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# Do Benefit Cuts Boost Job Findings?

## Swedish Evidence from the 1990s[§]

by

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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 slow and shaky. Unemployment in 1998 stood at 6.5 percent and 71.6 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

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

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]

The remainder of the paper is organized as follows. 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

[2] The reason for our focus on the 1996-reform is that this change is most suitable to analyze as a natural experiment. The changes that took place in 1997 involved benefit increases for all insured workers, thus making it difficult to identify treatment and control groups. The 1993-change could be analyzed as natural experiment if one is willing to regard workers without UI compensation as a control group, an approach adopted in Harkman (1997). However, the data for this earlier period contain less information on personal characteristics. By focusing on insured workers we can also compare treatment and control groups that are more similar than workers with and without UI compensation.

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 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.

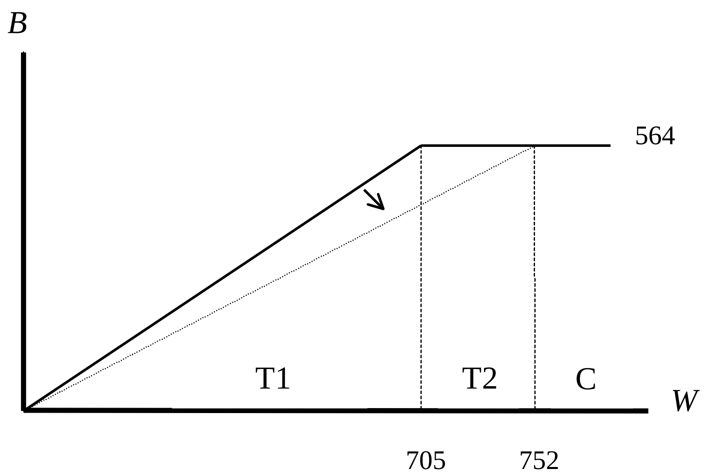
The ceiling on the benefit level – 75 percent of 16 500 SEK per month in 1996 – means that actual compensation rates can be much lower than the maximum rates. It has been estimated that 75 percent of all full-time *employees* had monthly earnings exceeding 16500 SEK in 1996 (see SOU 1996:51). However, 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. There is a ceiling on benefits at 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 in 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 repetitively 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 loosing 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 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.

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[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. See also Edin et al (1999), where the effect is less robust.

## 4. The Data

We have combined a number of different data sources for the empirical analysis. Three sources are included in the so-called LINDA database, a register-based longitudinal database for Sweden.[5] These three sources are HÄNDEL, AKSTAT and IoF. HÄNDEL originates from the public employment agencies in Sweden and contains the basic information on the length of spells on unemployment as well as some data on 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 as described in detail in Appendix B. We have also appended the data with information on regional unemployment rates.

HÄNDEL is a data set that only includes information on unemployment registered at the employment agencies. However, survey evidence indicate 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.

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 escaped 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

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

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 market programs.[6]

The evolution of the empirical hazard rates for exits to employment are shown in Figure 2. The rates are computed for time intervals of four weeks. We note that the job finding rates are generally higher among workers in the control group – who have relatively low replacement rates – compared to exit rates among workers in the treatment groups (with higher replacement rates). 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

[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.

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** | | | |
| Wage per day (month), SEK | 560.2 (12 324) | 729.3 (16 045) | 854.5 (18 799) |
| 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.

