IFAU – INSTITUTE FOR
LABOUR MARKET POLICY
EVALUATION

Do interactions between unemployment insurance and sickness insurance affect transitions to employment?

Caroline Hall

WORKING PAPER 2008:18

The Institute for Labour Market Policy Evaluation (IFAU) is a research institute under the Swedish Ministry of Employment, situated in Uppsala. IFAU's objective is to promote, support and carry out scientific evaluations. The assignment includes: the effects of labour market policies, studies of the functioning of the labour market, the labour market effects of educational policies and the labour market effects of social insurance policies. IFAU shall also disseminate its results so that they become accessible to different interested parties in Sweden and abroad.

IFAU also provides funding for research projects within its areas of interest. The deadline for applications is October 1 each year. Since the researchers at IFAU are mainly economists, researchers from other disciplines are encouraged to apply for funding.

IFAU is run by a Director-General. The institute has a scientific council, consisting of a chairman, the Director-General and five other members. Among other things, the scientific council proposes a decision for the allocation of research grants. A reference group including representatives for employer organizations and trade unions, as well as the ministries and authorities concerned is also connected to the institute.

Postal address: P.O. Box 513, 751 20 Uppsala
Visiting address: Kyrkogårdsgatan 6, Uppsala
Phone: +46 18 471 70 70
Fax: +46 18 471 70 71
ifau@ifau.uu.se
www.ifau.se

Papers published in the Working Paper Series should, according to the IFAU policy, have been discussed at seminars held at IFAU and at least one other academic forum, and have been read by one external and one internal referee. They need not, however, have undergone the standard scrutiny for publication in a scientific journal. The purpose of the Working Paper Series is to provide a factual basis for public policy and the public policy discussion.

ISSN 1651-1166

Do interactions between unemployment insurance and sickness insurance affect transitions to employment? ♣

by

Caroline Hall♦

September 3, 2008

Abstract

Previous research suggests that there are substantial interactions between the unemployment insurance (UI) and the sickness insurance (SI) in Sweden. Moral hazard arises in the interplay between these two social insurance systems, since by reporting sick an unemployed person can postpone the UI expiration date and sometimes also receive considerably higher benefits. This paper examines whether these interactions affect the transition rate from unemployment to employment. To study this question I utilize a reform which greatly reduced the incentives for unemployed persons to transfer to the SI. While there is evidence that this reform substantially lowered the incidence of sick reports among the unemployed, I find no evidence suggesting that the reduced sick report rate in turn affected the transition rate to employment.

Keywords: Unemployment insurance, sickness insurance, unemployment duration, health, duration analysis.
JEL-codes: C41, J64, J65, H55, I18

♣ I am grateful to Peter Fredriksson, Erik Grönqvist, Laura Hartman, Pathric Hägglund and Per Johansson for valuable comments and discussions. I would also like to thank Linus Liljeberg for helpful suggestions regarding the data work. The paper has benefited from comments at seminars at Uppsala University/IFAU and University College London. The financial support from the Wallander & Hedelius Foundation, and the Swedish Council for Working Life and Social Research (FAS) is gratefully acknowledged.
♦ Institute for Labor Market Policy Evaluation (IFAU) and Department of Economics, Uppsala University, e-mail: caroline.hall@ifau.uu.se.

Table of contents

1 Introduction

In recent years, several academics as well as policy makers have pointed out that undesired incentive effects arising from the interplay between different social insurance programs within a welfare state may be an overlooked and financially costly phenomenon (see e.g. Krueger and Meyer 2002, and the European Economic Advisory Group, 2007). Many countries have complex social insurance systems and their various parts sometimes overlap in ways that can generate unintended flows between them. This has, for instance, been noted with regard to unemployment (UI) and sickness insurance (SI) in Sweden and Norway (see e.g. Larsson 2006, and Henningsen 2006), UI and disability pensions in Sweden and Finland (see e.g. Karlström *et al* 2006, and OECD 2006), and UI and workers' compensations programs in Canada (see e.g. Fortin and Lanoie 1992). In the presence of such interactions, limiting access to one program may just result in an overflow to other programs. Reforms intended to increase transitions to employment by a change of a single program may then not be very effective. For example, reducing the amount or the duration of UI benefits may not be an efficient way of inducing the unemployed to search harder for jobs if they can easily shift to other benefit programs.

While there is evidence that the interplay between different social insurance programs sometimes does give rise to benefit arbitrage (see e.g. Larsson 2006, and Larsson and Runeson 2007), little research has been done on whether such interactions actually matter for transitions to employment. Pellizzari (2006), who studies interactions between UI and social assistance in 15 EU countries, is an exception. His findings suggest that UI recipients who are also eligible for social assistance are less sensitive to changes in the level and the duration of their UI benefits, and that the interplay between these programs may provide an explanation for the scant success of many labor market reforms in Europe in the past decades. In this paper, I provide Swedish evidence on the interplay between UI and another type of social insurance program, namely the SI, and on whether this interplay affects transitions to employment.

