemployed worker indifferent between unemployment and non-participation.

[18] The 5 percent cut in the replacement rate corresponds to a 6.25 percent reduction in benefits (5/80=.0625). If the rise in the hazard is taken to be 10 percent, the implied elasticity is 1.6 (10/6.25). Of course, the elasticity of the expected duration is equivalent to the elasticity of the hazard rate only in the absence of duration dependence in the hazard rate.

Table 5. Estimation results for competing exits. Asymptotic standard errors in italics.

	Exits to employment (1)	Exits to labor market programs (2)	Exits to non-participation (3)
D_t^{96}	-.181 *.048*	.079 *.070*	-.628 *.121*
D^{T_1}	-.126 *.041*	+.099 *.068*	+.492 *.075*
D^{T_2}	+.006 *.041*	+.051 *.073*	+.175 *.093*
DPOL	**.116 *.057***	**.078 *.077***	**-.043 *.128***
Local unemployment rate	-.684 *.399*	2.263 *.574*	.185 *.093*
Demographic characteristics			
Age	-.010 *.002*	-.007 *.002*	-.030 *.003*
Female	-.221 *.025*	-.071 *.037*	.484 *.040*
Foreign citizen: Nordic	-.067 *.070*	.012 *.093*	-.017 *.098*
Foreign citizen: Non-Nordic	-.478 *.068*	.008 *.073*	-.025 *.077*
Cohabitant	.046 *.050*	.052 *.072*	.051 *.071*
Married	.128 *.043*	.002 *.061*	.091 *.060*
Children, 15 yrs old or less	-.192 *.026*	.016 *.040*	.171 *.041*
Children, 16 yrs old or more	.153 *.040*	.099 *.062*	.120 *.067*
Education and work experience			
Less than 9 yrs	-.041 *.152*	.221 *.193*	-.086 *.220*
9 yrs	-.077 *.148*	.318 *.184*	.081 *.210*
High school, 2 yrs	.045 *.147*	.410 *.182*	.019 *.209*
High school, 3 yrs	-.007 *.149*	.498 *.184*	.109 *.211*
University, 1-3 yrs	.048 *.149*	.238 *.187*	.702 *.210*
University, 4 yrs	.278 *.150*	.240 *.193*	.389 *.215*
No work experience	.413 *.098*	.479 *.101*	-.056 *.076*
Some work experience	.605 *.094*	.441 *.098*	-.080 *.072*
Long work experience	.699 *.093*	.289 *.097*	-.154 *.071*
Previous wage and non-labor income			
ln wage	.067 *.070*	.114 *.099*	.135 *.111*
ln (1 + income from capital)	.002 *.005*	.000 *.008*	.012 *.008*
ln (1+ income of spouse)	-.002 *.005*	-.007 *.007*	-.027 *.007*
Proportion exiting	0.512	0.238	0.243
ln L	29 498.5	16 003.4	14 137.4

Notes: There are 18 429 spells. The reference individual is a single male Swedish citizen in the control group. He has no children and has missing values on education and work experience. Column (1) is identical to column (4) in Table 4. Regional dummies and dummies for the quarters of inflow are always included.

of the estimated effects. There seems to be no clear pattern here. Hunt (1995) uses a difference-in-difference approach close to the one in the present paper and finds no robust effects of benefit cuts in Germany (although she does find significant and substantial disincentive effects of extended benefit entitlement periods). By contrast, the papers on benefit sanctions in the Netherlands – Abbring et al (1998) and van den Berg et al (1998) – report very strong incentive effects of benefit cuts.[19]

The Swedish study most comparable to the present one is Harkman (1997). Harkman examined the effects of the cut in replacement rates from 90 to 80 percent in 1993 by a methodology similar to the one adopted here. Cox proportional hazard models were estimated on a data set that included both workers with UI compensation and workers without UI, with the former category serving as the treatment group and the latter as the control. The study found generally significant increases in the exit rate from unemployment at the time of the benefit cut, with a stronger effect on transitions to non-participation (28 percent) than on transitions to employment (7 percent and an only marginally significant effect).