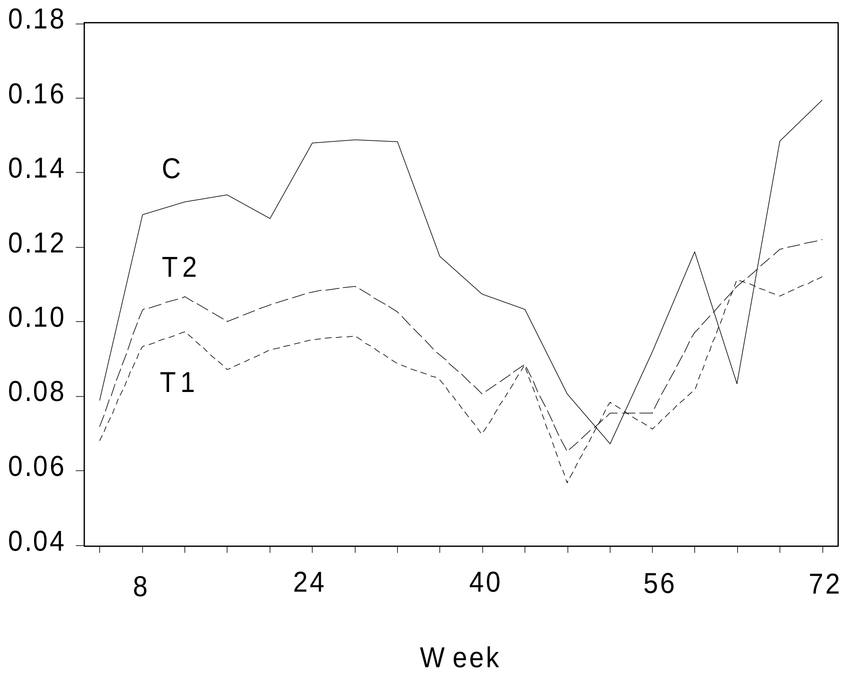
| | Before 1996 | After 1996 |
|---|---|---|
| **Interval** | **Percent in interval** | |
| 0.80 | 72.33 | 0.0 |
| [0.775, 0.80) | 3.10 | 0.0 |
| [0.75, 0.775) | 5.34 | 80.78 |
| [0.725, 0.75) | 3.83 | 3.83 |
| [0.70, 0.725) | 3.75 | 3.75 |
| [0.65, 0.70) | 5.71 | 5.71 |
| [0.60, 0.65) | 3.05 | 3.05 |
| [0.50, 0.60) | 2.17 | 2.17 |
| [0.40, 0.50) | 0.62 | 0.62 |
| < 0.40 | 0.076 | 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 |

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.

Figure 2. Job finding rates for the control and treatment groups.




## 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.[7] 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

[7] 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).

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 affect 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 group(s) and the control group before and after the policy change.[8] 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. This "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:

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

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 $\Omega$ 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

[8] Hunt (1995) and Steiner (1997) have adopted a similar methodology in studies of changes of the German UI system.

and after the 1st of January 1996. The effect of the policy change is given by the coefficient on the interaction variable, i.e., $\lambda$.

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 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:

$$(2) \qquad h(t) = h_0(t)\exp\begin{pmatrix} m(x, z(t);\Omega) + \delta \cdot D_t^{96} + \gamma_1 \cdot D^{T_1} + \gamma_2 \cdot D^{T_2} \\ +\beta \cdot \left[D^{T_1} \cdot D_t^{96} + \{(R-0.75)/0.05\} \cdot D^{T_2} \cdot D_t^{96}\right] \end{pmatrix}.$$

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

The effect of the benefit reform is given by the coefficient in front of the interaction terms in (2), i.e., $\beta$. 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.

The variable of main interest is *DPOL* in the fourth line of the table. The estimates of its coefficient, $\beta$, 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.

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 1. The coefficient on T2 is insignificantly different from zero, although the raw hazards indicated lower exit rates than for the control group.

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) and Edin et al (1999); in these studies, the escape rates to employment were

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

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| $D^{96}$ | -.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, where 53.2 percent are right censored. The reference individual is a single male Swedish citizen in the control group. He has no children, became unemployed in the second quarter and has missing values on education and work experience.

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 other studies as opposed to the mid-1990s in the present study.[9] Note also that this paper uses a sample that is restricted to insured workers, whereas the two other studies have pooled all categories of unemployed.

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, 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, we note that non-Nordic immigrants have job finding rates that are more than 40 percent lower than the exit rates for Swedish citizens. Having young children (age 15 or less), implies much lower exit rates, whereas older children are associated with higher exit rates. 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.

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

[9] 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.

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) | |
|---|---|---|---|---|---|---|
| $D^{96}$ | -.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* | .413 | *.101* | -.056 | *.076* |
| Some work experience | .605 | *.094* | .605 | *.098* | -.080 | *.072* |
| Long work experience | .699 | *.093* | .699 | *.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 | 0.468 | | 0.238 | | 0.243 | |
| ln L | 29 498.5 | | 16 003.4 | | 14 137.4 | |

Notes: There are 18 429 spells, where 53.2 percent are right censored. The reference individual is a single male Swedish citizen in the control group. He has no children, became unemployed in the second quarter 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.

risk model can be estimated by treating exits to states other than that of interest as censored observations at the relevant point in time.[10] Table 5 shows the results of the estimations. There is no significant effect of the policy change on exits to labor market programs and non-participation. Among other results, we note 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.

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.[11]

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 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

[10] 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.

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.[12]

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.

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[11] 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).

