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The effect of unemployment benefits on re-employment rates: evidence from the Finnish UI-benefit reform

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The effect of unemployment benefits on re-employment rates: evidence from the Finnish UI-benefit reform*

by

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Abstract

In January 2003, the unemployment benefits in Finland were increased for workers with long employment histories. The average benefit increase was 15 per cent for the first 150 days of the unemployment spell. In this paper we evaluate the effect of the benefit increase on the duration of unemployment by comparing the changes in the re-employment hazard profiles among the unemployed who became eligible for the increased benefits to the changes in a comparison group whose benefit structure remained unchanged. We find that the benefit increase reduced the re-employment hazards at the beginning of the unemployment spell. The effect disappears after the eligibility period for the increased benefit expires.

Keywords: Unemployment insurance, duration models
JEL-codes: J64, J65

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1 Introduction

In January 2003, the unemployment insurance (UI) benefits in Finland increased for workers with long employment histories. The average benefit increase was 15 per cent for the first 150 days of unemployment spell. The benefit increase was part of a reform aiming to simplify the rules regarding unemployment benefits. At the same time a severance pay system that had existed until the end of 2002 was abolished.

The Finnish benefit reform provides a clean policy experiment that can be used to evaluate the effect of UI benefits on the re-employment rates. The reform took place at a time when the macroeconomic environment was stable with aggregate unemployment rates almost constant over the four-year period that we use in the analysis. In addition, no other major policy reforms that might have had an effect on the re-employment rates were implemented simultaneously. These two facts together minimise the risk that our results would be contaminated by macroeconomic cycles or other policy changes.

The eligibility for increased UI benefits was based on the length of the previous work history and on the length of membership of a UI fund. This allows us to estimate the effect of the benefit increase by comparing the changes in the job-finding rates after the reform in the "treatment group" that became eligible for higher benefits to the changes in a "comparison group" whose benefit system was unchanged but otherwise was rather similar to the treatment group. This difference-in-differences approach overcomes the fundamental identification problem caused by the fact that UI benefits are linked to previous earnings. Previous earnings, again, may well be correlated with other factors affecting re-employment rates. Lack of independent variation in UI benefits in typical cross-section data makes it very difficult to disentangle the effect of the benefit level from other factors correlated with previous earnings and re-employment rates.[1]

[1] Most previous Finnish studies on the effect of UI-benefits on the duration of unemployment rely exclusively on cross-section variation in the replacement rates (e.g. Kettunen, 1993; Kyyrä, 1999). Variation in benefit rules has been used by Uusitalo & Moisala (2003) but due to small sample sizes, measurement problems and volatile economic situation at the time of benefit change their estimates were not very precise.

Our paper is related to several previous papers that identify the causal effects of the level of unemployment benefits by using data on policy reforms that lead to different changes in benefits in different groups of unemployed workers. Similar analyses have been performed earlier in Germany (Hunt 1995), Sweden (Carling, Holmlund & Vejsiu, 2001; Bennmarker, Carling & Holmlund, 2005), Austria (Lalive, van Ours & Zweimüller, 2006) and the New York State (Meyer & Mok, 2007). Compared with these papers our set-up differs in two ways. First, we identify the effect of UI benefits based on differences in the benefit changes across groups that differ mainly in the length of previous work experience, while most of the others are based on different changes across groups that differ in the pre-unemployment wage. Second, in our case the benefit increase involved only new entrants to unemployment, which makes it easier to account for any possible anticipatory effects.

Like the authors of some of the previous papers we also evaluate the effect of the benefit increase on the entire hazard profile of exiting from unemployment into employment. We specify a flexible baseline hazard function and allow the effect of the benefit increase to vary across the elapsed duration of unemployment.

We have access to administrative data on the dates of entry into and exit out of unemployment. Our data also include detailed information on the benefits, reported by the UI funds themselves. These data include the daily amounts of benefits, the dates when the benefits are paid out and, importantly, administrative information on the remaining benefit eligibility at the end of each quarter. The data also contain information on all variables that determine the eligibility for increased benefits though there are some clear classification errors. We address this problem by the two-sample IV approach used previously in a different context by Angrist & Krueger (1992) and Björklund & Jäntti (1997).

We find that the increase in the unemployment benefits had a large negative effect on the job-finding rates during the first months after entry into unemployment. However, the effect diminishes over time so that after the first 250 days the re-employment hazard is actually higher in the group whose benefits were increased than in the comparison group. The point estimates, therefore, suggest that the benefit increase might have substantial entitlement effects but the hazard estimates at higher durations are very imprecise. In addition, our results do not suggest that the unemployed anticipate the changes in benefits; the re-employment hazard in the treatment group increases only after the higher benefits have already expired.

The remaining part of this paper is organised as follows. In *Section 2*, we present the details of the Finnish unemployment benefit system and the 2003 benefit reform. *Section 3* describes the data and *Section 4* the empirical methods. The main results are presented in *Section 5*. Extensions and robustness checks follow in *Section 6*. *Section 7* concludes.

2 The Finnish unemployment benefit system

The Finnish unemployment benefit system consists of an unemployment allowance paid by the unemployment insurance funds and a flat-rate labour market subsidy paid by the State through the Social Insurance Institution. Eligibility for the unemployment allowance requires that the applicant has been employed for at least 43 weeks during the past 28 months before entering unemployment. Those unemployed workers who belong to a UI fund receive earnings-related benefits, and the non-members receive the flat-rate basic allowance. The unemployed who do not fulfil the employment condition or who have exhausted their UI benefits are eligible for the labour market subsidy. This labour market subsidy is means-tested and depends on the earnings of the other family members. In 2002 the full rate of both the labour market subsidy and the basic unemployment allowance without child supplements was 22.75€ per day, or 21 per cent of the median wage.

The unemployed who fulfil the employment condition and have been members of an UI fund for at least ten months before becoming unemployed are eligible for an earnings-related allowance. This consists of a basic component equal to the basic allowance and an earnings-related component that is 45 per cent of the difference between the previous daily wage and the basic component. There is no cap in the benefit level but the benefits are regressive so that monthly wages exceeding 2,047€ (in 2002) increase the benefits only by only 20 per cent of the exceeding amount. For a median earner (2,300€/month) the earnings-related benefits are 52 per cent of the pre-unemployment wage. For a low-income earner (1,500€/month) the replacement rate is 60 per cent and for a high income earner (4,000€/month) 38 per cent. In 2002, the average earnings-related benefit was 41.30€ per day.

The earnings-related unemployment allowance can be paid for five days per week up to 500 days after which those who are still unemployed may receive the labour market subsidy. At the end of 2002, a total of 130,000 persons were receiving the earnings-related allowance, 19,000 the basic unemployment allowance and 151,000 the labour market subsidy.

An important feature of the Finnish Unemployment benefit system is a benefit extension for those who are over 55 when they become unemployed. These unemployed workers can receive earnings-related unemployment benefits up to age 60 and then apply for an unemployment pension. This benefit extension has dramatic effects for the unemployment rates for those over 55. (Hakola & Uusitalo, 2005; Kyyrä & Wilke, 2007). To make sure that the changes in the early retirement schemes do not affect our estimates regarding the changes in the UI benefits we exclude all persons over 55 from the analysis.

2.1 The 2003 reform

On January 1st 2003 those unemployed workers who had lost a permanent job for "economic or production-related reasons" and who had been members of an UI fund for at least five years before losing their job, and who had at least 20 years of employment history, and had not received severance pay during the past five years became eligible for increased earnings-related benefits.