A major difference between Harkman's results and ours is thus that he found significant and substantial effects on exits to non-participation, whereas we have been unable to detect any effect on this escape route. The reasons for the different results can only be a matter of speculation. Our focus on workers with UI compensation means that we analyze a group with a relatively strong labor force attachment. The participation decisions of this group may well be relatively insensitive to benefit changes.

Does the benefit effect vary by age?

Our basic specification imposes the same benefit effect across all groups of workers. Earlier studies, such as Narendranathan et al (1985), have found that the benefit effect tends to be stronger for young workers. One conjecture, given in Narendranathan et al, is that the effect is stronger because the wage offer distribution probably is more compressed for young workers. A given change in the reservation wage has a stronger effect on the exit rate if the wage offer distribution is very dense in the relevant region. We have checked for possible age differences in the benefit effect by a number of alternative specifications.

[19] The estimates imply that temporary benefit cuts in the interval of 5 to 30 percent cause increases in job finding rates of 77 percent (the metal industry) and 107 percent (the banking industry). Similar

Table 6 shows results for a specification with age dummies and interactions with *DPOL* and the age dummies. The results are clear: the benefit cut does seem to have had a larger impact on the job finding rates among young workers (under 25) than for the rest of the unemployed. The difference between the effect for the young and the reference group (aged 25-44) is over 20 percent.[20] The precise reason for these differences between age groups remains as a largely open question, however.[21]

Table 6. Benefit effects by age.

	Estimate	*Std error*	Estimate	*Std error*
Under 25	.210	*.036*	.171	*.041*
25-44 (reference group)	0		0	
45-54	-.087	*.032*	-.050	*.036*
DPOL	.115	*.056*	.083	*.058*
(Under 25)·*DPOL*			.108	*.040*
(45-54)·*DPOL*			-.019	*.041*

Notes: The other covariates are those included in column (4) in Table 4 (except age that is replaced by age dummies).

Sensitivity analysis

We have briefly mentioned possible pitfalls associated with the difference in difference approach, such as omitted controls for divergent labor market opportunities among treatment and control groups. Indeed, the distribution of wages among treatment and control groups differs substantially and the labor markets for these groups may have

estimates are reported for exits out of welfare.

[20] The effect for young workers (under 25) is 0.191 (with standard error 0.082), whereas the effect for old workers is 0.064 (with standard error 0.078). We have also estimated hazard models for the three age groups separately. The results (not reported) are very similar to those displayed in Table 6.

[21] We have also tried to test the hypothesis that the benefit effect varies by elapsed duration, a prediction derived by Mortensen (1977). The test involves including interactions between *DPOL* and the hazard. The results (not reported) do not give any support for the hypothesis that the benefit effects are attenuated (or reversed in sign) at long durations; in fact, the results tend to suggest that the effects are stronger for those with relatively long elapsed durations. Of course, we should not expect any sign

evolved very differently around the time of the benefit reform. To address this issue, we have undertaken a sensitivity analysis so as to check whether the results are robust to changes in the composition of the treatment and control groups. In particular, we have successively eliminated cases with the *lowest* wages in the T1-group and the *highest* wages in the control group, thereby reducing the heterogeneity of the sample with respect to pre-unemployment wages. We excluded 5, 10, 20, and 50 percent of the lowest wages in the T1-group and analogously (and simultaneously) 5, 10, 20 and 50 percent of the highest wages in the control group. The outcome of this exercise is shown in Table 7, where the results for the full sample are replicated for comparison. There is no evidence that the estimated effect is much affected by excluding workers at the bottom and the top of the wage distribution.

