Workfare for the old and long-term unemployed*

Helge Bennmarker†, Oskar Nordström Skans‡, Ulrika Vikman§

March 8, 2013

Abstract

We estimate the effects of conditioning benefits on program participation among older long-term unemployed workers. We exploit a Swedish reform which reduced UI duration from 90 to 60 weeks for a group of older unemployed workers in a setting where workers who exhausted their benefits received unchanged transfers if they agreed to participate in a work practice program. Our results show that job finding increased as a result of the shorter duration of passive benefits. The time profile of the job-finding effects suggests that the effects are due to deterrence effects during the program-entry phase. We find no evidence of wage reductions, suggesting that the increased job-finding rate was driven by increased search intensity rather than lower reservation wages.

Keywords: Activation, program evaluation, UI, duration
JEL:J64, J65, J68, J26

*We are grateful for useful comments by Olof Åslund, Peter Fredriksson, Kostas Tatsiramos, Johan Vikström, Peter Skogman Thoursie and seminar participants at SOFI, UCFS, the UCLS Brown bag seminar and participants at EALE 2012, Bonn, and the 4th Joint IZA/IFAU Conference on Labor Market Policy Evaluation.

†Institute for Evaluation of Labour Market and Education Policy (IFAU), Email: Helge.Bennmarker@ifau.uu.se

‡IFAU, Department of Economics, Uppsala University, UCLS and IZA. Email: Oskar.Nordstrom_Skans@ifau.uu.se

§Corresponding author IFAU, Box 513, 75120 Uppsala, Sweden. Phone: +46 (0)18 471 7085 Fax: +46 (0)18 471 7071 Email: Ulrika.Vikman@ifau.uu.se, Department of Economics, Uppsala University, UCLS and UCFS.

1 Introduction

The burden of unemployment is in many countries to a disproportional extent placed on workers in both ends of the age distribution. The anatomy of unemployment spells does however differ dramatically between the young and the old; where the young on average have high inflow rates into unemployment but short unemployment durations, the opposite pattern holds for older workers. Few old workers become unemployed, but those who do tend to stay unemployed for very long periods of time. In terms of policy, the two groups are also treated remarkably different; while youth unemployment often is tackled by specially provided mandatory activation or workfare programs, old unemployed instead tend to be granted extended periods of passive benefit receipt. One possible rationale for this difference is that the old and jobless are considered unemployable, regardless of which policy measures are used, and that passive financial insurance therefore is to be preferred over workfare policies. This paper contributes to the stock of policy relevant knowledge by studying how the job finding rate is affected by a policy shift from very long passive benefits towards (earlier) workfare policies for older long-term unemployed workers.

There is an existing, quite extensive, literature on how the design of unemployment insurance systems affects unemployment duration. Numerous studies have found that the probability of leaving unemployment increases when jobseekers approach the time of benefit exhaustion (e.g. Addison and Portugal 2004; Card et al. 2007; Carling et al. 1996; Ham and Rea 1987; Katz and Meyer 1990; Meyer 1990; Røed and Zhang 2003) and that the unemployment duration increases when maximum benefit duration is extended (e.g. Card and Levine 2000; Hunt 1995; Katz and Meyer 1990; Lalive et al. 2006; Meyer 1990; van Ours and Vodopivec 2006).

The interpretation of benefit exhaustions is, however, likely to vary depending on the institutional context (see e.g. Røed and Westlie (2012) or Carling et al. 1996). One reason is that other public transfers may be available when UI-benefits expire. Our data are drawn from Sweden in the 1990s, where expired passive UI-benefits implied that the unemployed got access to other, equally generous, benefits if they agreed to participate in an active labor market program which was universally offered. Thus, benefit exhaustion did not change the financial incentives to search for jobs, but it made program participation an additional requirement for continued benefits. Importantly, programs were available as an option already before benefits expired, but became mandatory (i.e. a condition for continued benefits) at the time of UI-exhaustion.

There are a number of previous studies on the effects of mandatory programs, in particular for youths. Examples include Carling and Larsson (2005), Forslund and Skans (2006) and de Georgi (2005) who use age discontinuities to study the impact of mandatory programs on youths in Sweden and the UK, all finding evidence of positive short run pre-program (threat) effects. Dolton and O'Neill (1996, 2002) evaluate the restart program for long-term unemployed in the UK and find positive long run effects, at least for males. Hägglund (2011) analyses a set of randomized experiments in Sweden where job seekers were called to participate in mandatory job-search programs with a few weeks' notice and found evidence of increased job finding before the programs started. These types of pre-program effects are also found by Black et al. (2003), Geerdsen (2006) and Geerdsen and Holm (2007). In a particularly relevant study, Graversen and van Ours (2008) document the effects of mandatory programs in Denmark using data from an experiment where unemployed workers were randomly assigned into a mandatory program. They find evidence of positive average effects (mainly stemming from increased monitoring), which appear to be particularly large for older unemployed.

Whereas activation has been extensively used (and hence studied) for youths, the policies aimed at older unemployed instead tend to focus on extended durations of passive benefits (see e.g. Tatsiramos 2010). The theoretical underpinnings for this policy route appear weak, however. Michelacci and Ruffo (2011) argue that the distortions from UI benefits are larger, and that the insurance motive is smaller, among older workers, suggesting that UI-benefits in fact should be less generous for the older unemployed. In general, the probability for unemployed workers to find employment also tends to decrease when approaching retirement age (Hairault et al., 2011). Interestingly, this seems to be a margin that can be affected by policy. Lalive (2008) analyze very clear age and regional discontinuities in an Austrian setting and the results show that extended benefits for older workers leads to longer unemployment spells, but also that the effect becomes enormously large if the UI-periods become sufficiently long to bridge into the retirement system. On the other hand, cutting older workers off benefits altogether may be a politically infeasible policy option, suggesting a potential role for workfare policies. A rationale for making programs mandatory is as a means to verify that the worker is available for work and at the same time reduce the value of unemployment without inducing poverty among those who cannot find employment (see e.g. Andersen and Svarer 2007).

This paper studies a Swedish policy reform in 1998 which raised an age threshold in the UI system and thereby effectively reduced the maximum duration of passive UI-benefits from 90 to 60 weeks for workers aged 55 or 56. Program slots (mostly work practice) were offered to all unemployed who lost their UI-benefits and benefit levels during program participation was exactly at par with the UI system. A new spell of passive benefits was granted after six months of program participation. As expected, the inflow into active labor market programs increased massively around 60 weeks after registration for the covered group.

We analyze the changes in job finding probabilities due to the reform using a control group consisting of a mix of slightly older and slightly younger workers. Our results show that the reform increased transitions to jobs among the covered workers. The effects appear around the time of inflow into the programs, suggesting that the effects are due to the confiscation of leisure, rather than arising from human capital accumulation due to program participation. We further show that the reform caused average monthly earnings among the workers who returned to employment to increase. Assuming that the reform did not improve the average unobserved earnings potential among those finding jobs, this result suggests that the main effects are driven by increased search intensity rather than by reduced reservation wages, and that match quality did not deteriorate.

Overall, the results suggest that earlier, mandatory, program participation may induce older workers to find jobs earlier, that the extensive use of long periods of passive benefits for older workers in many European countries may contribute to long-term unemployment among this group, and that the older workers' unemployment periods can be reduced without inducing poverty among those who are unable to find jobs.

The paper is structured as follows: In Section 2 we discuss the overall institutions and describe the relevant labor market conditions. In Section 3 we describe the data and the empirical methods. Section 4 presents the empirical results. Section 5 provides a discussion.

2 Institutions and labor market conditions

2.1 UI and ALMPs in the 1990s

Our empirical analysis uses a reform in the Swedish UI system in 1998. Here we therefore briefly describe the relevant Swedish institutions in the mid to late 1990s[1]. During this period, an unemployed worker was entitled to UI benefits if he or she

i had been a member (voluntary) of a UI-fund for 12 months,

ii had been employed for six months before becoming unemployed, and

iii was registered (and complied with search requirements) at the Public Employment Service (PES).