The reform increased the earnings-related component of the unemployment allowance from 45 to 55 per cent of the difference between the daily wage and the basic allowance. The increase also affected the higher earnings bracket. There the earnings-related component increased from 20 to 32.5 per cent of the wages exceeding the threshold. The increased benefits could be paid up to 150 days, after which those still unemployed were eligible for the usual earnings-related benefits.

Figure 1 displays the effect of the reform on the unemployment benefits by plotting the monthly UI benefits against the pre-unemployment monthly wage in 2003. On average, the reform increased the unemployment benefits for those unemployed workers who were eligible by 8.72 euros per day i.e. by about 15 per cent. The replacement rate for an eligible median earner increased from 52 to 60 per cent. The increases in the replacement rates were larger for high-income earners and smaller for low-income earners.

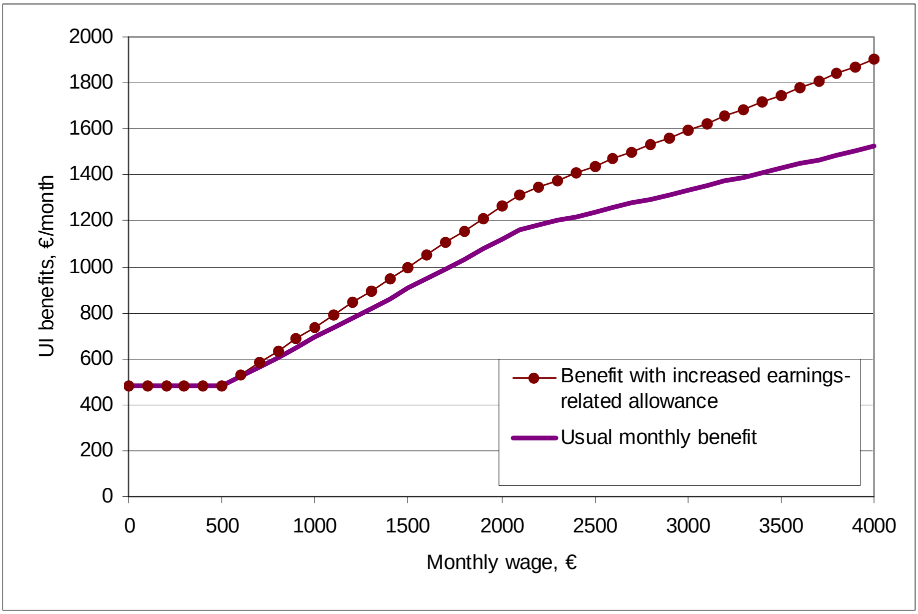

Figure 1 Earnings-related UI-benefits as a function of pre-unemployment wage

Figure 2 illustrates the time profile of the unemployment benefits for the median earner before and after the reform. For the unemployed who are not eligible for the increased earnings-related benefits the replacement rate is 52 per cent for the entire 500-day eligibility period. After 500 days the unemployed can receive labour market support, which implies a drop in the replacement rate to 21 per cent for the median earner. The reform increased benefits for the unemployed who were eligible for the increased earnings-related benefits over the first 150 days. For this group the reform creates a declining time sequence of benefits where the replacement rate for a median earner is 60 per cent for the first 150 days, decreases then to 52 per cent and decreases again to 21 per cent after 500 days of unemployment.

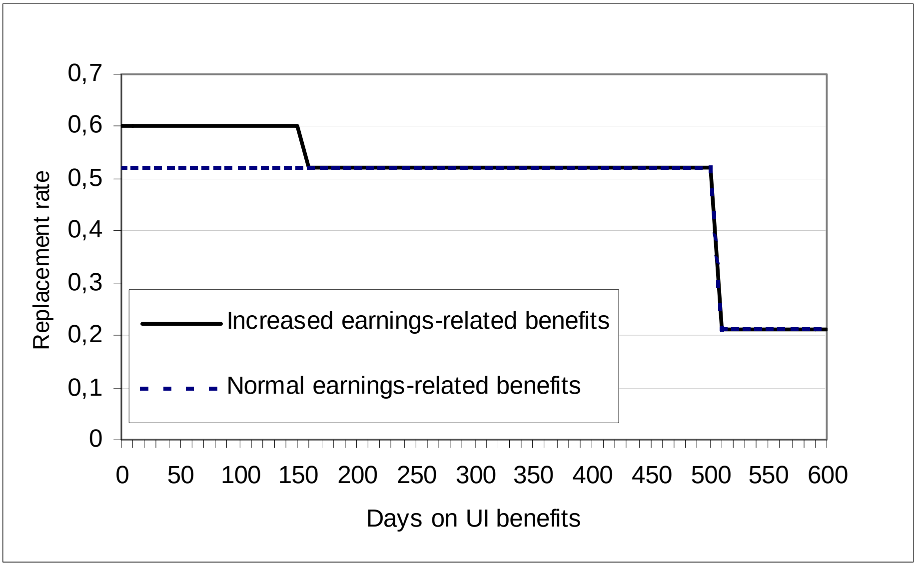

Figure 2 Replacement rate for a median earner without children

According to the government proposal to Parliament the main motivation for the changes that took place in 2003 was to simplify legislation that governed the unemployment benefit system. In this spirit, it was proposed that a severance pay system[2] that existed prior to 2003 would be merged into the unemployment benefit system. The government proposal noted that the severance pay system was created in 1970, when the unemployment insurance benefits were much lower and not all workers were covered by the unemployment insurance system. The proposal stated that the severance pay system had become a separate and unnecessary additional benefit.

The government proposed replacing severance pay with higher earnings-related benefits for the first 130 days of the unemployment spell. The increase in benefits was calculated so that the expected direct cost for the UI funds would be unchanged. As only the unemployed with long work histories were eligible for severance pay, increased benefits were also tied to the length of the

[2] Severance pay was a lump sum payment for the workers who had lost a permanent job due to plant closing or downsizing and whose re-employment was expected to be difficult due to "age or other reasons". The lower age limit was 45. The size of the severance pay depended on age, previous earnings and number of years employed with somewhat different rules in different sectors. On average, severance pay corresponded to roughly one month's pay.

previous work history. Parliament eventually changed the proposal so that the length of the increased benefit period was extended to 150 days.

2.2 Other simultaneous changes

Change in the unemployment benefit system rarely takes place in isolation. Other macroeconomic changes and other changes in legislation that are implemented simultaneously may also affect the changes in unemployment duration. As noted by, for example, Card & Levine (2000) and Lalive & Zweimüller (2004), an increase in benefits may also be an endogenous policy response to an increase in unemployment. The effect may also work in the opposite direction. Increasing unemployment may force the government to curb unemployment benefits in order to reduce the effects of increasing unemployment on the government budget. Both of these mechanisms would make the benefit level endogenous with respect to the re-employment probabilities and cause a bias in the estimated effect of the benefit change.

Finnish economic development during the past twenty years has been extremely volatile. Starting from a very low level of about three per cent in 1990, the unemployment rate rose rapidly to around seventeen per cent in 1994. After that, unemployment declined to around nine per cent in 2001. Then the decline halted, and around the date when the UI reform was implemented the unemployment rate had been quite stable for two years. Seasonally adjusted unemployment remained very close to nine per cent from the beginning of 2001 to the summer of 2004. The unemployment rate did not start to decrease until towards the end of 2004. This is important for our analysis because it indicates that the increase in UI benefits in January 2003 was not a response to worsening re-employment opportunities but can safely be treated as an exogenous event with respect to job-finding rates.