As a crude check for divergent labor market opportunities among treatment and control groups, we have examined employment-to-population rates by educational groups (Figure 5). If our estimated benefit effect is due to divergent labor market opportunities, the implication would be that these opportunities have become relatively *more* favorable for the less educated over the period 1994-1997 (since the less educated are typically in the treatment groups due to lower earnings). Figure 5 gives little support for this possibility; if anything, the data suggest the reverse. Indeed, there is a widespread view that the demand for labor in the wake of the computer revolution has become "skill-biased", i.e., favoring the better skilled relative to the less skilled in the labor force. It seems unlikely that our estimated effect is biased upwards for reasons of unobserved favorable trends in the relative labor demand of less skilled workers.

Anticipatory effects

The benefit cut was decided already in June 1995, i.e., over half a year before the actual implementation of the policy in January 1996.[22] Is there any evidence of anticipatory behavior among the unemployed during the months preceding January 1996? We have investigated this issue by redefining the time dummy so that it kicks in already during a sequence of months (four-week periods) in the second half of 1995. Figure 6 illustrates the results of this exercise. There is clear evidence that the effect operates several months

reversal of the benefit effect if benefits are paid forever, which arguably is the case in Sweden because of the availability of labor market programs for workers at risk of benefit exhaustion.

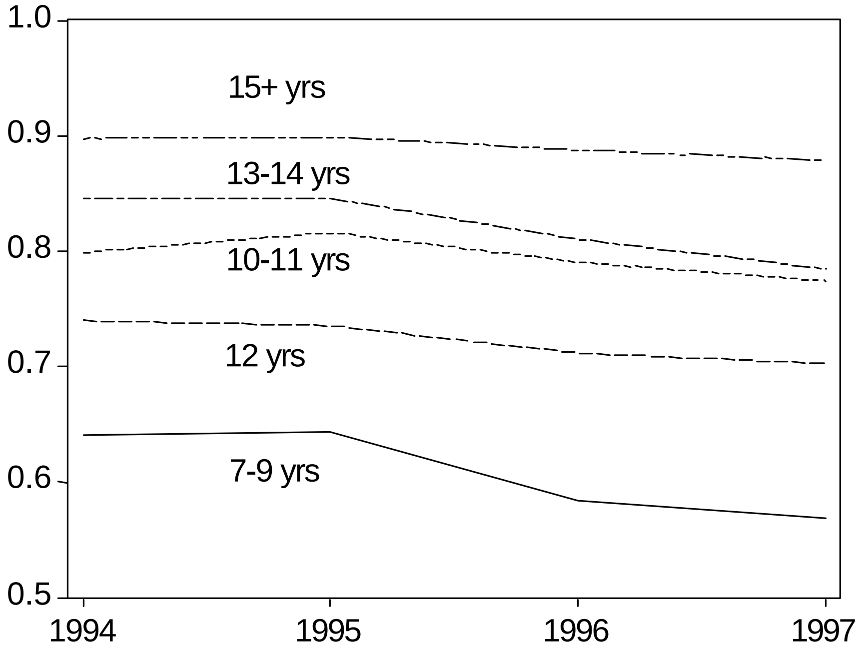
before the law change came into force. This pattern lends additional support to the claim that the benefit reform did in fact affect search behavior among the unemployed.

Table 7. The effects of excluding workers with high and low wages.

	Estimated effect (b)	Standard error
Fraction of T1 and C excluded		
0 % (full sample)	.116	*.057*
5 %	.087	*.063*
10 %	.078	*.064*
20 %	.097	*.068*
50 %	.118	*.070*

Note: The specification of column (4) in Table 4 is used.