The PES is responsible for monitoring job search, administrating sanctions, providing job search assistance, and administrating active labor market programs. Program participants receive an alternative transfer, "Activity Support" (AS), with identical benefit levels as in the UI system. Workers who participated in programs (and therefore received AS) did not consume UI-days during the duration of the program.

A particular feature (see e.g. Sianesi 2008) was that UI-recipients who participated in an active labor market program lasting for at least six months re-qualified for a new period of passive UI-benefits. Thus, it was possible to remain on benefits for an indefinite time period by "cycling" between UI and AS.

New UI spells started with a five day uncompensated waiting period after which, for most workers, UI lasted for approximately 60 weeks (300 work days). Workers aged 55 or older after 60 weeks and who became unemployed before January 1, 1998 were however given 30 additional weeks (150 work days) of passive UI benefits. The age threshold was raised from 55 to 57 for unemployment spells beginning after January 1, 1998.

When passive UI benefits expired after 60 or 90 weeks, unemployed workers were offered to participate in a labor market program, and hence receive unchanged transfers through the AS-system. The contents of the programs could vary, but the primary focus was on work practice schemes.

The benefit levels were determined by the previous wage with a 75 percent replacement rate until 31th of August 1997 and thereafter with an 80 percent

[1] For a more detailed description, see Carling et al. (1996).

replacement rate. There was a floor compensation of 230 SEK (29 USD)[2] and a cap of 564 SEK (71 USD) until end of December 1997 when they were changed to 240 SEK (30 USD) and 580 SEK (73 USD) respectively. All of these numbers refer to both the UI and AS systems. A major reform in February 2001 changed many features of the system, including an abolishment of the age threshold for UI duration[3].

In Appendix A we illustrate the evolution of transfers and disposable income over the course of unemployment spells for older long-term unemployed workers. The data show that the net level of insurance (accounting for all taxes and transfers) was high, disposable income never fell below 74 percent of the pre-unemployment disposable income. The data are also fully in line with the institutional description above; UI payments fell in the second full year after job loss, but other transfers (AS) fully compensated.

2.2 Age and unemployment in Sweden

The period under study, with the reform taking effect at the beginning of 1998, was one of recovery from a very deep recession, as shown in Figure 1. The figure also shows that, as in most countries, unemployment is higher among the youths than among prime aged and older workers in Sweden. Unemployment among 55 to 64 year olds on the other hand has remained around the unemployment rates of prime-aged workers.

Although the unemployment rates among older workers do not stand out as particularly troubling in the Swedish context, this is entirely driven by low rates of unemployment inflow. The time it takes to find a job increases monotonically over the age distribution (see Figure 2).

[2] According to the exchange rate of about 8 SEK/USD in January 1998.

[3] Since 2001 workers are transferred into a never-ending (but low-intensive) program when passive UI-benefits expire. During 2001 to 2006 case workers at PES were given discretion over whom to offer extended benefits (an additional 60 weeks) and whom to place in mandatory programs after 60 weeks. Currently the 60 week limit for mandatory programs is binding for all except parents with children (90 weeks), and for youths (90 days). Participants receive AS when UI-benefits have expired also in the current system, but benefit levels can be lower.

Figure 1: Unemployment rates by age in Sweden, according to OECD
Note: We truncate the data series in 2004 due to a major break in the data series in 2005.

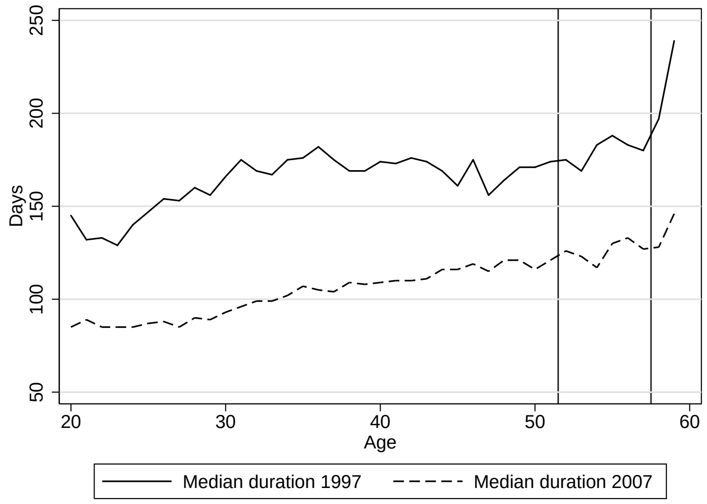

Figure 2: Median unemployment duration (in days) to jobs, by age at inflow (PES data) in 1997 and 2007
Note: Based on own calculations. The two vertical lines indicate the age span used in this paper.

3 Data, description and methods

3.1 Data

The data used in this paper are all drawn from population wide registers in the IFAU-database. We combine data originating from Statistics Sweden, the PES, and the UI-funds. More specifically, we use the PES registers on unemployment spells (Händel), the UI-payment register (AKSTAT), an income and population register (Louise) and tax records of employer-employee transfers (Rams).

The PES register is an event database which records start and end dates of registered unemployment spells (registration is a prerequisite for UI and AS benefits) as well as reasons for exits, together with start and end dates of various forms of "search categories" such as ("open") unemployed job search, on-the-job search, and participation in active labor market programs (ALMPs).

We construct our study population from the inflow into PES-registered unemployment among workers who are eligible for UI-benefits. In order to exclude workers with ongoing UI-spells we restrict the sample to workers who had a maximum of 10 days of unemployment during the year before registration. For the same reason, we also exclude workers who participated in some form of subsidized employment during the preceding year. We further require that they receive payments from the UI-fund. In order to increase precision we also exclude very short unemployment spells (lasting 5 weeks or less) under the assumption that the outflow this early is unaffected by changes in UI-rules which cover those that remain unemployed for at least 60 weeks.

Our empirical models (see below) analyses hazards from unemployment to jobs. We use a definition of unemployment which as closely as possible mimics official definitions of unemployment, letting PES-registration proxy for active job search. We therefore consider workers as unemployed if they are registered at the PES as long as they are not being registered as searching on the job. On-the-job search include those in temporary, part-time and partly subsidised employment. Participants in active ALMPs such as work practice or training are considered as unemployed. Unemployed who exit to a job according to the PES registers, or who spend more than 10 days registered as searching on the job, are considered as having found a job. Other exits are censored in our main specification. We also censor all spells after 120 weeks, as well as spells reaching into February 2001 when the entire UI system was reformed.

As an alternative source of information regarding outcomes we use a register of the total annual earnings received by each worker from each employer, as well as the first and last remunerated month during that year and employer-employee combination. We use these data for two purposes. The first is to analyse the impact of earnings conditional of job-finding. The second is as an alternative measure of transitions into employment. In order to quantify transitions, we code workers as employed in the first month where earnings (from all jobs) exceed one third of a minimum wage (corresponding to the 10 day limit).[4] These data are however somewhat imprecise in terms of the exact measurement of when employment takes place since the month indicators refer to the time of payment (not the time of work) and since missing month indicators are indistinguishable from spells lasting from January to December in our raw data. We therefore focus our main analysis on the more precise PES data.

Our key explanatory variables are age and calendar year. The combination of these defines the duration of passive UI-benefits. Consistent with the UI-rules, we define *year* as the year of inflow, and *age* in years 60 weeks after the start of the UI-spell.

As controls we include a very rich set of variables capturing seasonality, socioeconomic status, and previous labor market experience. The variables include gender, immigration status, three indicators for level of education, and a marital status indicator from population registers. From the PES register we code calendar month of entry, days unemployed previous four years, indicators for previous (two to four years back)[5] unemployment, a disability indicator[6], and ten indicators for type of municipality[7]. Finally, we include ten indicators for previous occupations[8] as well as the wage underlying the UI-benefit level, all drawn from the UI-records.

[4]The minimum wage is calculated as the 10th percentile in the overall wage data using data from the Structure of Earnings Statistics.

[5]One year back all individuals are working due to the restrictions we put on our sample.

[6]Recorded disabilities are heavily affected by unemployment duration (see (Johansson and Skedinger, 2008)) and are, in our data, recorded at the end of each spell. We therefore only use information from disabilities in previous spells.