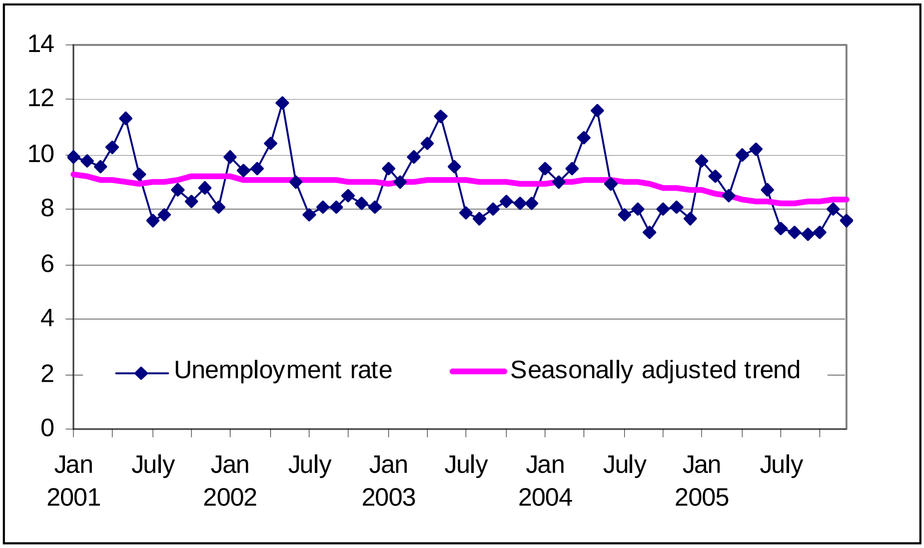

Figure 3 The monthly unemployment rate and its seasonally adjusted trend between 2001 and 2005

Source: Labor Force Surveys, Statistics Finland

Other changes in legislation that took place around the reform date had to do with an increase in the general benefit level and loosening of the employment condition. As we argue below, neither of these changes should have major impacts on our estimates for the reform effects.

Earnings-related benefits increased for all unemployed persons on March 1st 2002, ten months before the UI benefit reform that we analyse in this paper. This change increased the earnings-related component from 42 per cent to 45 per cent of the difference between the daily wage and the basic allowance. Since the change affected all the unemployed, its effects can be accounted for by using a difference-in-differences approach. We also experimented by restricting the sample so that only those who entered unemployment after March 1st 2002 were included in the sample, with no effects on the results.

In 2002, the general eligibility requirement for the unemployment allowance was that the unemployed should have 43 weeks (about 10 months) of employment history during the previous 2 years and 4 months before the start of the unemployment spell. In 2003, this condition was loosened so that after exhaustion of the 500-day benefit entitlement, only a 34-week employment spell was required to re-qualify for benefits. This made re-qualifying for UI benefits easier and could increase the incentives to search for temporary employment via the entitlement effect, but we would argue that the effect is likely to be minor. In any case, also this change also affected all the unemployed workers, so we can control for the effect using a suitable difference-in-differences approach.

3 Data

We analyse the effects of the benefit reform using individual-level administrative data from the Ministry of Labour, the Insurance Supervisory Authority and the Finnish Center for Pensions.

The Ministry of Labour (MOL) register covers all job-seekers registered at the unemployment agencies. Since registering at an unemployment agency is a requirement for receiving UI benefits, practically all the unemployed workers are in the database. The data contain information on the initial and final dates of each unemployment spell. Also the reasons for the entry and exit are also recorded in the data. Therefore, those who enter unemployment because they were fired for "economic or production-related reasons" and who, therefore, may be eligible for increased unemployment benefits can be identified from the data. We can also analyze exits from unemployment into employment, to out of the labour force and to various labour market programs separately. Background data on individuals are also available from the register, including sex, age, education, occupation, region and previous unemployment history. The major weaknesses of the dataset are that it contains no information on pre-unemployment wages, on the unemployment benefits or even on the eligibility for the earnings-related benefits.

We complement the information in the MOL database with information on the unemployment benefits from the registers of the Insurance Supervisory Authority (ISA). Each quarter the unemployment funds submit detailed reports to the ISA on the benefits paid during the quarter. These reports include daily benefit amounts and days compensated itemised by the individual and the four-week period. The benefits are further disaggregated so that increased benefits are reported separately. Data also include the date when the individual joined a UI fund, which is needed for determining eligibility for increased benefits. Another useful variable in the database is the remaining days of the benefit eligibility at the end of each quarter, a number that is extremely hard to calculate in a reliable way based on unemployment spell data alone.

The final piece of information required for determining the eligibility for higher benefits comes from the registers of the Finnish Centre for Pensions. The UI funds check the twenty-year work history requirement from the pension registers. We use the same source and add to each worker the information on the number of months worked after turning 18. This information has been recorded in the pension records since 1962, when the current earnings-based pension system was created.

We drew a 50 per cent sample from persons entering unemployment between January 1st 2002 and December 31st 2004. Since the reform increased the UI benefits for those with at least 20 years of work experience, the average eligible unemployed are well over forty years old. To allow flexible choices of comparison groups we included in the data all unemployed persons over 37 at the start of their UI spell. We follow these individuals until the end of 2005. By then all those unemployed whose unemployment spell started in 2002 or 2003 will have exhausted their 500-day benefit eligibility. Many unemployment spells that started in 2004 are still ongoing at the end of 2005. These spells are treated as censored observations at that point. We also treat as censored observations all unemployment spells that end for any other reason than job finding, and all unemployment spells that are ongoing after 600 days.[3]

By drawing the sample from different registers, using the same personal identity numbers, we can match the data from different registers. While linking the individuals is relatively easy, linking the unemployment spell dates from different sources turned out to be burdensome. The details of the matching procedures used are given in the Appendix.

In the final dataset used in the analysis the observation unit is an unemployment spell. Time is measured in days of benefit recipiency (5 days per week). We focus on the unemployed who lost a permanent job and keep only those who had no previous unemployment spells during the previous three years, counting backwards from the date of entry into unemployment. Only the unemployed who receive some earnings-related benefits are included, since the ISA data contain no information on those who are not receiving these benefits.

[3] The reason for exit is missing or unknown in 5 per cent of the spells. Examining the labour market status at the end of the year reveals that most of these are employed. We therefore code all these as having found a job.

All time-varying background information is observed at the starting date of each spell.

3.1 Descriptive statistics

In *Table 1* we report some descriptive statistics of the sample that is used in the analysis. We report these statistics separately before and after the reform and separately for the treatment group that became eligible for increased benefits and for the comparison group whose benefits remained unchanged.

There are some clear differences between the treatment group and the comparison group. Since the key criterion for eligibility was the length of the previous work history, it is natural that the treatment group has more work experience. The treatment group is also older, on average, and has higher earnings than the comparison group. On the other hand, the average level of education is lower in the treatment group, reflecting the fact that those with more education have, on average, less work experience at a given age and the fact that younger generations tend to have better education. Also, the occupational distribution is somewhat different. A large fraction of the treatment group had been employed in manufacturing occupations, while healthcare occupations are over-represented in the comparison group.