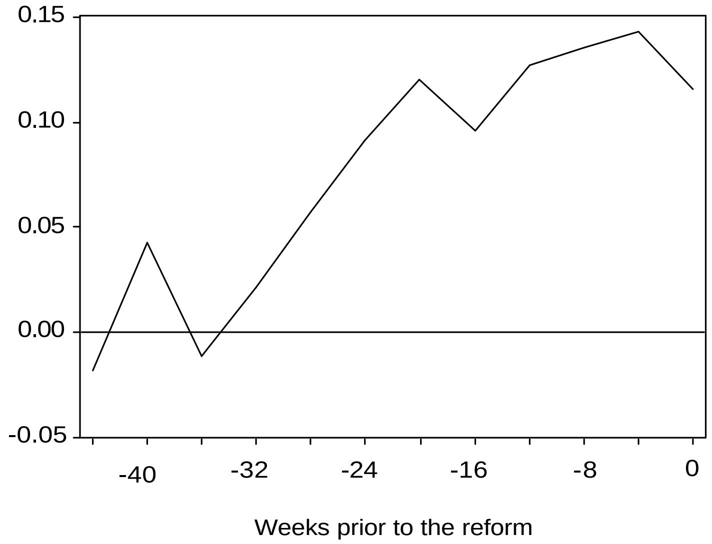
Figure 5. Employment-to-population rates by years of education 1994-1997.



Source: Labor force surveys, Statistics Sweden.

[22] The government announced the policy change earlier during the spring of 1995.

Figure 6. Anticipatory effects of the benefit cut, i.e., estimates of β for weeks prior to the 1st of January 1996.



Notes: The benefit cut was decided in June 1995 and implemented in January 1996. The estimates correspond to the specification in column (4) of Table 4. The time dummy is successively redefined and set equal to unity for up to 10 four-week intervals prior to the 1st of January 1996. The standard errors are about 0.05, deviating at most by 0.005.

6. Concluding Remarks

Our study of the benefit cut that came into force in 1996 has yielded a fairly clear result. The reduction in the replacement rate from 80 to 75 percent had a significant positive effect on the transition rate from unemployment to employment. The magnitude of the effect – a 10 increase in the exit rate – is relatively large compared to the results from earlier studies. The decision on the benefit cut was taken half a year before it was actually implemented and we find evidence of anticipatory behavior among the unemployed: there is an increase in job finding rates already several months before the law change came into force. In contrast to some other studies we do not find any effects on transitions to non-participation.

Would it be appropriate to conclude from our results that a more aggressive benefit cutting strategy would have speeded up the rebound of the Swedish labor market? There are reasons to pause before jumping to this conclusion. One issue concerns the nature of Swedish unemployment in the early 1990s and the shocks that caused it. There is little doubt than the main shock was a severe contraction of aggregate demand, in which case a large cut of benefits may be a two-edged instrument; the positive incentive effects on the supply side have to be weighed against adverse effects on aggregate demand induced by a fall in private consumption. Another issue is whether partial equilibrium results, as those obtained in this paper, are offset or reinforced in general equilibrium. There is no general theoretical presumption here, the answer being sensitive to the details of the model of equilibrium unemployment. There is a compelling argument that more generous benefits will raise wage pressure in economies where wage bargaining is pervasive, thus *reinforcing* the adverse incentive effects on job search. In models where wages are set by firms, however, the partial equilibrium results may sometimes be *offset* in general equilibrium; see Albrecht and Axell (1984) and Axell and Lang (1990).

This being said, the bulk of the empirical studies on aggregate data suggest that high replacement rates do contribute to high unemployment. For example, the results presented in Nickell (1998), based on a panel of 20 OECD countries, imply that a 10 percentage point increase in the replacement rate would raise unemployment by 13 percent, corresponding to an increase in the average EU unemployment rate in the late 1990s by roughly one percentage point. The estimates by Scarpetta (1996) are fairly similar. The exact relationship between these macro-estimates and the microeconometric estimates of benefit effects on hazard rates is, however, a largely unresolved issue.