In Sweden, unemployed individuals are able to report sick and receive SI benefits. The rationale behind this rule is that job search is comparable to work. In order to be eligible for UI, an unemployed person should actively search for jobs and be able to accept employment at short notice. A person who looses his or her work (search) capacity due to sickness should therefore be funded by the SI rather than the UI. Previous research on the interplay between these two insurances, however, suggests that health deterioration is not the sole explanatory factor for transitions to the SI. The probability of transferring to the SI is affected by the relative compensation in the two systems; unemployed persons are more likely to report sick if their replacement rate is higher in the SI than in the UI (Larsson and Runeson 2007). The probability of reporting sick is also found to increase drastically as the UI expiration date approaches, suggesting that the SI may be used as a way of postponing the UI expiration date (though it cannot be excluded that the peak is at least partly driven by health deterioration due to stress) (Larsson 2006, and SFU 2007).

If transitions to employment would follow a similar trajectory regardless of shifting to the SI or remaining in the UI system, this type of interplay should perhaps not cause too much concern. Of course, government spending increases if the SI benefits are higher than the individual's alternative benefits, but the costs would be much larger if these UI-SI interactions in general also worked to prolong the individuals' time out of employment.

There are in fact several reasons for why the source of funding (UI or SI) may matter for the incentives to find work. Being on UI is associated with a number of rules, the purpose of which is to increase transitions to employment: the worker is obliged to apply for and accept jobs, otherwise a sanction may be imposed[1]; benefits are reduced after 100 benefit days[2]; and there is a formal time limit on how long benefits can be received[3]. SI benefits, on the other hand, are not associated with any similar requirements

[1] Some recent empirical studies from the Netherlands and Switzerland suggest that imposing sanctions in the UI substantially raises the exit rate from unemployment, see e.g. Abbring *et al* (2005) and Lalive *et al* (2005).
[2] The question of how the UI benefit level affects job finding has received extensive attention in the economic literature. A recent survey of the evidence is provided by Krueger and Meyer (2002). The US studies surveyed imply an elasticity of unemployment duration with respect to the UI benefit level in excess of 0.5. The results from other countries are more varied. Carling *et al* (2001) suggest an elasticity of 1.6 for Sweden.
[3] Several empirical studies find that the transition rate from unemployment to employment increases as the UI expiration date approaches. See e.g. Moffitt (1985), Meyer (1990), and Katz and Meyer (1990) for evidence from the US. Swedish evidence is reported by Carling *et al* (1996).

and have in principle unlimited duration.[4] Hence, if the UI rules work as intended, funding from the UI rather than the SI could (for a given health status) be expected to be associated with a higher search effort.

In order to study whether transitions to SI among the unemployed affect the transition rate to employment, I use a reform in July 2003 which changed the relative compensation in the two systems. The reform reduced the SI benefit cap (i.e. the maximum amount) so as to correspond to the UI benefit cap, in order to prevent unemployed workers from receiving higher benefits by reporting sick. Before the reform, SI benefits could exceed UI benefits by up to 20 percent. Larsson and Runeson (2007) find that this policy change led to a large (36 percent) decline in the sick report rate among the unemployed affected by the reform. The question of interest here is whether the reduced sick report rate in turn translated into a higher rate of job finding.

To identify the effect of the reduced SI benefits (relative to the UI benefits) on the transition rate to employment, I use the fact that the reform affected various groups of unemployed persons differently and at different durations of unemployment. First, as workers became unemployed at different dates, the reform affected them at different lengths into their unemployment period. This variation can be used to separate the reform effect from the effect of unemployment duration. I do this by comparing the hazard to employment for people who experienced the reform at different stages of their unemployment period. Second, only those with previous wages above the UI benefit cap were affected by the reform. The change in transitions to employment for those with previously lower wages can thus be used to control for calendar time effects (such as business cycle effects) around the time of the reform, which were common to the two groups.[5]

My results suggest that, while the reform significantly reduced sickness absence among the unemployed, this did not matter for the transition rate to employment. For those who reduced their sick report rate due to the reform, spending more time in the UI

[4] These were the rules in place during the time period for which I have data in this paper.
[5] Larsson and Runeson (2007) use a similar identification strategy.

rather than the SI did not seem to shorten the time out of employment. This finding is robust across various sensitivity tests. Hence, while there are important interactions between these two social insurance systems, I find no evidence suggesting that these interactions affect the job finding rate among the unemployed workers.