Since we will be evaluating the effects of increased UI benefits by comparing the changes in the re-employment rates between the eligible and ineligible unemployed, we will have to assume that the composition of the unemployed does not change in a different way among the eligible and the ineligible unemployed. In the second last column of *Table 1*, we present p values testing this assumption. We run simple linear regression models explaining each background characteristic with the eligibility and post-reform dummies and their interaction, and test whether the coefficient of the interaction term is zero. For the categorical variables the test is based on a multinomial logit-model, where we explain the probability that a categorical variable takes a certain value and test with a likelihood ratio-test that the effect of the interaction of the eligibility and post-reform dummies on these probabilities is zero.

For most background characteristics there are no signs of different changes in composition between the eligible and ineligible groups. Only the change in occupational distribution seems to be significantly different. Examining the changes in actual distributions reported in Columns 1-4 reveals that even these differences in changes appear to be small. The increase in the UI benefits is naturally significantly larger in the eligible group, because their benefits were affected by the reform. The descriptive statistics on the reason for exit suggest that the reform might have had an effect on the re-employment rates. The fraction re-employed decreases in the eligible treatment group while it increases in the ineligible comparison group.

According to *Table 1,* only 69 per cent of the group that should have been eligible for the increased benefits actually received higher benefits according to the ISA data.[4] In the last column we report the same descriptive statistics for those who actually received increased benefits. According to *Table 1*, there seem to be no large differences between the actual recipients and all who should have been eligible, which indicates that there are no clear signs of selectivity within the treatment group.

Table 1 Descriptive statistics

	Ineligible		Eligible		Diff-in-diff	Actual
	Before	After	Before	After	p-value	recipients
Age	44.2	44.2	48.8	48.7	0.606	48.7
Male	0.44	0.45	0.56	0.56	0.476	0.50
Education					0.350	
Primary	0.16	0.11	0.32	0.25		0.24
Secondary 1	0.12	0.12	0.12	0.12		0.13
Secondary 2	0.39	0.40	0.37	0.38		0.38
Lower tertiary	0.18	0.19	0.11	0.14		0.16
Higher tertiary	0.15	0.18	0.08	0.11		0.10
Occupation					0.006	
Other	0.03	0.03	0.03	0.02		0.01
Specialist	0.15	0.17	0.10	0.12		0.11
Healthcare	0.10	0.10	0.04	0.02		0.01
Administration	0.17	0.18	0.14	0.15		0.18
Commercial	0.13	0.12	0.11	0.14		0.16
Transport	0.05	0.04	0.03	0.04		0.04
Construction	0.06	0.06	0.06	0.06		0.04
Industrial	0.23	0.20	0.41	0.40		0.39
Services	0.09	0.09	0.07	0.06		0.06

[4] Also about 8 per cent of the ineligible group also received increased benefits according to the ISA database. This reflects classification errors in eligibility. We will discuss its implications after presenting the basic results.

	Ineligible		Eligible		Diff-in-diff	Actual
	Before	After	Before	After	p-value	recipients
Previous wage, €/mo	1,866	1,963	2,026	2,172	0.145	2174
Disability	0.05	0.06	0.05	0.05	0.487	0.04
Work experience	18.6	18.4	26.9	26.6	0.997	26.3
UI-membership duration	10.4	10.3	17.7	17.7	0.160	16.0
Daily benefits, €	51.42	54.14	52.97	61.47	0.000	64.02
Receives increased benefits	0	0.08	0	0.69	0.000	1
Reason for entry					0.836	
Unknown	0.05	0.05	0.14	0.11		0.03
Displaced	0.22	0.24	0.86	0.89		0.80
Other	0.29	0.28	0.00	0.00		0.03
Temporary contract ended	0.44	0.43	0.00	0.00		0.14
Reason for exit					0.002	
Re-employed	0.47	0.49	0.44	0.40		0.38
Unknown	0.05	0.07	0.04	0.05		0.03
Exit from LF	0.42	0.37	0.46	0.46		0.50
End of follow-up	0.06	0.08	0.06	0.09		0.09
N	5,483	10,327	1,422	2,652		2,700

Notes: The entries in the table are mean values calculated separately according to the eligibility status and separately for the unemployment spells starting before and after January 1st 2003. The p-values reported in fifth column are based on the test of the hypothesis that sample composition changes in a similar way in the eligible and in the ineligible groups. The rightmost column report mean values for those actually receiving increased benefits.

4 Methods

According to the search theory, an increase in the unemployment benefits increases the reservation wages and decreases the incentives to search for work affecting the exit rates from unemployment during the entire benefit period. The reduction in job finding rates is strongest at the beginning of the unemployment spell because at that point the change in the value of the remaining future benefits is the highest. By the time the unemployed have received increased UI benefits for 150 days, the benefits are reduced to the normal level, and the search intensity should increase to the pre-reform level. At this point the search intensity may be even higher than before the reform because of the "entitlement effect" i.e. the increase in the value of finding a job that could re-qualify for higher benefits.

To evaluate the effect of the benefit increase we have to model the effects on the exit hazards in a way that allows different effects at different points during the unemployment spell. We do this by specifying a proportional-hazard model with a flexible baseline hazard and time-varying effects of the benefit increase. Although the determinants of the hazard rate are also interesting, we are primarily interested in the changes in the baseline hazard that are due to the reform. The empirical hazard function

$$\theta(t) = \lambda(t)\exp\{x\beta\}, \tag{1}$$

where $\lambda(t)$ is a time-varying baseline hazard function, x a vector of time-invariant individual characteristics measured at the start of the unemployment spell, and t indexes weeks on benefits starting from the date of entry into unemployment. We assume that the baseline hazard function is constant within each four-week interval but place no restrictions on the change in the baseline hazard between these intervals. To reduce the noise in the estimates at long durations we aggregate the intervals where the hazard is assumed to be constant to 12 weeks after 48 weeks in unemployment.

$$\lambda(t) = \exp\left\{\sum_{i=1}^{12}\lambda_i I(4(i-1) < t \le 4i) + \sum_{i=13}^{18}\lambda_i I(12(i-9) < t \le 12(i-8))\right\}. \tag{2}$$

To identify the effects of the benefit increase on the hazard profile we then compare the changes in the interval-specific hazard rates in the treatment and the comparison group using a difference-in-differences approach

$$\lambda_i = \beta_{i0} + \beta_{i1}TREAT + \beta_{i2}REFORM + \beta_{i3}TREAT \times REFORM\ , \tag{3}$$

where *TREAT* is an indicator of the eligibility for increased benefits and *REFORM* an indicator that the unemployment spell started after January 1st 2003. We are primarily interested in the coefficients of the interaction terms (β_{i3}) that measure the differences in the changes of the hazard estimates after the reform between the treatment and the comparison groups.[5]

We interpret the differences in the change of the hazard between the treatment and the comparison groups as the effect of the reform at a certain interval of elapsed unemployment duration. Strictly speaking, this interpretation is only valid at $t = 0$. If there is unobserved heterogeneity, and if the increase in the benefits in the treatment group lowers the re-employment hazards, the remaining unemployed in the treatment group will be more employable than the remaining unemployed in the comparison group at dates $t > 0$. This could cause an upward bias in the effect estimates.

We also estimate a more restrictive model where the benefit increase has a constant proportional effect at all elapsed durations. This model is nested within the more general model, allowing a simple test of constant effects. Even if the constant effect model is rejected, the results are interesting, as they provide a point of comparison with previous studies that have imposed this restriction.