[7]Grouped according to classification by Swedish Association of Local Authorities and Regions (SKL): Metropolitan centre, metropolitan suburban, larger centre, larger suburban municipality, commuting municipality, smaller tourism oriented municipality, smaller goods producing municipality, rural municipality, municipality in densely populated area, municipality in mainly unpopulated area.

[8]Based on UI-fund information, indicators for each of the 8 largest funds and the rest aggregated into one residual white collar category and one residual blue collar category.

3.2 Description

3.2.1 The unemployed

We start by describing our sample of workers. In Table 1 we show descriptive statistics for pre-determined characteristics. We provide four descriptive columns, before (1996-97) and after (1998-99) the change in age threshold for the "control group" (i.e. workers aged 53-54 and those aged 57-58) and for the treatment group (workers aged 55-56). In the final column we show the differences in differences between these. We use the composite control group (i.e. a mix of slightly older and younger workers) since using any of the two parts (older or younger) separately gives us a very unbalanced experiment in terms of background characteristics due to age and cohort differences.

Starting with the overall characteristics of the sample, we see that the average age is close to 56 as expected. Slightly less than half are female and nearly two thirds are married. A large fraction of the sample is low educated, which is natural due to the cohorts involved (born in 1938-46). Almost 90 percent are Swedish born and half of the immigrants are from one of the neighboring Nordic countries. The average worker had approximately 100 days of registered unemployment during the four years preceding the analyzed spells, and the proportion of individuals being unemployed at all during two, three or four years before the current spell is between 15 and 22 percent. Between one and two percent of workers had a recorded disability from a previous unemployment spell. Pre-unemployment wages are slightly higher than the wage corresponding to the UI cap[9], and about half (somewhat increasing over time) received the maximum (cap) UI benefit level.

The final column shows DD estimates of the effect of the reform on the characteristics of the unemployed. Most of the covariates are extremely well balanced. In particular this holds for all variables capturing previous labor market performance. We do however find a very marginal significant effect on the fraction of east European immigrants, as well as significant effects on the fraction married and on the average age. Although the latter effect is quite small, and do not seem to be important enough to affect any of the other covariates (except for the marriage rate), we will analyze its importance for the results in the robustness section.

[9] Ranging from about 15,000 SEK to 16,000 SEK during the period, depending on the replacement rate and the level of the cap.

Table 1: Descriptive statistics for treatment and controls before and after the reform.

	1996-1997		1998-1999		
	Control mean	Treatment mean	Control mean	Treatment mean	DD
Age after 60 weeks	55.75	55.98	55.90	55.99	-0.136***
Female	0.444	0.447	0.436	0.432	-0.007
Married	0.656	0.640	0.630	0.640	0.027**
Schooling					
Compulsory or less	0.412	0.416	0.388	0.388	-0.003
Upper secondary	0.433	0.419	0.450	0.446	0.010
Tertiary	0.155	0.166	0.162	0.166	-0.006
Immigration status					
Born in Sweden	0.891	0.896	0.892	0.908	0.011
Other Nordic country	0.054	0.049	0.051	0.043	-0.003
Western Europe	0.020	0.018	0.015	0.016	0.002
Eastern Europe	0.019	0.022	0.022	0.019	-0.006*
Born outside Europe	0.015	0.015	0.019	0.014	-0.004
Labor market history					
Days in unemployment during previous 4 years	112	106	118	115	3.4
Unemployed:					
2 years earlier	0.169	0.159	0.156	0.159	0.013
3 years earlier	0.223	0.208	0.204	0.200	0.011
4 years earlier	0.209	0.198	0.225	0.221	0.007
Disability (previous spell)	0.016	0.018	0.018	0.016	-0.003
Previous wage (1000 SEK)	16.03	16.10	17.18	17.25	-0.003
Receiving max UI	0.422	0.424	0.570	0.567	-0.006
Observations	8,717	4,132	8,107	4,19	25,148

Note: Description of the used data set. DD estimates are from regressions with treatment and year interval dummies, and the estimates show the impact of the interaction of these. Standard errors are available upon request. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Finally, Table 2 shows reasons for exiting ongoing unemployment spells within the sample. About two thirds of the subjects exit to jobs, half of which exit to regular full time employment and the other half to jobs including temporary jobs and part time jobs. Some caution is however warranted in interpreting the relative magnitudes since the distinction between different forms of job exits in the data somewhat arbitrarily depends on how individual case workers choose to record the events. In our empirical analysis we treat all job exits as positive outcomes and perform a number of robustness checks regarding how to treat other exits.

Table 2: Reasons for exits from unemployment

	1996-1997		1998-1999	
	Control	Treatment	Control	Treatment
Regular employment	34.17	32.79	33.64	34.32
Other jobs	31.90	35.31	29.91	31.12
Studies	2.88	1.48	3.21	2.60
Lost contact	2.93	3.24	3.00	3.27
Other	9.44	8.42	10.92	10.29
Censored due to time	18.68	18.76	19.33	18.40
Observations	8,717	4,132	8,107	4,19

Note: Spells are censored due to time after 120 weeks and in February 2001. "Other jobs" include exits to part time jobs, temporary jobs, partly subsidized self-employment and partly subsidized regular employment. "Other exits" include transfers to other authority.

3.2.2 Benefit duration and transitions to programs

Figure 3 describes the flows into programs over the duration of an unemployment spell from the registration at PES. The first panel is for the period before the reform which shortened UI-duration for workers aged 55 to 56 and the second panel is for the period after. The vertical lines at 60 and 90 weeks indicate the two thresholds for when benefits on uninterrupted UI will expire depending on age and period. There are three lines in each panel, one for the younger part of control group which always had 60 weeks of UI (aged 53-54), one for the treatment group where the duration changed (aged 55-56) and one for older part of the control group which remained at 90 weeks throughout (aged 57-58).

Firstly, we see that there is a very clear peak at benefit expiration for each of the three groups. The fact that the peaks are bell shaped rather than per-

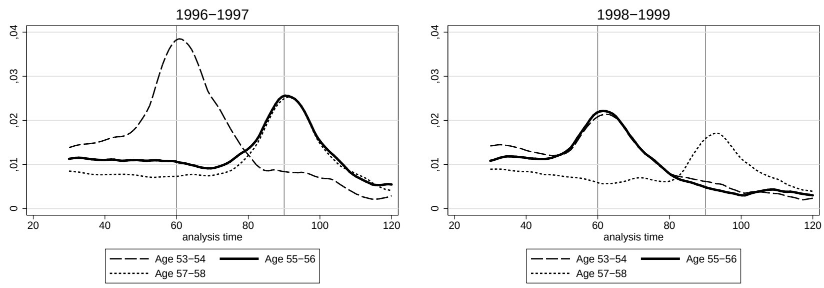

Figure 3: Smoothed hazard estimates to program by age

fectly aligned with the 60/90 week thresholds is expected since some workers may choose to enter ALMPs earlier, whereas other workers may postpone the expiration of benefits by taking shorter breaks in the UI-sequence. In particular, we do not treat short low-intensive job search programs whose participants also are financed through AS as ALMPs in this context. This "fuzziness" implies that our analysis should be interpreted in a reduced-form sense; we estimate the effects of changing expectations regarding the maximum duration of passive benefit weeks for workers who remain in unemployment.

Secondly, the figure shows that the reform moved the treatment group from exactly mimicking the older part of the control group to mimicking the younger part of the control group equally precise. We interpret this as showing that the reform affected program placement around benefit exhaustion as intended.

Since the focus of this paper is on the effects of changes in the duration of passive UI-benefits, it is useful to briefly describe the kinds of programs that were offered jointly with the AS-benefits when UI-benefits expired. In Table 3 we show the composition of programs for those who enter active labor market programs in the 30-week interval surrounding the expiration of passive benefits. The table shows that although there was a shift over time towards training (driven by computer training courses) the main thrust of the programs were offered as various forms of work practice programs.