[12] 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 estimates are reported for exits out of welfare.

We have checked for possible age differences in the benefit effect by a number of alternative specifications. 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.[13] The precise reasons for these differences between age groups remain as a largely open question, however.[14]

Table 6. Benefit effects by age.

| | Estimate | *Std error* | Estimate | *Std error* |
|---|---|---|---|---|
| Under 25 | .210 | *.036* | .171 | *.040* |
| 25-44 (reference group) | 0 | | 0 | |
| 45-54 | -.087 | *.032* | -.080 | *.034* |
| *DPOL* | .115 | *.056* | .089 | *.059* |
| (Under 25)·*DPOL* | | | .215 | *.086* |
| (45-54)·*DPOL* | | | -.082 | *.098* |

Note: 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

[13] 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.

[14] 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.

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

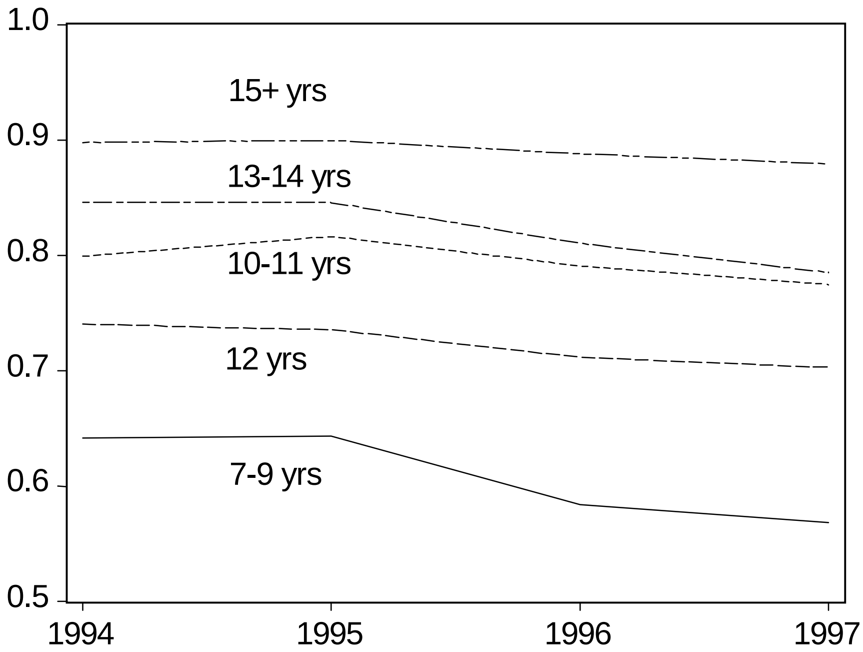
| | Estimated effect ( $\beta$ ) | Standard error |
|---|---|---|
| **Fraction of T1 and C excluded** | | |
| 0 % (full sample) | .116 | *.057* |
| 5 % | .087 | *.063* |
| 10 % | .078 | *.064* |
| 20 % | .097 | *.068* |
| 50 % | .118 | *.070* |

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

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 3). 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 3 gives little support for this possibility; if anything, the data suggest the reverse. Indeed, there is a widespread view

reversal of the benefit effect if benefits are paid forever, which arguably is the case in Sweden (because 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.

Figure 3. Employment-to-population rates by years of education 1994-1997.




Source: Labor force surveys, Statistics Sweden.

*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. 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 4 illustrates the results of this exercise. There is clear evidence that the effect operates several months 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.

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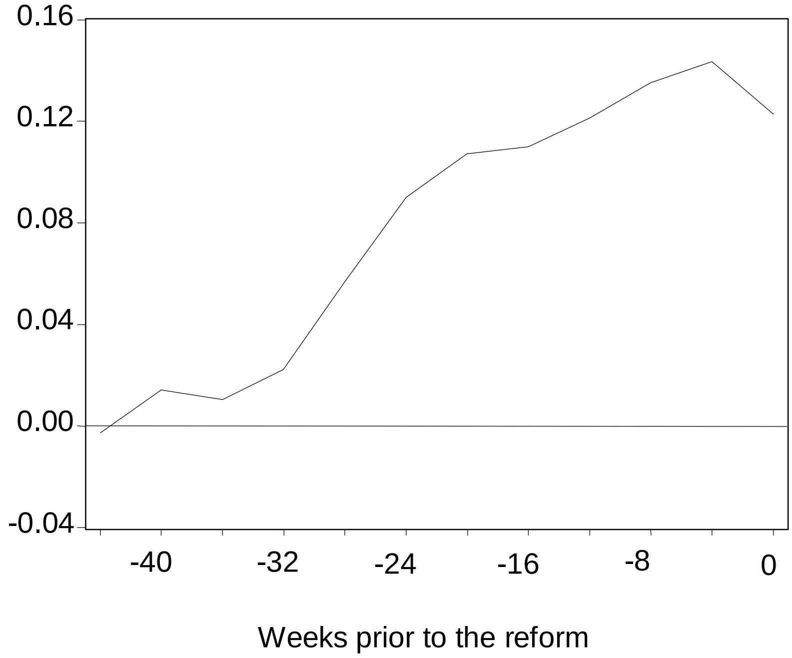
of the availability of labor market programs for workers at risk of benefit exhaustion).