The outline of the rest of the paper is as follows: In Section 2 I describe the central features of Sweden's UI and SI systems, as well as the SI reform in July 2003. Section 3 discusses theoretical issues. The empirical strategy is described in Section 4, and Section 5 presents the data. The results as well as a number of sensitivity checks are shown in Section 6. Finally, Section 7 contains concluding remarks.

2 Unemployment and sickness insurance in Sweden[6]

The UI and SI constitute essential parts of the public social insurance system in Sweden. Their purpose is to insure against income losses due to involuntary unemployment (the UI) and sickness (the SI). Both insurances provide income-related compensation up to a cap and the benefits are for the most part financed by taxes.

2.1 Description of the unemployment insurance

The UI consists of two parts: a *basic insurance* offering a fixed amount of compensation and a voluntary *income-loss insurance* providing income-related benefits. In order to be eligible for any kind of UI benefits, an unemployed person must be registered at the public employment service (PES) as a 'job seeker' and be willing to accept employment. Qualification for income-related benefits additionally requires that the person has been a member of a UI fund for at least twelve months prior to unemployment (the membership condition) and that he or she has worked during at least six of these months (the working condition). If the person fulfills the working condition but not the membership condition, he or she is entitled to the fixed basic amount of compensation.[7]

[6] This section describes the rules in place during 2003.
[7] For a detailed description of the UI see e.g. www.aea.se.

The UI is administered by a number UI funds that together enroll about 85 percent of the work force. The PES controls that the unemployed fulfill the rules concerning job search. To receive UI benefits, an unemployed person has to meet his or her caseworker at the PES regularly and apply for any job the caseworker assigns him or her. If the person does not meet these requirements, he or she can be submitted to a sanction in the form of reduced or suspended benefits. The sanctions can be time-limited or permanent, depending on if the person has broken the rules before, and on the expected length of the job he or she refuses to accept.[8]

In 2003, when the reform was implemented, the UI benefits were time-limited to 300 workdays (60 weeks) and could be received either continuously or with breaks in the unemployment period. For individuals reaching the end of their benefit period, a PES caseworker would assess the need for intensified counseling. If such a need was found, the person would get assigned to a specific labor market program[9]. Refusing to participate would lead to benefit expiration. If intensified counseling was not found necessary, the unemployed would get entitled to a new benefit period of 300 days. Such an extension was however only possible once.

The UI replaced 80 percent of the worker's average earnings during the last six month of employment, with a lower and an upper limit. See *Figure 1* for an illustration. The lower limit was SEK 7,040 (≈ € 760)[10] per month and equaled the fixed basic amount. The maximum amount varied depending on how long the person had been unemployed. During the first 100 benefit days, the cap was 80 percent of a monthly wage of SEK 20,075 and after that the cap dropped to 80 percent of SEK 18,700. No compensation was given during the first five days of an unemployment period.[11]

[8] An unemployed person does not necessarily have to accept *any* job in order to receive further UI benefits. Factors such as the family situation and the duration of unemployment are taken into account in the judgement.

[9] The program was called *Activity Guarantee* and implied full time activity. Participants were offered counselling and the whole spectrum of programs and services available at the PES. The economic compensation equaled the UI benefits.

[10] Exchange rate May, 2007.

[11] If the unemployment was voluntary, that is if the person had quit his or her job without a valid reason, the uncompensated period was up to 45 benefit days. For those who had been laid off because of improper behaviour, the suspension period could be even longer.

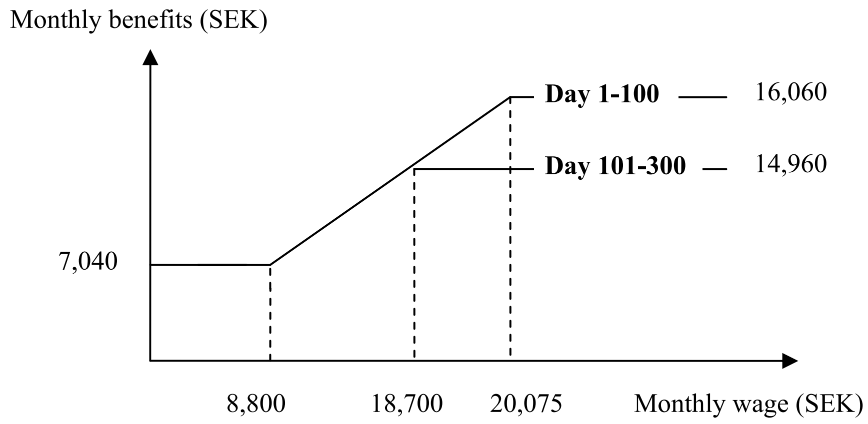

Figure 1 UI benefits in 2003

2.2 Description of the sickness insurance

The SI provides economic compensation when a worker is too sick to carry out his or her regular job. All employed workers are automatically covered by the insurance. Unemployed workers who have previously been employed are also eligible, as long as they are registered as 'job seekers' at the PES. The size of the SI benefits depends on the person's wage prior to the sick period. For unemployed workers however, the benefits are based on the wage prior to unemployment.