Table 3: Programs around end of UI

	1996-1997		1998-1999	
	Control	Treatment	Control	Treatment
Work practice	71.99	71.18	54.24	50.57
Labor market training	21.41	21.30	36.30	34.85
Other programs	6.60	7.52	9.46	14.58
Observations	1,303	399	719	528

Note: Data cover transitions into programs during the weeks 45-75 after the start of the unemployment spell for those with 60 UI weeks and during weeks 75-105 for those with 90 UI weeks. "Work practice" includes relief work, work experience schemes, workplace introduction and resource jobs within the public sector. "Labor market training" also includes computer training courses (Datortek); "Other programs" include start-up grants and the employability rehabilitation program.

3.3 Empirical specifications

The purpose of the empirical exercise is to pin down how the duration until workfare affects the job finding rate among older unemployed. In order to do so we rely on the change of age threshold in the UI-system. As shown in the descriptive section above, our data include workers aged 53-58, where the two intermediate cohorts (55-56) are affected by a reduction in passive UI-duration. The dual control group consists of both older and younger workers to ensure that potential exogenous changes in age related job-finding rates over time should be less of a concern. We use the unemployment inflow in 1996-97 (pre-reform) and 1998-99 (post-reform).

As our main model we use a standard stratified Cox-proportional hazards (CPH) model where other exits than jobs are censored, but where program participation is treated as continued unemployment.

Our covariate of interest is the duration of passive benefits. We model this as a dummy variable taking the value one for workers with 60 weeks of passive benefit duration (D^{60}) and zero for those with 90 weeks duration. We further let the impact of the dummy vary over the duration of the spell in pre-specified bins (τ) which is a function of analysis time t[10].

[10] This is necessary since the effect is likely to change sign over the duration of the spell making the average effect uninformative (see below).

The stratified CPH model conditions out the baseline hazard for each stratum. We stratify the model on *age*. Allowing for age-specific baseline hazards implies that each age group is allowed to have a specific relationship between job-finding hazards and unemployment duration. This is necessary for the identification of the effect of interest since UI-duration vary with age within the control group (the younger part has 60 weeks of passive benefits, and the older has 90 weeks). The stratification accounts for these differences and thereby forces the identification of the effect of interest (D^{60}) to come from changes in UI-duration over time within an age group (i.e. from the reform).[11]

Our model further includes a set of individual characteristics capturing socioeconomic status and the pre-unemployment labor market history (X) as well as year-of-in-flow dummies ($Year$). Formally the log hazard is given by:

$$\log h_i(t) = \log \lambda_0^{age}(t) + \sum_{\tau=1} \gamma_\tau D_i^{60} + Year_i \beta^y + X_i \beta^X \qquad (1)$$

This model is varied in various ways in the empirical section in order to assess the robustness of the results. In particular we vary sample restrictions, the functional form of the age and time controls, the censoring, and the underlying data source. In addition, we also analyze the impact of the reform on the monthly earnings of those who find employment using a traditional (linear) difference-in-difference model.

It is important to note that the two policy regimes we compare only differ in the timing dimension. The short passive UI-duration is compared to longer UI-duration within the same overall framework. Our design is therefore well-suited to analyze effects on transitions relatively early on in the spells, but less well-suited to analyze the impact on transitions later on in the spells. This point is shown in a very stylized example presented in Table 4 below. The table denotes the period-specific baseline hazard by h and assumes that expiring UI (i.e. forced transitions into programs) has a deterrence effect (γ) in the period when UI expires and a post-program effect (ϕ) in the period thereafter.[12]

[11] In effect, specifying D^{60} as the interaction between time and being aged 55 to 56 therefore gives numerically identical results.

[12] The period-sequence is chosen to approximate the results presented below.

Table 4: A stylized model of shorter passive UI and hazards to jobs

Hazard to jobs, by period and duration of passive benefits				
	Period 1	Period 2	Period 3	Period 4
Passive benefit duration:				
Two-period UI	*h1*	*h2*+γ	*h3*+ϕ	Censored
Three-period UI	*h1*	*h2*	*h3*+γ	Censored
Effects of shorter UI	0	γ	$\phi - \gamma$	–

Note: The four periods approximates the structure of the empirical analysis. *h1*, *h2* and *h3* denote period-specific baseline hazards, γ denotes a potential deterrence effect and ϕ denotes a post-program effect.

An important insight from this table is that the set-up allows us to identify the deterrence effects fairly well (period 2), but also that these deterrence effects will generate a wave-like pattern with negative effects in period 3 even in the absence of post-program effects (i.e. if $\phi = 0$). Thus, it is difficult to identify post-program effects if the deterrence effects are substantial.[13]

4 Results

4.1 Main results

Given that our identification of the causal effects relies on a differences-in-differences type of identification, we first plot the evolution of the job finding hazard for the different age groups in Figure 3. The left panel shows, for each two-year age group, the log cumulative hazard up to week 75 by year of inflow. The picture shows that the trends in hazards are converging, rather than being parallel, before the reform. This time pattern, which is likely to be driven by a relationship between age and the impact of improving business cycle conditions, motivates our use of a composite control group mixing slightly older and slightly younger workers. The right hand side panel of the same figure shows a comparison between the full control group and the treatment group. Our interpretation

[13] This adds to the dynamic selection bias which may plague estimated effects periods following impacts earlier on in the spells. The combination of the problems makes us reluctant to identify post program effects by imposing structure in the form of a constant deterrence effect over the two periods.

of this panel is that the trends for the treatment and the full control group respectively were parallel before the reform and then diverge. This suggests that the identification strategy is valid, and indicate that the reform did increase the hazard to jobs in the treated group relative to the control group.

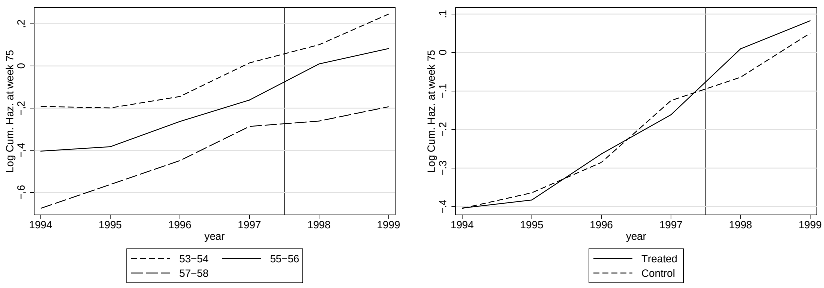

Figure 4: Log cumulative hazard at week 75, among those still unemployed at week 6.

We proceed by presenting results from the Cox proportional hazards model outlined in equation (1), where all exits except those to jobs are censored. The estimates are presented in Table 5, with and without our set of X-variables which capture seasonality, marital status, education, disabilities, region, occupation, immigrant status, unemployment history and previous wage.

The main message from the table is that the reduction of time until workfare from 90 to 60 weeks caused an increase in the hazard to jobs by about 10 percent in the period 30 to 75 weeks from registration.[14] Letting the effect vary within even finer interval gives a very similar pattern although the precision is substantially reduced as expected. The table also shows that the impact of the very rich set of covariates on the estimate of interest is relatively minor.

Our results do not suggest any significant effects during the first 30 weeks. On the other hand, we see a fairly dramatic negative effect after week 75. As explained in Section 3.3 above this estimated effect does however contain a mixture of (the negative of) the deterrence effect and potential post-program effects. Furthermore, the estimate may also be negatively biased through dynamic selection effects if the positive effect in earlier periods made the better equipped (in an unobserved sense) of the unemployed to leave for jobs. For these reasons, we do not wish to give the estimate effects for weeks after 75 any economic interpretation, but continue to report them for completeness.

[14]The baseline job finding rate is about 1 percent per week in this interval.