Figure 4. Anticipatory effects of the benefit cut, i.e., estimates of $\beta$ for weeks prior to the 1st of January 1996. Three-point moving averages.




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 of the order 0.05.

## 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 and relatively large effect on the transition rate from unemployment to employment. The decision on the benefit cut was taken half a year before it took effect 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 at least two reasons to pause before jumping to this conclusion. The first 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. The second issue is whether partial equilibrium results, as those obtained in this paper, are offset or reinforced in general equilibrium. There is no general presumption here; the answer is sensitive to the precise details of the model of equilibrium unemployment.[15] But there is a compelling argument that more generous benefits will raise wage pressure in an economy where wage bargaining is pervasive, thus reinforcing the adverse incentive effects on job search.

This being said, there are a number of macro-studies of unemployment that suggest that high replacement rates contribute to high unemployment.[16] The exact relationship between these macro-estimates and the microeconometric estimates of benefit effects on hazard rates is, however, a largely unresolved issue.

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[15] In models where wages are set by firms, the partial equilibrium results can be overturned in general equilibrium as in the models of Albrecht and Axell (1984) and Axell and Lang (1990).
[16] Two examples are Nickell (1998) and Scarpetta (1996)

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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[17]

(A1) $$h(t) = h_0(t)\exp\left\{m(x, z(t); \Omega) + \delta \cdot D_t^{96} + \gamma_1 \cdot D^{T_1} + \gamma_2 \cdot D^{T_2} + \beta \cdot DPOL_t\right\},$$

where $h_0(t)$ is the baseline hazard, $m(.)$ is some function which links the factors to the duration variable with the finite set of unknown parameters $\Omega$, and 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 factors do not vary within the time-intervals, is

(A2) $$h_d(t) = 1 - \exp\left[-\exp\left\{m(x, z(t); \Omega) + \delta \cdot D_t^{96} + \gamma_1 D^{T_1} + \gamma_2 \cdot D^{T_2} + \beta \cdot DPOL_t + \eta(t)\right\}\right],$$

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

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.[18] The log likelihood function, with $m(.)$ given as $m(.) = x\varpi_1 + z(t)\varpi_2$, for a sample of $n$ random observations on $T$ and $c$ is

$$\ln L(\varpi_1, \varpi_2, \delta, \gamma_1, \gamma_2, \beta, \eta) =$$
$$\sum_{i=1}^{n}\left\{c_i \ln\left(1 - \exp\left\{-\exp\left[x_i\varpi_1 + z_i(t)\varpi_2 + \delta \cdot D_i^{96} + \gamma_1 \cdot D_i^{T_1} + \gamma_2 \cdot D_i^{T_2} + \beta \cdot DPOL_{It} + \eta(t_i)\right]\right\}\right)\right.$$
$$\left.-\sum_{s=1}^{t_i}\exp\left[x_i\varpi_1 + z_i(t)\varpi_2 + \delta \cdot D_i^{96} + \gamma_1 \cdot D_i^{T_1} + \gamma_2 \cdot D_i^{T_2} + \beta \cdot DPOL_{It} + \eta(s)\right]\right\},$$

(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.[19] The baseline hazard is estimated for time-intervals of four weeks and over the span 0-72 weeks.

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

[18] Altman and de Stavola (1994) provide a careful discussion on 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).

[19] 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).

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.[20] 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. We have also appended the data with information on regional unemployment rates.

HÄNDEL

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.