The Social Insurance Agency is responsible for the SI compensation for unemployed workers. The first day of a sickness period is always uncompensated. During the first seven days it is up to the individual to judge whether he or she is too sick to work (search). Thereafter, the person needs a certificate from a doctor in order to receive additional benefits. In 2003, there was no formal time-limit for how long SI benefits could be received.

In the beginning of 2003, the SI replacement rate was 80 percent of the previous (pre-unemployment) wage. Hence, the replacement rate was the same as in the income-related UI.[12] The minimum wage for receiving any SI benefits at all was SEK 775 per month, and the maximum SEK 24,125 per month. In other words, SI benefits varied

[12] However, the two insurance systems define the earnings on which the benefits are based somewhat differently. While the UI benefits are based on the worker's average earnings during the last six months of employment, the SI benefits are based on an estimate of the earnings a worker would have had during the sickness period.

between SEK 620 and SEK 19,300 per month.[13] This meant that the maximum monthly SI benefits exceeded the maximum monthly UI benefits. The reform on 1 July 2003 changed the marginal replacement rate in the SI in two ways. *Figure 2* illustrates how the changes affected unemployed workers. First, the reform reduced the marginal replacement rate to 77.6 percent. This change concerned all insured, employed as well as unemployed. Second, for the unemployed insured, the maximum SI benefits were reduced to SEK 16,060 per month, which corresponded to the maximum monthly UI benefits. The purpose of the latter part of the reform was to prevent unemployed persons from receiving higher benefits by reporting sick.

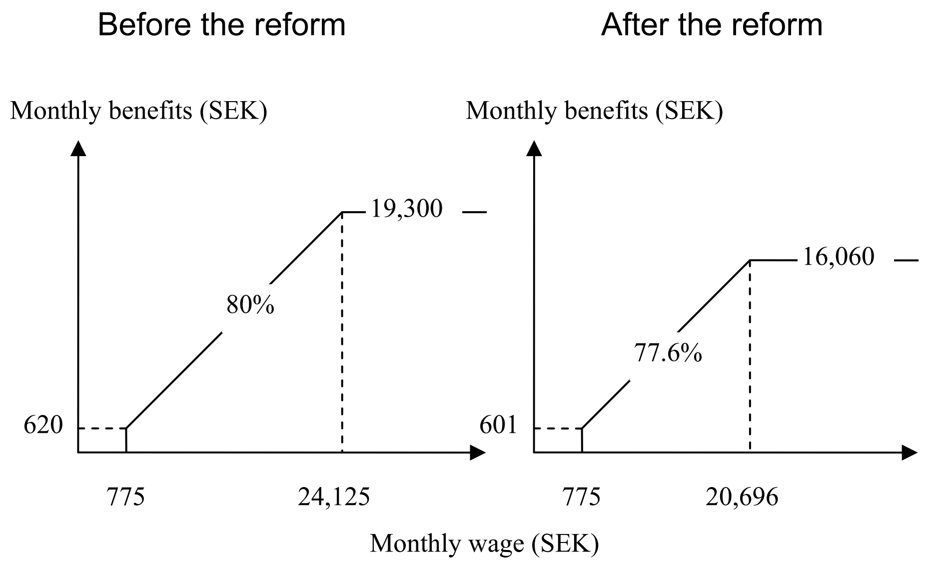

Figure 2 SI benefits for unemployed workers, before and after the reform in July, 2003

An additional aspect of the second part of the reform, which is important for this study, is that it affected *all* unemployed insured, i.e. even those with already ongoing UI spells had their SI benefits reduced on 1 July 2003. This feature turns out to be important for the identification strategy (see *Section 4*).

[13] The numbers in this section do not account for the first uncompensated day in a sickness period.