5 Results

We first compare the changes in duration of unemployment in the treatment and the comparison groups after the reform. In *Table 2* we report the median durations for all UI benefit spells without any restrictions on the reason for exit. It turns out that the median durations are very similar in the treatment and comparison groups before the reform. After the reform on January 1st 2003 the median duration declined in the comparison group but increased in the treatment group. A simple difference-in-differences estimate indicates that the reform increased the median duration by 19.5 days. The difference-in-differences estimate is highly significant with a bootstrapped standard error of 7.1 days.

[5] Note that we do not assume that the duration dependence is similar in the treatment and the comparison groups but we estimate all β_{i1} terms freely. However, in the empirical analysis we assume that duration dependence is constant over time i.e. that $\beta_{i2} = \beta_2$ for all $i = 1,\ldots, 18$. This restriction seems plausible, given that the time horizon is only three years.

Table 2 Median duration of unemployment, days

	Before Jan 1st 2003	After Jan 1st 2003	Difference	Difference-indifferences
Comparison	127	118	-9	
	(3.1)	(1.9)	(3.6)	
Treatment	126.5	137	10.5	19.5
	(4.5)	(4.1)	(6.1)	(7.1)

Note: Bootstrapped standard errors with 2,000 replications in parenthesis

The comparison of median durations in *Table 2* reveals that the median duration increased in the treatment group while it decreased in the comparison group. However, it does not tell whether the effect is due to changes in the job-finding rates or changes in the exit rates to other destinations. In addition, it provides no evidence on whether the effect is due to a decrease in the re-employment rates at the beginning of the unemployment spell or to a change in the employment prospects for the long-term unemployed.

Figure 4 displays the unconditional hazard rates of exiting into employment in each four-week interval separately for the treatment and the comparison groups. Exits out of the labour force and into labour market programs, as well as ongoing spells after 600 days, and ongoing spells at the end of 2005 are treated as censored observations. The figure indicates that re-employment hazards decrease rapidly at the beginning of unemployment spells. This could be due to genuine duration dependence or heterogeneity in the re-employment rates. Since we are using single spell data, differentiating between duration dependence and heterogeneity is empirically difficult and we make no serious effort in differentiating between these. As the unemployed approach the expiry date of unemployment benefits (500 workdays), the job-finding rate starts to increase in both groups, though the effect seems to be stronger among those eligible for increased benefits. The shape of the hazard rate is consistent with previous research (e.g. Meyer, 1990) and has been interpreted as evidence of the effect of the limited duration of UI benefits. Note, however, that this conclusion is not based on a comparison with some other group whose benefits do not expire after 500 days. In fact, Kyyrä & Wilke (2007) use Finnish data to show that extending the duration of benefits beyond 500 days for workers over 55 dramatically reduced the job-finding rates throughout the unemployment spell, and not just close to the benefit expiry date.

Comparing the hazard rate before and after the reform reveals that the re-employment hazards decrease in the treatment group but only at the beginning of the unemployment spell. After about 200 days on benefits, the hazard rates are higher than before the reform but the estimates are rather noisy. In the ineligible comparison group the increase in re-employment hazards is roughly constant across different points of elapsed duration.

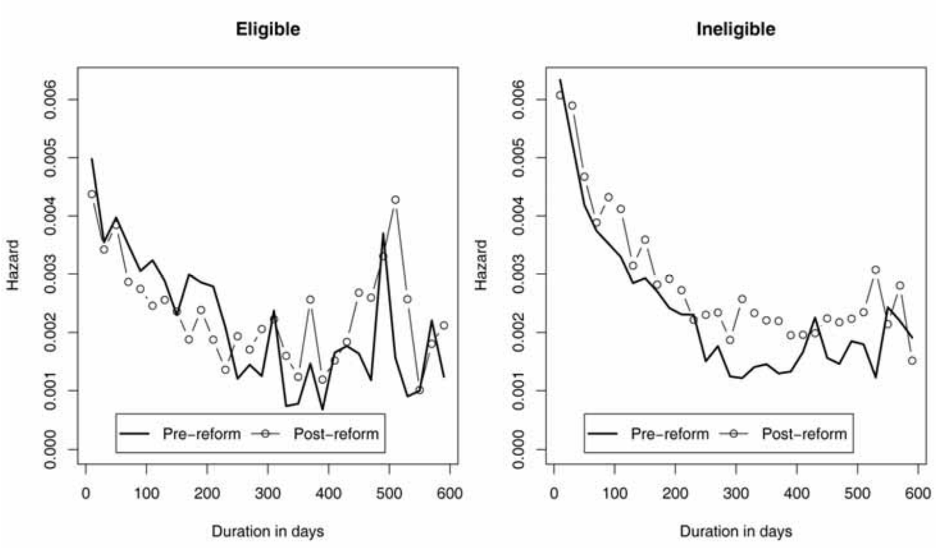

Figure 4 Re-employment rates before and after UI-reform

To account for the differences in the composition of the treatment and the comparison groups we estimate a proportional-hazard model as described in the previous section. In addition to the treatment status and the reform effects we add to the model indicators of age, sex, disability, education (5 categories), broad occupation (9 categories), region (15 categories), previous work experience, duration of UI-fund membership, pre-unemployment wage, reason for entry into unemployment (5 categories) and indicators for the month and year when the unemployment spell started. Duration dependence is accounted for by 18 duration-specific dummies and the difference in duration dependence between the treatment and the control groups with a set of another 18 dummies. These parameter estimates can be found in the Appendix. Here we concentrate on the reform effects.

Figure 5 plots these estimates and they are specified so that each point in the figure refers to the reform effect at a specific interval of elapsed benefit duration. The estimates in the figure are presented as relative hazards with 1 indicating no effect. We use four-week intervals up to 48 weeks in unemployment then aggregate the data into twelve-week intervals. The hollow circles report the unconstrained estimates where the effect of the reform on the re-employment hazard may vary freely across the elapsed duration of unemployment. These estimates indicate that the increase in benefits caused a substantial decline in the re-employment hazard but that the effect only occurs during the first 250 days of unemployment. After that, the effect of the reform is positive, but the estimates have wide confidence bands. The dashed line presents estimates from a model where the effect of the reform on the re-employment rates is restricted to be equal across all elapsed durations. The point estimate indicates a 16 per cent decline in the hazard and the estimate is highly significant (z = 3.4, p = 0.001). According to a likelihood ratio test the restrictions implied by the constant-effect model are not rejected when tested against the unrestricted alternative (p = 0.14).