Table 5: Impact of 60 rather than 90 weeks of passive benefits on job finding

Job finding interval:	Without covariates		With Covariates	
-15 weeks	0.056		0.040	
	(0.055)	-30 w	(0.055)	-30 w
16-30 weeks	0.068	0.062	0.051	0.045
	(0.058)	(0.042)	(0.058)	(0.042)
31-45 weeks	0.146**		0.116	
	(0.073)	31-75 w	(0.073)	31-75 w
46-60 weeks	0.091	0.133***	0.063	0.109**
	(0.087)	(0.051)	(0.087)	(0.051)
61-75 weeks	0.165*		0.153	
	(0.096)		(0.097)	
76-90 weeks	-0.210*	76-120 w	-0.223*	76-120 w
	(0.116)	-0.280***	(0.117)	-0.291***
91-105 weeks	-0.488***	(0.082)	-0.504***	(0.083)
	(0.145)		(0.146)	
106-120 weeks	-0.113		-0.110	
	(0.175)		(0.175)	
N (subjects)	25,146	25,146	25,146	25,146
Controls:				
Age at 60 weeks	Yes	Yes	Yes	Yes
Time of inflow	Yes	Yes	Yes	Yes
X-covariates	No	No	Yes	Yes

Note: Non-job exits are censored. Stratified on Age and Year of inflow is controlled for by dummies. X-covariates are dummies for registration month (season), female, married, education, disabled, 10 municipality groups, occupation (eight largest UI funds, blue and white collar workers), immigrant background, if unemployed 2-4 years before registration date, days unemployed previous 4 years and wage before unemployment. Standard errors clustered on individual are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A classical profile of effects for labor market programs might include three phases: pre-treatment deterrence, a non-positive locking-in-effect while participating, followed by a potentially positive effect of increased human capital after the program. Although these three effects in theory should follow in exactly this sequence, it is easy to imagine that they may sometimes overlap. Deterrence might for example act also in the beginning of the program (for those who do not appreciate the program) and human capital effects might take force before the program has come to an end. However, thinking of treatment effects in these terms, it seems straightforward to interpret our estimates as deterrence effects since they appear during the phase when program entry increased dramatically (see Figure 3 above) due to the shortening of passive UI benefits. Still, it might be illuminating to explicitly allow for different kinds of program effects in the analyses. In Appendix B we therefore perform a formal analysis where controls for program participation are included to try to separate deterrence effects from the other mechanisms through which programs might work. This analysis does not change the conclusions. The analysis also shows very large negative effects of being in a program, and small, insignificant effects of lagged participation.

4.2 Effects on different exit margins

The results presented above are all based on specifications where exits other than jobs are censored. However, it is evident that workers with a high value of leisure may choose to leave the labor force due to the workfare requirements. This process is interesting in itself, e.g. Card et al. (2007) find that the increased spike at benefit exhaustion gets much smaller when looking at time to first job instead of time spent in the unemployment system. But exits from the registers may also lead us to overestimate the job finding effects if the composition in the group at risk is altered. To verify the importance of these concerns, Table 6 shows the effects on different outcomes and under varying censoring schemes. The first column replicates the job finding estimate of Table 5. In the second column we display the estimated effect on all exits, including transitions out of the labor force. This effect is larger than the effect on job finding, suggesting that workfare also generates exits from the labor force. On the one hand, this is an expected effect. Conditioning benefits on program participation should reduce the value of continued unemployment and therefore lead to higher exits out of the labor force. On the other hand, it should be noted that previous studies have found that half of the unemployed who leave the PES registers due to lost contact actually have found jobs (see e.g. Forslund et al. 2004). Thus, some of these exits may in fact be misclassified job exits.

In the final column we choose a rather extreme specification where we treat all who leave the PES for other reasons than jobs as continuously unemployed.

Here we still find positive effects on the job finding hazard of around eight percent.[15] In this model we only censor due to time making the results robust to deviations from the competing risk assumption required for censoring other exits in a CPH-model.

Table 6: Impact of 60 rather than 90 weeks of passive benefits on different outcomes

		Outcomes	
	(1)	(2)	(3)
	Job	All exits	Job, w/o cens
-30 weeks	0.045	0.024	0.093**
	(0.042)	(0.039)	(0.042)
31-75 weeks	0.109**	0.164***	0.084*
	(0.051)	(0.045)	(0.050)
76-120 weeks	-0.291***	-0.188***	-0.384***
	(0.083)	(0.068)	(0.067)
N (subjects)	25,146	25,146	25,146
Censored	Other exits and time	Time	Time
Controls:			
Age at 60 weeks	Yes	Yes	Yes
Time of inflow	Yes	Yes	Yes
X-covariates	Yes	Yes	Yes

Note: Stratified on Age, Year of inflow is controlled for by dummies. X-covariates are dummies for registration month (season), female, married, education, disabled, 10 municipality groups, occupation (eight largest UI funds, blue and white collar workers), immigrant background, if unemployed 2-4 years before registration date, days unemployed previous 4 years and wage before unemployment. Standard errors clustered on individual are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.3 Robustness and heterogeneity

In this section we discuss a number of model variations which should be interpreted as robustness checks of the baseline specification. These variations all

[15]Workers are treated as finding jobs if they reenter the PES and leave for jobs later (using the later date).

imply that we relax some of the initial assumptions, and precision will therefore in general be slightly poorer than in the baseline specification. For this reason, and in order to conserve space, we focus on the model which only allows the effect to vary in three broad segments of analysis time. The first estimate in Table 7 is, again, the baseline specification. Specification (2) only uses data from 1997 and 1998, finding similar point estimates but with much poorer precision. Specification (3) narrows down the age span in order to address the potential concern left from the descriptive analysis where it was found that the age composition in the inflow had tilted somewhat during the sample period. Reassuringly, the estimates using a narrower age span is similar to, albeit somewhat larger than, those based on the full sample.

Next, specification (4) relies on the alternative, earnings based, indicator of job finding. Here we measure the time until monthly earnings exceed one third of the minimum wage (the 10th percentile in the overall wage distribution) for the first time since becoming unemployed. Again, we find results that are in line with, but somewhat larger than, the baseline specification. A problem with these data is that monthly earnings are distributed evenly across the months of employment within each employment spell (worker-firm pair) and calendar year.[16] We are therefore more confident in the timing derived from the PES data. Nevertheless, we find it reassuring that estimated effects of interest on time until the workers pass a monthly earnings threshold seem to agree with the pattern derived from the PES data.

In specification (5), we include controls for calendar time (months) which are allowed to change over the unemployment spell. The baseline specification relies on inflow year and seasonal dummies for month of inflow, but these may fail to properly account for seasonal changes in the labor market for long-term unemployed. However, results presented in specification (5) show that the results are insensitive to modelling dummies for calendar time in months.

Finally, we provide three additional excercises related to concerns raised by the fact that Table 1 suggested that the age composition of the inflow changed with the reform. In In specification (6), we reweight the analysis to preserve the same age composition accross years. The results are completely unchanged. In addition, we provide a "'placebo"' analysis where we instead only use age variation within the control group. The model defines 54 and 56 year olds as placebo-treated, and keep those aged 53 or 57 as the control group. The ensuing

[16]This may explain the somewhat puzzling positive estimates in the 75+ week interval.

Table 7: Robustness checks of the effects on job finding

	Specification							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	Closer in time	Closer in age	Using earnings data	Time dummies	With weights	Placebo 1 Only Control	Placebo 2 Pre-period
-30 weeks	0.045	0.083	0.080*	0.018	0.011	0.044	-0.020	-0.074*
	(0.042)	(0.059)	(0.047)	(0.030)	(0.042)	(0.042)	(0.046)	(0.044)
31-75 weeks	0.109**	0.084	0.144***	0.158**	0.109**	0.108**	-0.035	0.017
	(0.051)	(0.070)	(0.055)	(0.062)	(0.051)	(0.051)	(0.056)	(0.052)
76-120 weeks	-0.291***	-0.041	-0.254***	0.259***	-0.244***	-0.293***	-0.420***	0.276***
	(0.083)	(0.101)	(0.085)	(0.085)	(0.083)	(0.083)	(0.087)	(0.066)
N (subjects)	25,146	12,866	16,619	25,146	25,146	25,146	16,824	26,245
Censored:	Other exits and time	Other exits and time	Other exits and time	Time	Other exits and time	Other exits and time	Other exits and time	Other exits and time
Controls								
Age at 60 weeks	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time of inflow	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
X-covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Non-job exits are censored. Stratified on Age and Year of inflow is controlled for by dummies. X-covariates are dummies for registration month (season), female, married, education, disabled, 10 municipality groups, occupation (eight largest UI funds, blue and white collar workers), immigrant background, if unemployed 2-4 years before registration date, days unemployed previous 4 years and wage before unemployment. Standard errors clustered on individual are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

estimate of interest is small (-0.035) and insignificant as expected. This support the indetifying assumption that non-linear interactions between time and age are negligable. Similarly, the final column present a placebo analysis which roles back the analysis two years so that we only use data from a pre-reform period. Again, the estimate of interest is small (0.017) and insignificant.