3 Theoretical issues

Sickness absence and unemployment, though two states both representing substantial losses of work time, are typically not analyzed jointly. Sickness absence has most commonly been analyzed within the framework of a labor supply model, and the focus has generally been exclusively on employed workers (see Brown and Sessions 1996, for a survey of the work absence literature). Holmlund (2005) develops a theoretical framework that incorporates both unemployment and sickness absence as distinct labor force states. Moreover, sickness absence may occur both among employed and among unemployed workers. This model thus allows for interdependencies between policies concerning unemployment and sickness absence and is suitable for a unified analysis of labor market effects of changes in sickness and unemployment benefits.[14]

The Holmlund (2005) model includes four different labor force states: *work*, *sickness absence*, *unemployment* and *non-participation*. Individuals in the first two states are employed, whereas those in the second two states are non-employed. Sickness absence among unemployed workers is regarded as non-participation. Contrary to the state of unemployment, individuals in this state do not search for jobs actively, and hence the probability of finding employment is lower (though it is still positive since the individuals may be contacted by an employer).

Each of the four labor force states is associated with a particular present discounted value of utility. This value depends on the income in the current state as well as on incomes in all other potential states, since chance and choice induce the (homogenous) workers to move between states. Employed workers face a risk of job loss and non-employed workers face a chance of finding a job. Workers are also exposed to random (daily) shocks of sickness, which affect their disutility of work and job search. The key decision for employed individuals is to choose between work and sickness absence, and for the non-employed, to choose between search and inactivity, i.e. between unemployment and non-participation.

[14] This model ignores the behavior of firms and focuses on the supply side. See Engström and Holmlund (2007) for an extension of the model that also incorporates firm behavior.

The optimal behavior is characterized by *reservation values of sickness*. The employed prefer sickness absence rather than work for sufficiently severe sickness shocks; and similarly, the non-employed prefer non-participation rather than job search for sufficiently serious realizations of sickness. The critical values of sickness generally differ between employed and non-employed workers, and are determined by benefits and other parameters of the model. For example, the reservation value of sickness is higher, the higher is the relative economic gain of being active rather than inactive. Hence, for non-employed workers, the probability of reporting sick is lower, the lower are SI benefits relative to UI benefits. The reservation level of sickness also depends on differences in transition probabilities; e.g., the higher the relative returns to active job search, the lower the probability that non-employed workers report sick.

A decrease in SI benefits targeting only non-employed workers, such as the one in Sweden in July 2003, has straightforward implications in this framework. First, reduced SI benefits for non-employed individuals will have a direct positive effect on their reservation level of sickness, making non-employed individuals less inclined to report sick. There will also be a wealth effect working in the same direction since the value of non-employment decreases relative to the value of employment, which makes active job search more attractive.[15] Second, since the probability of finding a job is higher in unemployment than in non-participation by assumption, the higher reservation level of sickness will also translate into a higher job finding rate among the non-employed workers.

4 Identification strategy

The question of interest in this paper is whether the reform in July 2003 affected the transition rate to employment, through its effect on the sick report rate. To identify the effect of the reduced SI benefits (relative to the UI benefits), I exploit two features of the reform: (i) As workers became unemployed at different dates, the reform affected them at different durations of unemployment. By exploiting this variation, one can

[15] If the risk of job loss is higher for workers on sick leave than for those at work, the reservation level of sickness will also increase for employed workers, since the incentives to prevent a job loss by attending work increases.

separate the reform effect from the effect of unemployment duration. (ii) Only those with previous wages above the UI benefit cap were affected by the reform. The change in transitions to employment for those with previously lower wages can thus be used to control for calendar time effects (such as business cycle effects) around the time of the reform, which were common to the two groups.

I begin by describing more closely how the reform affected the difference between UI and SI benefits for various types of unemployed persons.[16] Recall that the difference depended on: (i) the wage prior to unemployment, and (ii) whether the person had received UI benefits for less or more than 100 days. *Figure 3* shows the case of an unemployed person who has not yet passed the 100-day limit in the UI, i.e. before the UI benefit cap drops.