The quasi-experimental design of the benefit reform we have investigated is due to the presence of a ceiling on the level of benefits. Existing UI systems often involve a benefit maximum of this sort, thus offering flat-rate benefits to workers with relatively high earnings and a constant replacement rate for the rest. The rationale for this kind of two-tiered benefit schedule does not appear to have been analyzed in the normative UI literature. It would be interesting to see whether future theoretical work on UI design can shed some light on the optimal *structure* of replacement rates, with explicit recognition of worker heterogeneity. Likewise, it might be worthwhile to explore the relationship between the benefit structure and the political sustainability of public unemployment insurance.[23]

APPENDIX A

The Statistical Model

Let the random variable T be the duration of unemployment until exit and define an indicator variable c that takes the value of unity if the exit occurred to the state of interest and zero otherwise. The model to be estimated is[24]

$$\text{(A1)} \quad h(t) = h_0(t)\exp\left\{m(x, z(t); \Omega) + d \cdot D_t^{96} + g_1 \cdot D^{T_1} + g_2 \cdot D^{T_2} + b \cdot DPOL_t\right\},$$

where $h_0(t)$ is the baseline hazard and $m(.)$ some function which links the control variables to the duration variable with the finite set of unknown parameters Ω. The policy variable is defined as $DPOL_t \equiv D^{T_1} \cdot D_t^{96} + \{(R - 0.75)/0.05\} \cdot D^{T_2} \cdot D_t^{96}$. The discrete time version of (A1), assuming that the hazard and the control variables do not vary within the time-intervals, is

$$\text{(A2)} \quad h_d(t) = 1 - \exp\left(-\exp\left\{m(x, z(t); \Omega) + d \cdot D_t^{96} + g_1 D^{T_1} + g_2 \cdot D^{T_2} + b \cdot DPOL_t + h(t)\right\}\right),$$

where $h(t) = \ln\left(\int_t^{t+1} h_0(u)du\right)$.

The functional form of $m(.)$ was chosen after some exploratory data analysis using complete observations only, i.e., observations where the actual duration was observed.[25] The log likelihood function, with $m(.)$ given as $m(.) = x v_1 + z(t) v_2$, for a sample of n random complete and incomplete observations on T and c is

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[23] See Casamatta et al (2000) for an interesting analysis of the political economy of social insurance in a model with heterogeneous individuals but without labor market distortions.

[24] See Meyer (1990), Narendranathan and Stewart (1993), and Carling et al. (1996) for earlier applications of this model.

[25] The precise relationship between the duration and the control variables is *a priori* unknown. The aim of the exploratory data analysis is to find transformations of the control variables that permit an additive structure of the model, thereby reducing the bias due to mis-specification. Altman and de Stavola (1994) provide a careful discussion of available techniques for duration models. Exploratory tools for ordinal and categorical variables are treated by Hoaglin, Mosteller, and Tukey (1985). For literature on non-parametric regressions, see Cleveland (1979), Cleveland, Devlin, and Grosse (1988), and Härdle (1990).

$$\ln L(v_1, v_2, d, g_1, g_2, b, h) =$$

$$\sum_{i=1}^{n}\Big\{c_i \ln\Big(1-\exp\Big\{-\exp\Big[x_i v_1 + z_i(t) v_2 + d\cdot D_{it}^{96} + g_1\cdot D_i^{T_1} + g_2\cdot D_i^{T_2} + b\cdot DPOL_{it} + h(t_i)\Big]\Big\}\Big)$$

$$-\sum_{s=1}^{t_i}\exp\Big[x_i v_1 + z_i(s) v_2 + d\cdot D_{is}^{96} + g_1\cdot D_i^{T_1} + g_2\cdot D_i^{T_2} + b\cdot DPOL_{is} + h(s)\Big]\Bigg\},$$

(A3)

where $c_i = 1$ if the duration was observed to be terminated due to exit to the state of interest. The function is maximized with respect to its arguments.[26] The baseline hazard is estimated for time-intervals of four weeks and over the span 0-72 weeks.

APPENDIX B

The Data

We have combined several different data sources for the empirical analysis. Three sources are included in LINDA, a register-based longitudinal database for Sweden.[27] These three sources are HÄNDEL, AKSTAT and IoF. HÄNDEL originates from the public employment agencies in Sweden and contains information on spells of unemployment, participation in labor market programs as well as some personal characteristics. AKSTAT includes information on benefits for unemployed individuals who are entitled to regular UI or KAS. IoF contains information on income and wealth as well as a host of data on personal and household characteristics. We have merged these data sources and appended the data with information on local unemployment rates. A detailed documentation is available on request.