We have also conducted a number of unreported specification tests. We have excluded subjects that are on the verge of passing the age threshold when UI expires. Borderline workers can, in principle, manipulate their spell to get a longer UI duration by making sure that they do not use all their UI days before they pass the age threshold. Excluding workers who have less than 0.2 years to their 55th or 57th birthday does however not change the results at all. We have included spells shorter than 5 weeks and the estimates change very little although precision is reduced (main estimate is significant at 10 percent level). We have estimated a model relying on age dummies instead of the age stratification while forcing the variable of interest to be identified from the reform by defining it as an interaction of age and time, again finding very similar results.

We have also re-estimated the model relying only on workers who enter week 30 without having found a job. By doing so, we make sure that we estimate the full model in the interval of the estimate of interest. We also exclude those that enter a program before week 30 and who therefore are unaffected by workfare around week 60 (remember, early program participants also re-qualify for new benefit periods). This creates a sample where the time to benefit exhaustion is binding. A drawback is that the analyzed sample may be affected by selection effects. The assumption for a causal interpretation is that the included covariates handle any potential selection effects induced by the sample restriction, an assumption which should be fulfilled if there is no effect on transitions during the first 30 weeks (as indicated by most of our results). The estimates are displayed in Table 8. Here we also show the estimates using the different censoring assumptions discussed in Table 6. Columns (1) and (2) focus on the effects on job finding with and without covariates, as in Table 5 above. As expected by the fact that we force identification to be driven by observations which should be more directly affected by benefit exhaustion at 60 weeks, we find substantially larger effects for this sample. Estimates in Columns (3) and (4) where we look at all exits and exits to jobs without censoring are again of approximately the same magnitude.

Finally, we have estimated the model on various sub-samples. The precision is however in general too poor for the results to be informative. However, relying on the model presented in Table 8 to gain precision, we find that men and women are equally affected and that the impact is shared equally across very low skilled (9 years or less) workers and high school educated, whereas we find a zero effect for workers with a tertiary education.

Table 8: Estimates for sample entering week 30 w/o previous programs

	Outcomes			
	(1)	(2)	(3)	(4)
	Job, w/o cov	Job	All exits	Job, w/o cens
31-75 weeks	0.248***	0.217***	0.214***	0.250***
	(0.066)	(0.066)	(0.057)	(0.065)
76-120 weeks	-0.263***	-0.249**	-0.171**	-0.383***
	(0.099)	(0.099)	(0.080)	(0.083)
N (subjects)	11,661	11,661	11,661	11,661
Censored	Other exits and time	Other exits and time	Time	Time
Controls:				
Age at 60 weeks	Yes	Yes	Yes	Yes
Time of inflow	Yes	Yes	Yes	Yes
X-covariates	No	Yes	Yes	Yes

Note: : Non-job exits are censored. Stratified on Age and Year of inflow is controlled for by dummies. X-covariates are dummies for registration month (season), female, married, education, disabled, 10 municipality groups, occupation (eight largest UI funds, blue and white collar workers), immigrant background, if unemployed 2-4 years before registration date, days unemployed previous 4 years and wage before unemployment. Standard errors clustered on individual are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.4 Post-employment outcomes

In this subsection we explore how the reform affected two other aspects of treated individuals' short-run labor market outcomes; wages and job stability. There are two possible mechanisms which could explain the patterns we find, even if we take it as given that the positive effects, as argued, should be interpreted as deterrence effects: Either the unemployed search harder for work when being forced to participate in a program earlier, or workers lower their reservation wages for the same reason. The welfare consequences (even ignoring distributional aspects) differ between these two mechanisms if a lower reservation wage translates into lower average match quality and hence a lower average productivity. In order to shed some light on this issue we have estimated a straightforward difference-in-difference model with log of monthly earnings as the dependent variable and a dummy for 60 weeks duration until workfare as the variable of interest alongside dummies for age and time as well as all the covariates from the hazard models.

The regressions can only include those that do find jobs and it is therefore likely that the estimates will be negatively biased if those that do find jobs as a consequence of the reform are less qualified (in an unobserved sense) than those that would have found jobs even without the reform.

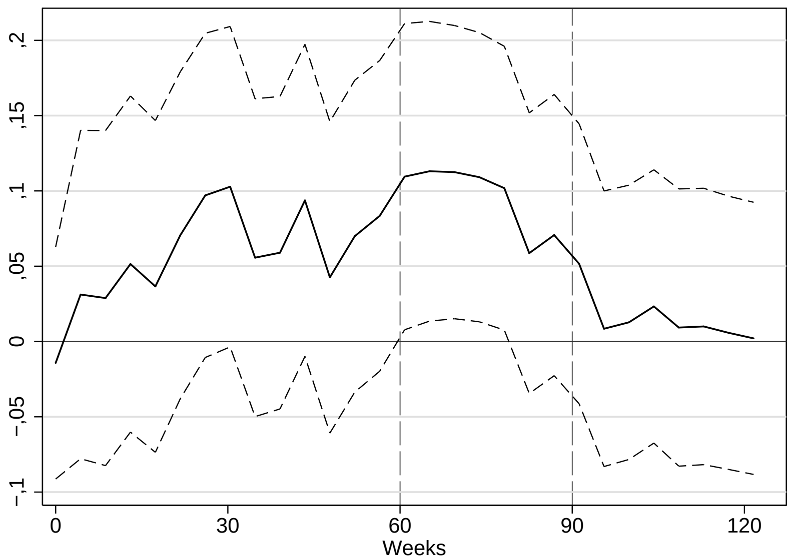

Figure 5: Estimated wage effects among those who find employment

Figure 5 shows the estimated effects from regressions where the sample consists of those who have employment in each of the months (respectively) since inflow into unemployment (we have rescaled the figure into weeks). The point estimates suggest that monthly earnings are largely unchanged during the first 30 weeks (7 months) since the start of the spell. In the 30 to 90 weeks interval, the point estimates suggest that wages have been increased somewhat (five to ten percent) as a consequence of the reform. Clearly, however, the precision of the estimates is fairly poor, and only a few of the individual estimates are therefore significant. When estimating an overall effect pooling observations in the 30 to 75 week interval, we do find a significant effect however. In particular since it seems reasonable to assume that these estimates are negatively biased through selection, these results thus speak against falling reservation wages as an important mechanism behind the main results.

Although the reform does not seem to have induced workers to accept lower wages as a means to leave unemployment, a related potential concern is that they may have started to accept jobs with lower job stability, and hence return to unemployment at a higher frequency. In order to investigate this issue Table 9 show difference-in-difference estimates for the *incidence* of employment at differnet (short-run) horizons. The results show, as expected, that the incidence increased as a results of the reform. To get a more direct description of the return frequency, we have also estimated the share returning as unemployed (among those finding a job) in a difference-in-difference setting. Results are presented in the lower half of Table 9. Conditional on the same caveats as the wage analysis described above, we do not find any indications suggesting that those leaving unemployment due to the reform should have higher (or lower) probability of returning to unemployment. Thus, our results suggest that jobs that were found as an effect of the reform, on average were about as stable as the average post-unemployment job.

Table 9: Share working and share returning to PES.