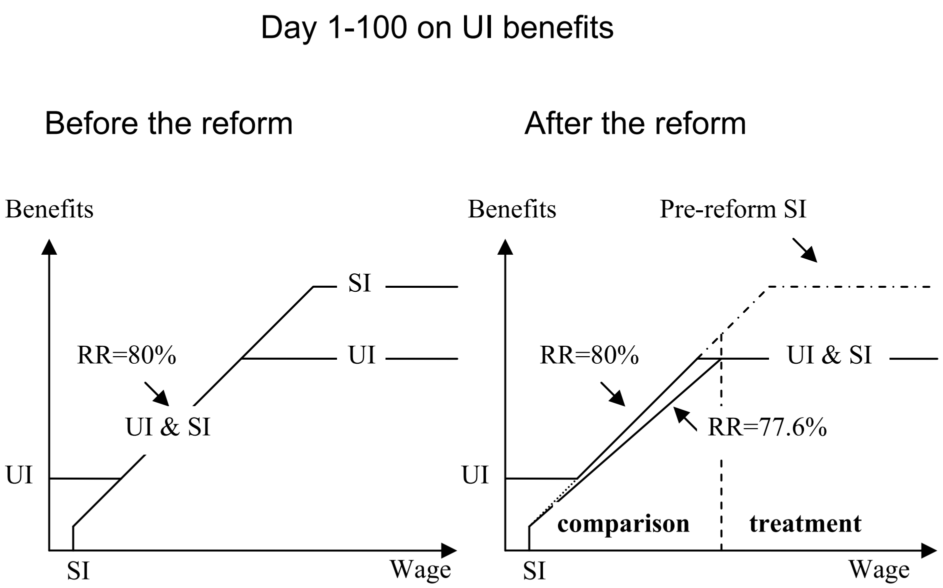

Figure 3 The change in SI benefits due to the reform, during the first 100 UI benefit days

[16] The following paragraphs in this section build extensively on the description in Section 3 in Larsson and Runeson (2007).

The reform lowered the SI benefits for everybody, as the marginal replacement rate was reduced from 80 to 77.6 percent. Thus, SI benefits were reduced relative to UI benefits for all unemployed persons. However, up to the previous wage of SEK 20,696 the reduction in SI benefits was relatively small; amounting to only 3 percent.[17] I will refer to this group as the comparison group. For unemployed persons with a previous wage above that cut-off, the treated, the reform implied a reduction of the SI benefits that varied from 3 up to almost 17 percent.

The situation is somewhat different for the unemployed who have passed the first 100 UI benefit days, as the cap in the UI then is lower. This implies that even after the reform, SI benefits are higher than UI benefits for high-wage unemployed persons. However, the effect of the reform on the *difference in SI benefits* is similar to *Figure 3*: up to a previous wage of SEK 20,696 the SI benefits were reduced by 3 percent. From that level upwards, the reduction was larger the higher the previous wage, varying between 3 and almost 17 percent. So again, the population can be divided into treated and comparisons according to the previous wage, the cut-off being at SEK 20,696.

I will analyze the effect of the change in SI compensation on the hazard rate to employment, that is, the rate at which a person finds a job at time *t* of unemployment, conditional on remaining unemployed up until this point in time. In addition to making use of the treatment and comparison group, the identification strategy used exploits the timing of the reform. The timing feature arises when one uses duration data and has a fixed reform date. As workers be-come unemployed at different dates, the reform affects them at different durations of unemployment. This variation can be used to separate the reform effect from the effect of unemployment duration. I do this by comparing the hazard to employment for people who experienced the reform at different stages of their unemployment period. For example, the unemployed who experienced the reform 8 weeks into their unemployment spell are compared with those whose unemployment spells are at least 8 weeks, but who did not experience the reform until after week 8 or never.

[17] Persons with very low previous earnings are an exception, as the reform also implied a marginal reduction of the minimum wage for SI eligibility; from SEK 620 to SEK 601. Hence, persons in this income group became eligible

This strategy makes it possible to identify the effect of the reform *date*. However, it is likely that other changes on the labor market occurred around the time of the reform which also affected transitions to employment. In order to separate the effect of the reduced SI benefits from such factors, I compare the reform-date effect for the treatment and the comparison group. A larger effect for the treated, who experienced a larger benefit cut, will indicate responsiveness to the SI compensation size. The policy change which is used to identify the behavioral response to the SI benefit level is thus not the entire reduction in SI benefits due to the reform, but rather the reduction over and above the general 3 percent reduction in the replacement rate. The effect of the 3 percent reduction cannot be separated from the effects of other changes around the time of the reform.

To estimate the effect of the policy change, I use a Cox regression model. The baseline specification to be estimated can be written as:

$$(1) \qquad \lambda(t) = \lambda_0(t)\exp\left\{f\left(\mathbf{x}, \mathbf{z}(t); \mathbf{\Omega}\right) + \delta D_t^{reform} + \gamma D^T + \beta D_t^{reform} D^T\right\},$$

where λ_0 is the baseline hazard, i.e. the pre-reform hazard to employment. $f(.)$ is a function of time-invariant covariates, $\mathbf{x}$, and time-varying covariates, $\mathbf{z}(t)$, and $\mathbf{\Omega}$ is a vector of parameters corresponding to the covariates.[18] D_t^{reform} is a time-varying dummy variable, where $D_t^{reform} = 0$ prior to the reform and $D_t^{reform} = 1$ thereafter. $D^T =$ is a dummy for the treatment group, where $D^T = 0$ if the previous wage is below SEK 20,696 and $D^T = 1$ for wages above that. The effect of the reduction in the SI benefit cap is obtained by comparing the change in hazard rates for the treated and the comparisons after the reform. The effect of the policy change is given by the coefficient of the interaction variable, β.

for SI and thus experienced a benefit increase. However, there are no observations in this income interval in the sample studied in this paper.