Information on the date of registration at the employment agency is given by the variable *AKTDM* (*inskrivningsdatum*). We required that the person entered the register without a job and use the variable *SKAT* (*sökandekategori*) to make sure that this is the case. In particular, the rule to identify spells of unemployment is $SKAT \in [11,12,13,14]$, which essentially means that the person has no job and is looking for one. To construct spells of unemployment we used the date of entry to the register as unemployed, given by the variable *INSKADM* (*datum för påbörjad sökandekategori*), and the date for exiting unemployment as given by the variable *UTSKADM* (*datum för avslutad sökandekategori*). The number of days of unemployment in a given spell is given by the variable *ANTDGR* (*antal dagar i sökandekategorin*). Some obviously erroneous observations with negative values of *ANTDGR* were deleted. Obvious coding errors were also present in some other cases: if two or more spells had the same starting date, as given by *INSKADM*, we kept only the last spell.

The resulting data set often included several unemployment spells per individual. We restricted the analysis to the first spell of unemployment. Moreover, we undertook a slight adjustment of the length of spells given by *ANTDGR* in order to disregard very short interruptions of the spells. In particular, we concatenated two adjacent spells – separated by a short break – into one "long" spell, provided that the break did not exceed seven days. Our measure of the length of unemployment spells thus gives slightly longer spells than what is implied by *ANTDGR*. Such adjustments have been made in about two percent of the cases. We also concatenated spells where the person's first period of registration at the employment agency is a short non-unemployment spell – of a length

[20] More specifically, we use LINDAp, a representative sample of Swedish and foreign citizens living in Sweden. For more information on LINDA, see the web-page http://www.nek.uu.se/Linda/

not exceeding seven days – immediately followed by an unemployment spell, i.e., $\text{SKAT} \in [11,12,13,14]$. This procedure has been applied in about one percent of the cases.

We merged these data from HÄNDEL with the IoF-data in LINDA. (Note that the original data from HÄNDEL cover the whole registered unemployed *population* whereas IoF is a random *sample* of people living in Sweden.) The resulting sample of unemployed individuals included 57 030 individuals, whereas the corresponding population in HÄNDEL include about 146 000 persons. We furthermore dropped 3190 cases where the coded start of a registration period at the employment agency (given by *AKTDM*) is not the same as the coded start of searching in a given category (given by *INSKADM*). We also excluded disabled people (3 481 individuals) as well as 54 individuals with missing information on the type of UI compensation, as given by the variable *KASNR* in HÄNDEL.

Merging HÄNDEL and AKSTAT

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. AKSTAT has a different structure than HÄNDEL and the matching of information from the two data sources is a nontrivial exercise.

The variables from HÄNDEL that we use to identify entries to and exits from unemployment have no exact 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 (*BIDNR*). To link the unemployment spells with information on benefits we used *INSKADM* and *UTSKADM* from HÄNDEL together with a variable from AKSTAT with information on weeks with benefit payments (*BDEKLV*).

We restrict our analysis to insured workers and thus required matching information from AKSTAT. 22 265 cases were non-matching and thus excluded. We also excluded cases where the UI compensation is not "regular" UI, but rather compensation for participation in certain labor market programs (2184 cases).[21] Moreover, we required that paid-out benefits per day during a week should not exceed the ceiling of 564 SEK (a condition violated in 2996 cases).[22] We also excluded KAS-recipients[23] (2384 cases) and individuals older than 54 years (971 cases).

[21] The condition is *BERSTYP*>2; *BERSTYP* contains information on the type of benefit received.

[22] This can happen because of delayed payments. The rule for exclusion is *TBEL*/*BANTDGR*>564.

[23] This is determined by the condition *BTYP*=1. There is one exception here. Some people received "KAS med dagpenning" (KAS with regular UI), as given by the variable *KASNR*=1. They are included

The values of variables like previous wages (*TLON*) and the amount of benefits that a worker is entitled to (*TDAGPENG*) may in general change over the weeks of unemployment as given by HÄNDEL. A spell of unemployment according to our measure may be associated with more than one "AKSTAT-spell" of UI compensation. We have chosen to use AKSTAT-observations from the first "AKSTAT-spell" that matches our spell of unemployment.

<u>Wages and replacement rates</u>

Our empirical strategy requires that we can classify the individuals into treatment and control groups on the basis of their pre-unemployment wages. The critical wage levels that separate the three groups of interest – T1, T2 and C – are 705 and 752 SEK per day, as illustrated in Figure 1 in the main text. The ceiling on benefits kicks in at 705 SEK before the benefit reform of the 1st of January 1996 and at 752 SEK thereafter. Measurement errors in the wage will translate into errors of classification of individuals into treatment and control groups. We have tried to correct for obvious sources of measurement errors in the wage information given in AKSTAT. To that end we used information on the statutory rules for replacement rates as well as the data in AKSTAT on previous wages, normal working hours and benefits.