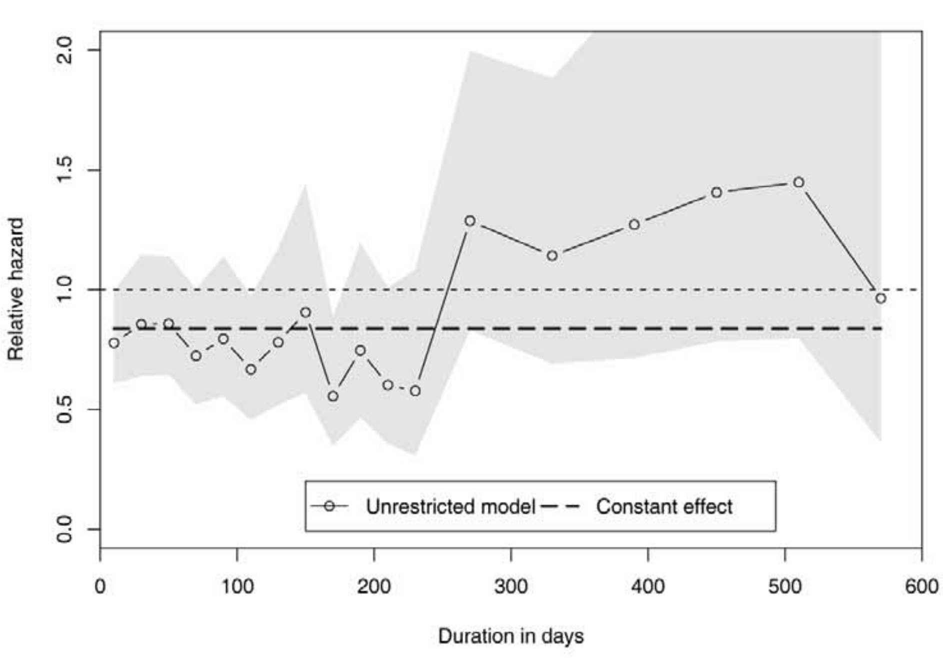

Figure 5 Effect of the reform on re-employment hazard

Note: Gray area indicates 95 percent confidence intervals.

6 Extensions

One of the concerns in the previous research has been that the unemployed may anticipate the changes in the benefit system. The search theory assumes that the unemployed are aware of the expiry date of UI benefits and increase their search efforts before the benefits actually expire. In a similar way, the unemployed might already react to the change in the benefit system already before the reform date if the change can be anticipated. It would be awkward to assume that the unemployed are forward-looking with respect to their future benefit sequence but completely myopic with respect to a change in the benefit system. For example, Carling *et al.* (2001) note that a benefit reform had already affected the hazard rates of exiting unemployment already several months before the policy change.

In the Finnish UI reform the benefit increase applied only to those entering unemployment after January 1st 2003. The benefits remained unchanged for those already unemployed on the reform date. By comparing the change in the hazard profile before and after the reform, we therefore compare the unemployed whose benefit sequence changes for the entire unemployment spell and avoid the confusion between future changes in the system and future changes in the benefits under a given benefit system.

However, there might still be anticipatory effects if the change in the benefit system had an effect on the incidence of unemployment. We are primarily concerned about the potential effects of changing a lump-sum severance pay to higher benefits. Even though the expected value of increased benefits in the whole eligible population is roughly equal to severance pay, it is possible that those who expect to find jobs quickly would try to affect the timing of dismissals so that they could still be eligible for severance pay. Such strategic timing of dismissals could affect our results.

By calculating descriptive statistics in *Table 1* separately for the eligible and the ineligible group we could already demonstrate that the reform did not have much effect on the composition of the new entrants. *Figure 6* attempts to provide further evidence on the question by reporting the monthly numbers of new entrants into unemployment around the reform date. The figure displays clear seasonal variation in the entry rates but no pattern that would suggest systematically higher entry rates just before the reform in the group eligible for severance pay. As a robustness check, we also dropped those entering unemployment in November or December, from the data with no notable changes in the results.

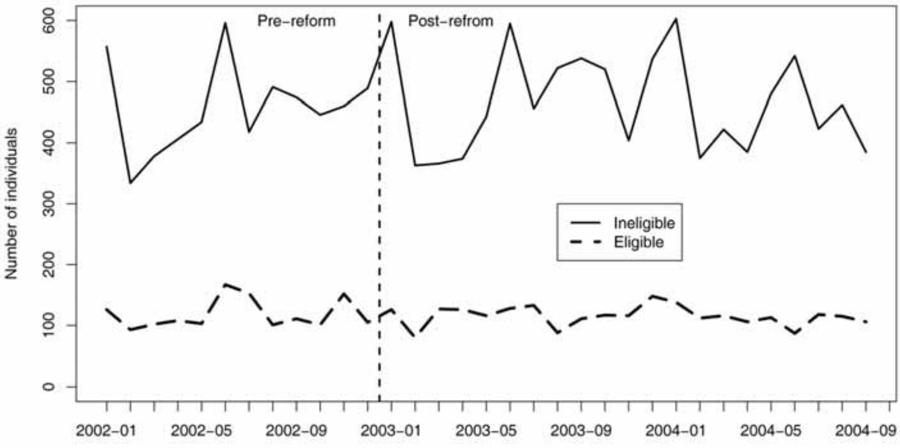

Figure 6 Number of new unemployment spells by month in the treatment and the comparison groups

A potentially more relevant question has to do with classification error in the eligibility for benefits. The eligibility for increased unemployment benefits depends on the work history, the UI-fund membership and previous unemployment experiences. In an ideal case we could observe all these factors and evaluate the effect of benefit increase by comparing the changes in exit hazards between the eligible and ineligible groups.[6] Unfortunately, none of these criteria can be precisely determined from the data.

The problem in identifying eligibility based on twenty-year work history criteria is caused by the fact that according to the Unemployment Security Act the twenty-year work history requirement may also contain spells of maternity leave, sick leave, military service, and disability that are not recorded in the pension register[7]. There is also some uncertainty about the length of UI-fund membership. The length of UI-fund membership is recorded in the data only for the current UI fund. Therefore, individuals who switched UI funds during

[6] This would also allow us to use these limits in a regression discontinuity framework to evaluate the effects of the benefit increase.

[7] When claiming increased benefits, the unemployed who are close to fulfilling the twenty-year work history criteria must provide the UI fund with documentation about periods of maternity leave, military service etc.

the previous five years may be falsely classified as not fulfilling the membership criteria. Third, we have no information on the recipiency of severance pay in the past. The unemployed who received severance pay during the five years prior to entry into unemployment may, therefore, be falsely classified into an eligible group though they are not entitled to increased benefits. We mitigated this problem by excluding from the data all those unemployed individuals who had a previous unemployment episode during the three years before entry into unemployment. In practice, this also limits the analysis to those displaced from a relatively stable career, which is also the main target group of the reform. Finally, some of the unemployed may not be aware that they might have a right to increased benefits. UI funds provide advice for the applicants, but since many applications are received by mail without a personal contact, not all claimants receive this information.[8]

However, since both actual benefits and the information used to determine benefit eligibility are included in the data, the accuracy of predictions can be assessed by comparing the rule-based classification with the actual recipiency of the increased benefits in the post-reform data. *Table 3* presents a cross-tabulation of the data according to whether an unemployed should be eligible for increased benefits and whether she or he actually received increased benefits. Based on information on the work history, the length of UI-fund membership, and the reason for entering unemployment we can correctly predict 87 per cent of the actual benefit recipiency, which still leaves a substantial classification error.

Table 3 Eligibility for increased benefits vs. actual recipiency after the reform

		Received increased UI benefits	
		No	Yes
Eligible for increased UI benefits	No	9,458	869
	Yes	821	1,831

[8] This explanation is based on personal communication with UI-fund managers in February 2005.

6.1 Correcting the effects of misclassification in the treatment status

By defining the treatment status according to the eligibility criteria that are available in our data we have estimated the effect of "the intention to treat". In an experimental setting this would be equivalent to including drop-outs in the treatment group and including cross-overs, who are assigned to the control group but still participate in the program, in the comparison group. If the classification errors are random, the effect of the program assignment is a downward-biased estimate of program participation. This bias can be corrected by using the treatment assignment as an instrument for the treatment status.