[18] The covariates included are sex, age, age^2, immigrant background, marital status, presence of children younger than 18, level of education (4 categories), type of education (10 categories), ln(pre-unemployment wage), number of days left until UI benefit expiration (6 categories), and indicators of the local labor market (county) as well as the month of entry into unemployment.

The identifying assumption behind this ‘difference-in-differences’ approach is thus that the development over time of the hazard to employment in the comparison group, captures the counterfactual development in the treatment group, had the reform not occurred. This assumption is violated if, for example, the labor market opportunities developed differently for the two groups around the time of the reform. Divergent trends at other times during the sampling period may also be problematic as they may lead to divergent compositional changes in the two groups regarding unobserved factors.[19]

In order to check whether the estimates are affected by compositional changes in unobserved factors, I also estimate stratified models. I use the month of entry into unemployment as well as the local labor market as stratification units. This specification should be less sensitive to unobserved heterogeneity as the reform effect is identified solely by comparing individuals beginning their unemployment period during the same month *and* in the same local labor market. To check whether the results could be biased due to divergent changes in labor market opportunities for the two groups, I try to assess whether such changes have taken place during the relevant time period. I also re-estimate the model for a few different sub-samples which are more similar than the treated and comparisons in the baseline model, in terms of the pre-unemployment wage. Finally, to test whether the results may be biased as a result of divergent seasonal patterns for the two groups, I analyze the effects of a hypothetical reform supposed to have occurred on the same date the year after the actual reform. The results turn out to be robust in these respects.

5 Data and sampling

5.1 Data

I combine data from several different sources for the empirical analysis. The database *ASTAT*, originating from the unemployment insurance funds, *HÄNDEL,* from the PES, and the *Sickness Benefit Register* (*SFR*) from the National Social Insurance Board,

[19] It is well known that problems with unobserved heterogeneity are particularly important to handle when estimating duration models. Contrary to usual regression models, even unobserved heterogeneity which is uncorrelated with the included covariates may cause biased coefficients.

constitute the main sources. These databases are all a part of *LINDA*, which is a register-based longitudinal database that includes about 3 percent of the Swedish population.[20] LINDA additionally contains several demographic variables collected from e.g. tax registers.

ASTAT contains weekly information on UI benefit payments, as well as on the number of days left until the benefits run out, for all unemployed persons who have received either basic-amount or income-related benefits. It is most common to receive income-related benefits; during 2003 only about 9 percent of all benefit days were on the basic-amount. For those entitled to income-related benefits, the database also includes information on the previous wage.

I use ASTAT as the data source for unemployment spells, which implies that the condition for being defined as unemployed is to receive funding from the UI.[21] Since data on the previous wage does not exist for those who are only entitled to the basic-amount of UI benefits, I exclude this group from the sample. Data on the pre-unemployment wage is needed in order to determine a person's SI compensation in case of sickness.[22]

SFR contains data on sickness spells for all persons who have been entitled to SI benefits. SI benefits can be given on a full or part-time basis, and they can be of a few different types: regular benefits for illness, compensation for work related injury, rehabilitation benefits, and benefits for preventive care. Regular SI benefits for illness are the most common and were paid out during about 89 percent of all SI spells starting in 2003.

I merge ASTAT with SFR in order to track the length of unemployment spells during which the individual switches to SI benefits. Hence, sickness spells that occur during a UI benefit period (at the latest, start the week after the payments from the UI stops) are considered to be a part of the unemployment spell. Naturally, the same spell continues if

[20] For a detailed description of LINDA, see Edin and Fredriksson (2000).

[21] This means that participants in labor market programs and individuals who are registered at the PES as unemployed, but who are not qualified for UI benefits, are not included in the sample.

[22] Since the income measure on which the SI benefits are actually based only exists for those who have reported sick, I use the income measure reported by the UI funds to calculate the individual's SI compensation in case of sickness. Unless the person worked very irregularly before unemployment, the two income measures should be approximately equal.

the individual later switches back to UI benefits. All types of SI periods are included and, for simplicity, I make no distinction between them.

Neither ASTAT nor SFR contain any information on why the benefits stop, at the end of a spell. Therefore, in order to determine whether or not an unemployment period ends in employment, I use information from HÄNDEL. HÄNDEL consists of data on the individual's labor market status, e.g. unemployed; employed; or participant in a labor market program, and on transitions between such states, for all persons who are registered at the PES. Since registration is compulsory in order to receive UI benefits, the records should include all UI benefit recipients.