First, we computed a measure of the pre-unemployment wage per day, *W*, and check for obvious errors in this measure caused by mismatch between the information on the amount of earnings, *TLON*, and the type of wage, *BLONKOD*; the latter variable contains codes for hourly, daily, weekly and monthly pay. For example, if *TLON* is supposed to represent monthly earnings, we have $W$=*TLON*/22. In some cases, the observations on *TLON* and *BLONKOD* suggest that there are coding errors in *BLONKOD*; a monthly wage may actually be coded as an hourly wage or vice versa. In 56 cases where such errors were obvious, we imputed a corrected measure of *W*.

We next checked if the replacement rates implied by AKSTAT, i.e., $R$=*TDAGPENG/W*, are consistent with the statutory rules before and after the benefit reform. The consistency requirements are as follows:

Before the reform: $R$=0.80 for $W \leq 705$, $R$<0.80 otherwise.

After the reform: $R$=0.75 for $W \leq 752$, $R$<0.75 otherwise.

When these requirements are not fulfilled, we consider the possibility that *TLON* is reported as fulltime earnings despite the fact that previous normal working hours, as given by the variable *BARBTID*, have been less than fulltime. In 394 cases, where such

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if *TDAGPENG*>245 (or *TDAGPENG*>230 after the 1st of January 1996) since they for our purposes are comparable to those who are insured through the UI funds.

mismatches between *TLON* and *BARBTID* were obvious, we adjusted *TLON* and *W* and check if the implied replacement rates fulfill the stated requirements.

The checks described resulted in a further exclusion of 1076 observations with inconsistent data on previous wages and replacement rates. The final data set thus consisted of 18 429 individuals**.**

Local labor market conditions

The labor market conditions are measured at the municipality level. The local unemployment rate is defined as $u \equiv (U+P)/(U+P+E)$, where $U$ is the number of persons registered as unemployed at the employment agencies, $P$ the number of persons in labor market programs (labor market training, relief jobs, job introduction projects, replacement schemes, youth practice schemes), and $E$ the number of employed in the municipality.[24] The unemployment rate is treated as time varying and is measured on a monthly basis in the data.

Income variables

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. IoF consists of annual information from the income registers. We have data for three years: 1994, 1995 and 1996. Information is not available for all individuals for all three years. We have therefore imputed missing observations by using values for the previous or the subsequent year. The values for 1996 are carried over to 1997 because of lack of information from this year.

We define the weekly real after-tax income from capital as:

$$YCAPITAL_{it} = [(1/52) \cdot KKAPS_{it} \cdot (1\text{-}0.3)]/(1+g)^t$$

*KKAPS* (set equal to zero whenever a missing value) stands for taxed income from capital and $g$ is the CPI-inflation per week and averages 0.0002691 for the period January 1994 until June 1997. Time $t$ refers to the calendar week starting from June 1994. The tax rate on income from capital is 30 percent.

The weekly real after-tax income of the spouse, including income from labor as well as capital, is defined as

[24] Information on the number of employed at the municipality level is available as annual averages from Statistics Sweden (the so called Årssys-data).

$$YSPOUSE_{it} = (1/52) \cdot \{(CSFVIH_{it} - CSFVI_{it} - 325) \cdot (1\text{-}trate) + [CFVIKIH_{it} - CFVIKI_{it} - (CSFVIH_{it} - CSFVI_{it})] \cdot (1\text{-}0.3)\}/(1+g)^t$$

where

| | |
|---|---|
| *CSFVI* | labor income of the individual (*sammanräknad förvärvsinkomst*), |
| *CSFVIH* | labor income of the family (*familjens sammanräknad förvärvsinkomst*), |
| *CFVIKI* | labor and capital income of the individual (*förvärvs- och kapitalinkomst*) |
| *CFVIKIH* | labor and capital income of the family (familjens *förvärvs- och kapitalinkomst*) |
| *trate* | the tax rate in the municipality. |

We impute only the local tax rate on labor income. This tax varies across municipalities and is proportional over a basic deduction set to 325 on a weekly basis. Tax progressivity enters through the state income taxes. These taxes are only relevant for wage earners in the median to high-income brackets. However, the actual tax rate is obviously endogenous and cannot be used as a covariate.

APPENDIX C

Estimates of the baseline 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.