In our case the recipiency of increased benefits is only observed in the post-reform data. Therefore, the standard IV approach cannot be used. However, we can use post-treatment data to estimate a first-stage equation that explains the recipiency of increased unemployment benefits with variables that are included in the eligibility criteria. We can then use these estimates to predict the treatment status in both the pre-reform and the post-reform data and use the predicted treatment status as an explanatory variable in our duration model. The method resembles the two-sample IV estimate (Angrist & Krueger, 1992; Björklund & Jäntti, 1997) where two different samples are used to construct the moments required for a consistent IV estimate.

Simply replacing the treatment indicator in a nonlinear duration model with the predicted treatment status would not only lead to biased standard errors but can also lead to inconsistent estimates, as shown, for example, in Cameron and Trivedi (2005, p. 198). A simple solution suggested by Angrist (2001) is to ignore the fact that the model is nonlinear and estimate a constant effect linear probability model instead. This does not recover the structural parameters of the duration model but, as long as the covariates are discrete, it provides an appropriate description of the underlying causal relationship.

A second issue that arises in this setting is that, because the treatment is binary, a nonlinear first-stage such as a logit-model might be appropriate. However, in this case the second-stage estimates are inconsistent, unless the model for the first-stage is actually correct. On the other hand, conventional two-stage least squares estimates using a linear probability model in both the first-stage and the second-stage are consistent whether or not the first-stage is linear. (See Angrist, 2001). This argument generalises to an estimator where a linear prediction from the first-stage equation is plugged into the second-stage linear probability model. The only remaining issue has to do with biased standard errors. We deal with this by bootstrapping.

To implement the estimator we first estimate a linear probability model explaining benefit recipiency after the reform using all the covariates included in the duration model and adding the interaction between the length of the previous employment history and the length on union membership. We use these coefficients to calculate predicted probabilities of benefit recipiency in both the pre-reform and the post-reform data. We then formulate a discrete-time version of the duration model by splitting the unemployment spells into four-week intervals and explain job finding rates in each interval with the linear probability model using the original covariates and the predicted treatment status from the first-stage.[9] The second-stage is identified through omission of the interaction terms from the second stage.

Figure 7 reports the results from the discrete time hazard model. To ensure that the linear probability model and discrete hazard formulation produce similar results to our proportional hazard model results presented in *Figure 5* we first present simple OLS results where job finding in each interval is explained by the rule-based assignment of benefit eligibility as in *Figure 5*. The line labelled as “two-sample IV” presents the results where eligibility rules are used as an instrument for benefit recipiency.

The effects reported in *Figure 7* are measured as percentage point changes in the job-finding rates instead of proportional effects on the hazard rate. Qualitatively, the results from the discrete-time duration model are still reasonably similar to those based on the proportional-hazard model presented in *Figure 5*. The estimates show that job-finding rates decrease by about two percentage points in each four-week period during the first 250 days in unemployment. The difference between the OLS and the two-sample IV-estimates is small, indicating that misclassification concerning the treatment status has only a small effect on the estimates. Both estimates are close to zero after 250 days, but the standard errors are large at long durations.

[9] The discrete-time duration model with complementary log-log link function is a discrete-time analogue of the continuous-time proportional hazard model if the hazard and the covariates are constant within each interval.

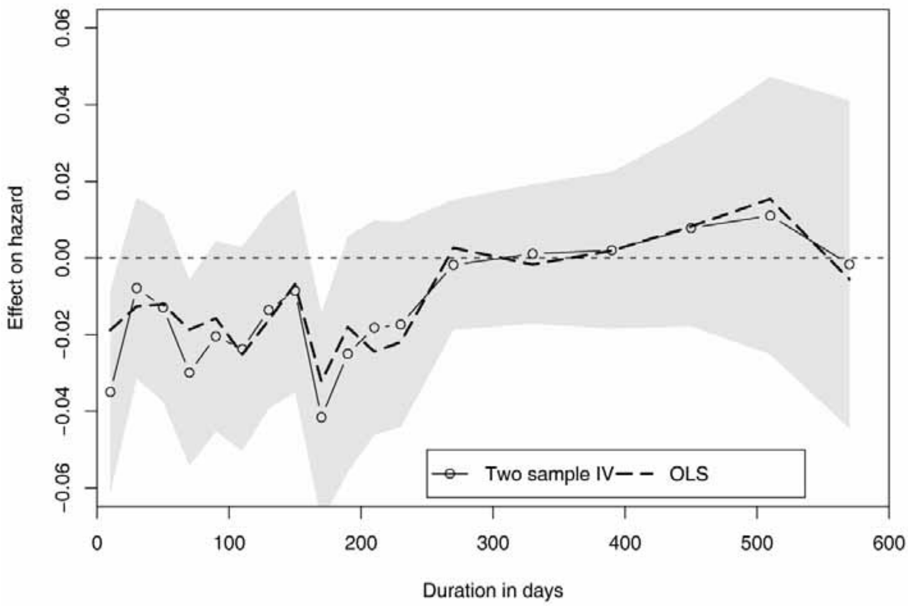

Figure 7 Effect of the reform based on discrete-time hazard function estimates

Note: The grey area indicates 95 percent confidence intervals of two-sample IV-estimates generated by bootstrapping with 1,000 replications.

7 Conclusion

Concerns about the effect of job destruction on the most vulnerable groups increase the demand for social insurance provided by the unemployment benefits. While higher benefits may cushion the effect of job loss in groups that have the greatest difficulty in finding new employment, such benefit increases also have a side effect of decreasing the incentives to search for new jobs. In this paper we have evaluated the effects of improving unemployment benefits for a group of older workers. According to our results the effects of benefit increase on re-employment rates may be substantial. Based on our estimates one can calculate that a fifteen per cent increase in benefits for the first 150 days of unemployment increases the expected time until re-employment by 31 days or about 11 per cent. This implies that the elasticity of time until re-employment with respect to the benefit level would be 0.75. However, since many unemployed individuals exit from the data for other reasons before finding work, this number cannot be directly interpreted as an effect on the duration of unemployment.

We also find that an increase in UI benefits decreases the re-employment hazard but the hazard rate returns to the pre-reform level once the period of increased benefits expires. We find no evidence that the unemployed anticipate the change in the benefit level by increasing their search effort before the benefits are decreased. In contrast, it seems that a decline in benefits increases re-employment rates only about one or two months after the benefits have been reduced. Taken at face value, this would imply that the unemployed are myopic and start searching more actively only after benefits have been reduced.

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Data Appendix

The analysis data is constructed by linking data from three administrative registers. The primary data source is the database of the Ministry of Labour, which contains all unemployment spells and a rich set of covariates. However, this database contains no information on the unemployment benefits. Earnings-related benefits are administered by the Insurance Supervisory Authority. Further, information on work experience is obtained from the Pension Security Institute. The supplementary data are linked to the unemployment spell data by using individual identifiers and payment dates.

Sampling

A representative inflow sample was drawn from the unemployment spell database. The sample contains all unemployment spells that began between 1.1.2002 and 31.12.2004 for individuals born on an odd date before 1967. These individuals were followed until 31.12.2005. Unemployment spells with no match in the unemployment benefit data are excluded from the data. We believe that a majority of the excluded individuals are ineligible for earnings-related benefits and receive only the basic allowance or labour market support.

Unemployment spell data

The observation unit in the unemployment spell data is a spell. The data consist of 104,941 individuals between 37 and 66 years of age. They experience 474,144 unemployment spells from the beginning of 2002 to the end of 2004. To obtain a more consistent picture of the length of unemployment, consecutive spells with short interruptions are merged. Merging spells with less than two-week break reduces the number of spells to 241,190.