HÄNDEL is the basic data source for the construction of unemployment spells. Our basic rule for sample inclusion is that the person entered unemployment during the 24-month period starting in July 1994 and ending in June 1996. We follow each person until the date of exit from unemployment or - if no exit is observed - until July 1997.

AKSTAT originates from the UI funds and includes only individuals with unemployment compensation (regular UI benefits or KAS). Among other things, AKSTAT includes information on previous earnings, the amount of benefit that the individual is entitled to and the type of benefit. We restrict our analysis to insured workers and thus require matching information from AKSTAT. We merge data from HÄNDEL and LINDA and then undertake a further merge with the IoF-data in LINDA. The variables from HÄNDEL that we use to identify entries to and exits from unemployment have no exact

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[26] Starting values are obtained from the Approximate Maximum Likelihood method (Carling, 1995), and used in conjunction with the BHHH algorithm (see Carling and Söderberg, 1998).

[27] For more information, see Edin and Fredriksson (1997) and web-page http://www.nek.uu.se/Linda/.

counterparts in AKSTAT. The basic strategy is to use the information from HÄNDEL to determine unemployment status and the length of unemployment spells. We next searched for information on benefits and previous wages in AKSTAT for each individual during their weeks of unemployment. HÄNDEL and AKSTAT have in common a personal identity code.

We include in the analysis two time-varying variables related to nonwage income, namely income from capital and the income of the spouse (if spouse is present). These two income measures are constructed from variables in the IoF data. The labor market conditions are measured at the municipality level. The local unemployment rate is defined as $u=(U+P)/(U+P+E)$, where U is the number of persons registered as unemployed at the employment agencies, P the number of persons in labor market programs, and E the number of employed in the municipality. The unemployment rate is treated as time varying and is measured on a monthly basis in the data.

APPENDIX C

Estimates of the baseline (log) hazard parameters. Asymptotic standard errors in italics.

Time-interval (weeks)	Exits to employment		Exits to labor market programs		Exits to non-participation	
1-4	-3.253	*0.498*	-6.463	*0.705*	-4.957	*0.771*
5-8	-2.826	*0.498*	-5.626	*0.703*	-4.023	*0.768*
9-12	-2.687	*0.497*	-5.202	*0.703*	-2.599	*0.766*
13-16	-2.729	*0.498*	-4.936	*0.703*	-2.358	*0.766*
17-20	-2.752	*0.498*	-4.914	*0.702*	-3.378	*0.769*
21-24	-2.710	*0.498*	-4.892	*0.703*	-3.486	*0.770*
25-28	-2.675	*0.498*	-4.775	*0.704*	-3.385	*0.770*
29-32	-2.715	*0.499*	-4.684	*0.704*	-3.230	*0.771*
33-36	-2.803	*0.500*	-4.601	*0.706*	-2.891	*0.771*
37-40	-2.924	*0.501*	-4.507	*0.705*	-3.056	*0.773*
41-44	-2.817	*0.501*	-4.414	*0.706*	-3.247	*0.777*
45-48	-3.116	*0.505*	-4.484	*0.706*	-2.891	*0.776*
49-52	-2.953	*0.504*	-4.395	*0.706*	-3.087	*0.779*
53-56	-2.928	*0.507*	-4.370	*0.707*	-2.973	*0.778*
57-60	-2.608	*0.506*	-3.938	*0.706*	-2.928	*0.784*
61-64	-2.405	*0.507*	-3.611	*0.706*	-2.539	*0.785*
65-68	-2.332	*0.515*	-3.847	*0.713*	-2.335	*0.787*
69-72	-2.362	*0.524*	-4.321	*0.728*	-2.446	*0.799*

Note: The estimates correspond to the specifications in Table 5 with all covariates set to zero.

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