	1996-1997		1998-1999		
	Control mean	Treatment mean	Control mean	Treatment mean	DD
Share working					
in w 75	0.429	0.416	0.470	0.500	0.043***
before w 76	0.643	0.634	0.668	0.691	0.033**
Share returning to PES within 1 year of:					
All finding work w 5-120	0.211	0.208	0.200	0.196	-0.001
Finding work w 30-75	0.191	0.194	0.194	0.188	-0.009

5 Discussion

We have studied the effects of reduced UI-duration on the employment probability of older workers. We argue that UI-exhaustion in the Swedish context is irrelevant as a financial incentive since other transfers form a perfect substitute. Since the main alternative transfer is conditional on program participation, and work practice programs were offered to all with expired benefits, we interpret the effects as being driven by changes in the time until programs become a pre-requisite for benefits (workfare).

We find substantial effects. The outflow to work increased by 10 percent in the 30 to 75 week interval. Since this effect appears before and during the program-entry phase it is most likely due to the fact that mandatory programs confiscate leisure. Our analysis of wages among those that do find employment does not suggest that earlier workfare reduces reservation wages or match quality, which points to increased search intensity as the most likely driving force.

We are unable to make firm conclusions regarding the effects on job hazards *after* the programs for two reasons. The first is the fact that we study changes in the duration until workfare. This means that the counterfactual is one where workfare starts to bind 30 weeks later. The comparison later on in the spells therefore becomes one between post-program effects among those in the "early workfare group" and pre-program effects among those in the "late workfare group". The second reason is the risk of dynamic selection. The large effects we find earlier in the spells suggest that the composition of workers may have changed substantially after the 75th week.

In order to provide a rough measure of account of how large the effects are, we calculated the expected number of days in unemployment with and without the treatment.[17] Results suggest that the behavioral response to the threat of treatment, on average, reduced the time until employment by 12 days.[18] In order to get a rough sense of how these benefits relate to the costs, we estimated the impact of the reform on the total number of days spent in a labor market program for the same sample.[19]. The estimates suggest an increase of 22 days due to the reform. Thus, taken at face value, the benefits of 12 days of reduced

[17] Based on the estimated effects for the interval 30 to 75 weeks displayed in table 8 and the empirical post-treatment hazard, we compute and compare actual and counterfactual cumulated job-finding hazards.

[18] Allowing for other exits, raises the estimate to 14 days, thus suggesting that the main impact was through increased transitions to jobs. The reduction in unemployment corresponds to 20 days per worker who actually remain in unemployment until the end of the period.

[19] Using data from weeks 30 to 120

unemployment was achieved at the cost of 22 more days in a program on average.

Interestingly, our pre-program results are of a similar magnitude (in terms of hazards), and show a similar time pattern, as in the Forslund and Skans (2006) study of mandatory programs for short-term unemployed youths in Sweden. This suggests that the pre-program effects of mandatory programs vary very little between these two groups, despite very different work histories and average job-finding rates. This suggests that the one-sided approach taken in many countries (e.g. the UK and Sweden) where early mandatory programs are focused solely on youths may be difficult to motivate, in particular since the workfare effects seems to be of a short-run nature also for the youths.

Our analysis also suggests that the fact that alternative transfers often are poorly documented in studies of how benefit durations affect job finding is a shortcoming in the existing literature. Although the Swedish example is extreme, there are many other examples of benefits that may take effect after UI expires, such as social assistance, pensions, sickness insurance, disability insurance, and student grants. Without knowing the extent of alternatives, and the conditions under which these are granted, it is very difficult to provide a clear interpretation of the effects of UI-benefit duration. Since the effects of economic incentives provide a keystone in many calibration studies of optimal unemployment insurance, it is important to filter out the exact magnitude of the economic incentives by providing proper measurement of alternative transfers.

References

Addison, J. T. and Portugal, P. (2004). How does the unemployment insurance system shape the time profile of jobless duration? *Economics Letters*, 85(2):229–234.

Andersen, T. M. and Svarer, M. (2007). Flexicurity - Labour market performance in Denmark. *CESifo Economic Studies*, 53(3):389 –429.

Black, D. A., Smith, J. A., Berger, M. C., and Noel, B. J. (2003). Is the threat of reemployment services more effective than the services themselves? Evidence from random assignment in the UI system. *The American Economic Review*, 93(4):1313–1327.

Calmfors, L., Forslund, A., and Hemström, M. (2004). The effects of active labor-Market policies in sweden: What is the evidence? In Agell, Keen, and Weichenrieder, editors, *Labor Market Institutions and Public Regulation*. MIT Press.

Card, D., Chetty, R., and Weber, A. (2007). The spike at benefit exhaustion: Leaving the unemployment system or starting a new job? *American Economic Review*, 97:113–118.

Card, D. and Levine, P. B. (2000). Extended benefits and the duration of UI spells: Evidence from the New Jersey extended benefit program. *Journal of Public Economics*, 78(1-2):107–138.

Carling, K., Edin, P., Harkman, A., and Holmlund, B. (1996). Unemployment duration, unemployment benefits, and labor market programs in Sweden. *Journal of Public Economics*, 59(3):313–334.

Carling, K. and Larsson, L. (2005). Does early intervention help the unemployed youth? *Labour Economics*, 12(3):301–319.

de Georgi, G. (2005). The new deal for young people five years on. *Fiscal Studies*, 26(3):371–383.

Dolton, P. and O'Neill, D. (1996). Unemployment duration and the restart effect: Some experimental evidence. *The Economic Journal*, 106(435):387–400.

Dolton, P. and O'Neill, D. (2002). The long-run effects of unemployment monitoring and Work-Search programs: Experimental evidence from the United Kingdom. *Journal of Labor Economics*, 20(2).

Forslund, A., Johansson, P., and Lindqvist, L. (2004). Employment subsidies - a fast lane from unemployment to work? Working Paper 2004:18, IFAU.

Forslund, A. and Skans, O. N. (2006). Swedish youth labour market policies revisited. *Vierteljahrshefte zur Wirtschaftsforschung*, 75(3):168–185.

Geerdsen, L. P. (2006). Is there a threat effect of labour market programmes? A study of ALMP in the danish UI system. *The Economic Journal*, 116(513):738–750.

Geerdsen, L. P. and Holm, A. (2007). Duration of UI periods and the perceived threat effect from labour market programmes. *Labour Economics*, 14(3):639–652.

Graversen, B. K. and van Ours, J. C. (2008). How to help unemployed find jobs quickly: Experimental evidence from a mandatory activation program. *Journal of Public Economics*, 92(10-11):2020–2035.

Hairault, J., Langot, F., and Sopraseuth, T. (2011). Distance to retirement and older workers' employment: The case for delaying the retirement age. *Journal of the European Economic Association*, 8(5):1034–1076.

Ham, J. C. and Rea, S. A. (1987). Unemployment insurance and male unemployment duration in Canada. *Journal of Labor Economics*, 5(3):325–353.

Hägglund, P. (2011). Are there pre-programme effects of active placement efforts? Evidence from a social experiment. *Economics Letters*, 112(1):91–93.

Hunt, J. (1995). The effect of unemployment compensation on unemployment duration in germany. *Journal of Labor Economics*, 13(1):88–120.

Johansson, P. and Skedinger, P. (2008). Misreporting in register data on disability status: evidence from the Swedish public employment service. *Empirical Economics*, 37:411–434.

Katz, L. F. and Meyer, B. D. (1990). The impact of the potential duration of unemployment benefits on the duration of unemployment. *Journal of Public Economics*, 41(1):45–72.

Lalive, R. (2008). How do extended benefits affect unemployment duration? A regression discontinuity approach. *Journal of Econometrics*, 142(2):785–806.

Lalive, R., Van Ours, J., and Zweimüller, J. (2006). How changes in financial incentives affect the duration of unemployment. *Review of Economic Studies*, 73(4):1009–1038.

Meyer, B. D. (1990). Unemployment insurance and unemployment spells. *Econometrica*, 58(4):757–782.

Michelacci, C. and Ruffo, H. (2011). Optimal life cycle unemployment insurance. *Mimeo*.

Røed, K. and Westlie, L. (2012). Unemployment insurance in welfare states: The impacts of soft duration constraints. *Journal of the European Economic Association*, 10(3):518–554.

Røed, K. and Zhang, T. (2003). Does unemployment compensation affect unemployment duration? *The Economic Journal*, 113(484):190–206.