Unemployment benefit data

The unemployment benefit data include all unemployment insurance payments from 2002 to 2005. The observation unit is a payment report provided by the unemployment insurance fund. The reports contain the dates of compensation periods and the amount of daily allowance. An important variable in the data is a counter of used benefits days that is recorded at the end of each quarter. Earnings-related benefit is paid up to 500 working days except for those over 55 who may receive it until retirement. If the employment condition is fulfilled between spells, the eligibility is renewed.

The counter information is only updated quarterly. No essential information, therefore, is lost in merging subsequent payment reports on an individual level within a quarter. Before merging, inconsistent rows are removed (duplicated rows and payment periods within another period). In cases where two rows contain different values, high values of the daily allowance are preferred to low values. The same criterion is used for other earnings information such as previous salary.

Linking datasets

The unemployment spell data provide unambiguous information but the benefit data may contain conflicting information, due to discrepancies between reports. Our objective is to get a reliable estimate for the number of benefit days used at the beginning of each unemployment spell. When linking the datasets, we check that the matched report periods do not intersect with a subsequent unemployment spell. In case of multiple reports matching an unemployment spell, the report closest to the beginning of the spell is preferred. If benefit information is missing, we use subsequent reports to complete the data. Lastly, the work experience data are linked. Because the information is available only for the end of 2001 and 2002, the time out of unemployment between the date of information and the beginning of unemployment is computed. This sum should provide a fairly accurate estimate of the length of work experience at the time of unemployment.

Analysis sample

After linking the datasets, we have information on the amount of paid benefits and the number of benefit days for most of the unemployment spells. For some individuals with repeated short spells, no unique benefit report match was found for every spell. To complete missing information, the information is derived by the use of subsequent spells that begin within six months. After this operation, rows with incomplete information are removed, which leaves 192,973 rows in the dataset.

Many individuals experience multiple unemployment spells. Typically, this is either because of short employment spells between unemployment or participation in active labour market programmes. These individuals are not likely to be eligible for the increased benefit because the rules exclude those who have received severance pay earlier. Therefore, only the first unemployment spell is included, which restricts the number of rows to 97,618, which now equals the number of individuals. In addition, to take into account possible severance pay prior to 2003, all those individuals who have been unemployed during past three years before the beginning of the observed spell are removed. After this, the sample includes 34,082 individuals, of whom 39 per cent fulfil the eligibility criteria for increased benefits. A large proportion of the sample consists of elderly people who are eligible for earnings-related allowance without a time limit. We focus only on individuals between 37 and 54 years of age, which gives us a sample of 19,884 individuals, of whom 20 per cent are eligible for increased benefits.

Appendix 2. Coefficient estimates from an unrestricted model

		Coefficient	Std. Error
Intercept		-4.678	0.094
Age	(ref 37-40)		
	41-46	-0.136	0.027
	47-54	-0.348	0.035
Sex	Female	-0.057	0.024
Education	(ref: primary)		
	secondary 1	-0.050	0.041
	secondary 2	0.117	0.032
	tertiary 1	0.110	0.041
	tertiary 2	0.232	0.045
Occupation	(ref: agriculture)		
	specialist	-0.151	0.065
	health care	0.323	0.066
	administration	-0.293	0.064
	commercial	-0.187	0.065
	transport	0.050	0.074
	construction	0.584	0.067
	industrial	-0.191	0.062
	Service	-0.022	0.067
Log wage	(ref: <1.37)		
	(1.37,1.63]	0.049	0.033
	(1.63,1.91]	0.080	0.034
	(1.91,2.36]	0.103	0.035
	>2.36	0.150	0.037
Region	(ref: uusimaa)		
	Vars.Suomi	0.187	0.037
	Satakunta	-0.014	0.040
	Häme	0.067	0.045
	Pirkanmaa	0.161	0.062
	Kaak.Suomi	0.209	0.054
	E.Savo	-0.012	0.048
	P.Savo	0.131	0.052

		Coefficient	Std. Error
	P.Karjala	-0.080	0.059
	K.Suomi	0.127	0.080
	E.Pohjanmaa	0.058	0.041
	Pohjanmaa	0.179	0.052
	P.Pohjanmaa	0.127	0.051
	Kainuu	0.075	0.041
	Lappi	0.263	0.058
Disability		-0.493	0.050
Experience	(ref <12)		
	[12,17)	0.143	0.034
	[17,20)	0.208	0.038
	[20,23)	0.224	0.042
	[23,27)	0.235	0.046
	>=27	0.165	0.050
UI-fund membership	(ref <3)		
	[3,5)	-0.174	0.038
	[5,7)	-0.138	0.040
	[7,15)	-0.197	0.030
	>=15	-0.226	0.032
Reason for entry	(ref. Unknown)		
	displaced	-0.571	0.040
	Other	-0.861	0.044
	temporary	-0.164	0.041
Month of entry	(ref: January)		
	February	-0.117	0.046
	March	-0.095	0.047
	April	-0.100	0.048
	May	-0.153	0.048
	June	-0.121	0.044
	July	-0.044	0.044
	August	-0.137	0.045
	September	-0.189	0.046
	October	-0.165	0.046
	November	-0.135	0.047
	December	0.012	0.047

		Coefficient	Std. Error
Year	(ref 2002)		
	2003	0.068	0.026
	2004	0.142	0.025
Duration dependence	(weeks, ref 1-4)		
	5-8	-0.060	0.039
	9-12	-0.267	0.043
	13-16	-0.412	0.047
	17-20	-0.347	0.048
	21-24	-0.386	0.050
	25-28	-0.605	0.057
	29-32	-0.496	0.057
	33-36	-0.678	0.064
	37-40	-0.685	0.067
	41-44	-0.741	0.071
	45-48	-0.866	0.078
	49-60	-1.002	0.056
	61-72	-0.953	0.061
	73-84	-1.033	0.072
	85-96	-0.915	0.078
	97-108	-0.843	0.088
	109-120	-0.763	0.106
Treatment group		0.127	0.106
Treatment * duration dependence			
	5-8	-0.265	0.157
	9-12	0.060	0.157
	13-16	0.089	0.168
	17-20	-0.110	0.180
	21-24	-0.006	0.183
	25-28	0.106	0.199
	29-32	-0.231	0.223
	33-36	0.221	0.211
	37-40	0.182	0.222
	41-44	0.218	0.233
	45-48	0.058	0.272

		Coefficient	Std. Error
	49-60	-0.268	0.223
	61-72	-0.278	0.242
	73-84	-0.253	0.271
	85-96	-0.192	0.273
	97-108	0.117	0.276
	109-120	-0.346	0.382
Treatment effects			
	1-4	-0.251	0.125
	5-8	-0.156	0.148
	9-12	-0.153	0.146
	13-16	-0.323	0.167
	17-20	-0.230	0.183
	21-24	-0.404	0.193
	25-28	-0.248	0.208
	29-32	-0.099	0.237
	33-36	-0.588	0.238
	37-40	-0.292	0.240
	41-44	-0.508	0.265
	45-48	-0.548	0.321
	49-60	0.253	0.224
	61-72	0.132	0.256
	73-84	0.241	0.294
	85-96	0.341	0.298
	97-108	0.370	0.305
	109-120	-0.036	0.501
	Log likelihood -68,163		
	n (spells) 19,884		
	n (intervals) = 169,632		

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