Sianesi, B. (2008). Differential effects of active labour market programs for the unemployed. *Labour Economics*, 15(3):370–399.

Tatsiramos, K. (2010). Job displacement and the transitions to re-employment and early retirement for non-employed older workers. *European Economic Review*, 54(4):517–535.

van Ours, J. C. and Vodopivec, M. (2006). How shortening the potential duration of unemployment benefits affects the duration of unemployment: Evidence from a natural experiment. *Journal of Labor Economics*, 24(2):351–378.

Appendix A Economic consequences of UI expiration

In the paper we show that program participation was indeed affected by UI-duration as expected from our institutional description. We also argue that the institutions are such that workers who remain in unemployment should receive unchanged total transfers when UI expires. Ideally, we would like to document this in the data as well. However, in order to do this, we must rely on less precise data since other transfers than UI-benefits are recorded at an annual frequency in our data.

It is difficult to map the effects of the reform into a good measure of annual transfers since the inflow is dispersed over different parts of the year, and we are therefore not able to analyse the impact of the reforms on the transfers. Instead, we try to illustrate the general picture: what happens to transfers for workers who are entitled to 60 weeks of benefits but who remain unemployed longer than that? In order to allocate the different parts of the unemployment spells into something which is meaningful to analyse at the annual frequency, we sampled all UI-entitled workers aged 45 to 54 who became unemployed in June to October 1997 and who remained unemployed until the end of 1999. These workers should have UI-benefits during most of 1998, but lose their benefits towards the end of that year, and thus be out of benefits during 1999. We put no restrictions on outcomes after the end of 1999.

Figure A1 shows how different forms of annual earnings evolve over time for this sample before (1995-96) and after (1997-) job loss and after UI-exhaustion (1999-). To recap, note that the unemployed can enter programs earlier than in the 60th week, that program participants should receive AS at par with UI during participation, and that receiving AS during six months re-qualifies for an additional UI-benefit period.

The main message from the picture is twofold. First, the level of total insurance is very high. Disposable income (which accounts for all transfers and taxes, and hence is lower than labor earnings before job loss) is on average 142,000 SEK before unemployment and never fall below 105,000 SEK, despite the fact that the sample remain unemployed during two full years. Second, disposable income is unaffected by UI-exhaustion. UI-transfers are much lower in 1999 (as expected), but other transfers (mostly AS) fully compensate for this.

Although the sample is restrictive, the message is fully in line with the in-

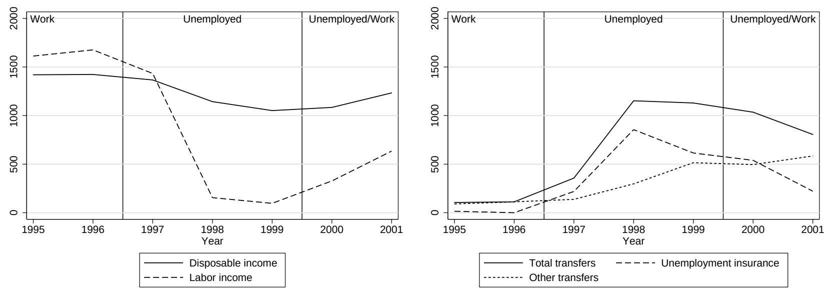

Figure A 9: Annual earnings evolvement over time for long-term unemployed

stitutions at the time: When UI-benefits expire, workers are offered programs and receive AS at an unchanged compensation level. Thus, we will interpret UI-duration as the expected time until program participation becomes a requirement for continued benefits.

Appendix B Modeling program participation

Our main model has been a reduced form Cox-proportional hazard model with job-finding as the outcome. Other exits, such as out-of labor force, have been censored, while program participation has not been separated from open unemployment. The models presented in the paper therefore identify the effects of changes in the anticipation of program participation. Here we also incorporate actual program participation in the model. Participation will, by necessity, vary over spells in a non-predetermined manner.

The reason for modeling program participation is that programs may affect job-finding through other mechanisms then deterrence. The main purpose is thus to separate the different mechanisms through which programs act, in order to rule out other explanations for what we in the main paper regard as deterrence. The mechanisms in mind are locking-in effects during participation and post-program human capital effects. The first mechanism is likely to result in a decrease in job-finding while in program. There may of course be several economical reasons for locking-in effects, which we however leave aside. To us it is merely a way of excluding in-program effects from pre-program deterrence. The second mechanism is the influence of increased human capital, due to participation, on job-finding. This is likely to take effect mostly after the program has come to an end. Since increased human capital and/or improved labor market contacts are probably the very reason for running programs, one would at first expect this to unambiguously improve labor market opportunities. However, since increased human capital also may affect reservation wages, the sign of the total effect is not obvious (Calmfors et al., 2004).

We will thus include variables for current and lagged program participation. Current participation (CP) in week t, is defined as participating in a program in week t-1 (since programs by definition end when jobs start). Lagged participation (LP) is defined as participating in a program in the time span (t-8, t-2). Formally the log hazard is given by:

$$\log h_i(t) = \log \lambda_0^{age}(t) + \alpha_1 CP_i(t) + \alpha_2 LP_i(t) + \sum_{\tau=1} \gamma_\tau D_i^{60} + Year_i \beta^y + X_i \beta^X \quad (2)$$

The estimated models are presented in Table B 1. We find very large negative effects of being in a program and insignificant effects of lagged participation. The table also shows estimates of the reform, γ_τ in three bins, analogous to what we do in the main paper. The effects of being close to benefit exhaustion, the week

31-60 coefficient, remain very close to that of the main specification, presented in Table 5. It should however be noted the interpretation differs somewhat because of the inclusion of program participation variables. These estimates should now be interpreted as the effect of being close to benefit exhaustion, while not yet participating in a program.

Table B 1: Impact of 60 rather than 90 weeks of passive benefits controlling for program participation

	(1) Without covariates	(2) With covariates
-30 weeks	0.072*	0.055
	(0.042)	(0.042)
31-60weeks	0.135***	0.110**
	(0.051)	(0.051)
61-75 weeks	-0.284***	-0.293***
	(0.082)	(0.082)
Current program	-0.173***	-0.178***
	(0.024)	(0.024)
Lagged program	0.054	0.048
	(0.040)	(0.040)
N (subjects)	25,146	25,146
Controls:		
Age at 60 weeks	Yes, strata	Yes, strata
Time of inflow	Yes	Yes
X-covariates	No	Yes
Program participation	Yes	Yes

Note: : Non-job exits are censored. All analyses are stratified on Age. Year of inflow is controlled for by dummies. X-covariates are dummies for registration month (season), female, married, education, disabled, 10 municipality groups, occupation (eight largest UI funds, blue and white collar workers), immigrant background, if unemployed 2-4 years before registration date, days unemployed previous 4 years and wage before unemployment. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Although this model relies on the assumption that selection into programs is handled by the covariates; the results support the notion that the effects of shorter benefits are driven by deterrence effects and not by human capital effects from program participation. Regarding the significantly negative effect of shorter benefits in the week 61-75 bin, the previously stated precautions still hold: it is both affected by selection in the previous period (week 31-60) and by the fact that the reference group (those entitled to 90 weeks of UI) might now be affected by deterrence, since they approaching benefit exhaustion. It should also be noted that the issue on selection into programs implies that estimated effects of current and lagged program participation should be interpreted with caution.

Another aspect of programs foreseen in the presented analyses is that programs can postpone benefit exhaustion, since UI-days are not consumed while participating in programs. Time until exhaustion would hence be measured with better precision if aggregated participation was subtracted. We have done this and estimated the effect of being close to exhaustion. In doing this, spells were measured along the ordinary time axis, from spell start, in the usual manner. This was done both with and without controls for present and previous programs, in the fashion described above. In these analyses we also censored spells when programs had lasted long enough for the jobseeker to get entitled to another 60 (or 90) weeks of UI-benefits. We think we ended up with a somewhat cleaner setup, although losing some of the experimental features of our original design. The results of these analyses were however very similar to those presented above.