I use the individual's registered labor market status the week after the UI (or SI) benefits stop to define whether or not the spell ends in employment. If the worker is still registered in a state pertaining to unemployment during this week, I instead use the labor market transition closest in time after as the reason for benefit interruption, given that the transition occurs within the next four weeks.[23] Both permanent and temporary jobs are classified as employment, as long as they cause a break of at least three weeks in the UI benefit payments.[24]

5.2 Sampling and descriptive statistics

I construct the sample by selecting all individuals who began an unemployment period with income-related UI benefits during the period 1 December 2002 – 31 December 2003. The reason for not sampling before December 2002 is that the wage information is incomplete before this point in time.[25] An unemployment period is considered to begin when a person who has not received UI benefits during the last three weeks, starts to receive UI benefits. I require that the unemployment spells begin with funding from the UI, i.e. I do not include persons who became unemployed during or directly after a sickness period.

[23] The reason for allowing this gap is that ASTAT and HÄNDEL do not match perfectly in this aspect. The discrepancy is most likely due to that there is no flow of information from the UI funds to the employment agencies regarding the individual's benefit payments or labor market status. The information in HÄNDEL is instead given to the employment agency by the individual or his or her employer.

[24] A person is defined as employed if, when the benefits stop, he or she has left the register due to permanent or temporary employment (the variable "avaktualiseringsorsak" is equal to 1, 2 or 3), or if he or she is registered as temporarily employed or as job changer (the variable "sökandekategori" is equal to 31 or 41).

[25] Before this date, the wage variable is capped for individuals belonging to some of the UI funds.

Each unemployment spell that begins during the sampling period is followed during, at most, 60 weeks or until the end of 2004. The spell length is measured in weeks. An unemployment period ends when there is a break in the UI payments, other than due to sickness, which is three weeks or longer. That is, very short intervening employment periods or other breaks are treated as part of the unemployment period. If a UI period ends for some other reason than employment, e.g. because the person starts an education; a labor market program (including subsidized employment programs); or if the reason is unknown, the spell is treated as censored.

Quite a large share of the unemployment spells, almost 15 percent, end for unknown reasons; either the PES has registered that they have lost contact with the person, or the data contains no reason for the UI benefit interruption[26]. It is likely that some of these spells end in employment. People who have found a job may not see any reason to contact the PES. If the job is short term only, such persons are likely to remain registered as unemployed during the subsequent employment period, and hence I do not observe any reason for the UI benefit interruption in the data. If the job is long-term, the PES will at some point report that they have lost contact with the person.[27] To the extent that the fraction of spells ending for unknown reasons that actually end in employment differ systematically between unemployment spells that include sickness spells and those that do not, this may bias the estimate of the reform effect. To check whether the results are sensitive to how these spells are treated, I have re-estimated the model treating all spells ending for unknown reasons as ending in employment. As it turns out, this does not affect my findings.

This sampling procedure results in a sample of 19,291 unemployment spells. However, almost 12 percent are excluded since the person cannot be found in the HÄNDEL registers during the relevant time period, or due to inconsistent information in HÄNDEL. I also exclude workers older than 60 and workers with reported work related disabilities. Finally, I exclude a few spells where the worker's previous wage is below the limit for SI eligibility. The resulting sample contains 11,022 unemployed persons

[26] In most of these cases the person is still registered as full time unemployed in HÄNDEL.

[27] Bring and Carling (2000) have conducted a follow-up study of 'lost contact' individuals. They find that almost 50 percent dropped out due to employment.

and 14,935 unemployment spells. About 24 percent of the individuals have multiple unemployment spells.

Table 1 and *Table 2* below present descriptive statistics, separately for the treatment and the comparison group. Table 1 gives statistics on the duration of the unemployment spells as well as on the reason for benefit interruption. We see that the average spell length is about two weeks longer for the treated than for the comparisons. This could be due to that it is more common among those in the comparison group to have breaks of a few weeks in their unemployment periods. Recall that an unemployment spell – as defined here – ends if there is a break in the benefit payments that is three weeks or longer. Since repeated unemployment is more common among the comparisons, this group in general has fewer days left until their UI benefits expire in the beginning of their unemployment spells. The proportion of spells ending in employment also differs between the groups; while 35 percent end in employment for the treated, this share is only 21 percent for the comparisons. Compared to previous Swedish studies on unemployment duration, these shares appear low; e.g. in Carling et al (2001) the proportion of spells ending in employment is nearly 47 percent. There are a couple of reasons for why this share is much lower in my sample: I censor spells at an earlier duration of unemployment; the proportion of spells ending for unknown reasons (which could be employment) is much higher; and I am more likely to observe breaks in the unemployment spells in my data, compared to in the data used in previous studies.[28]

The most common reason for benefit interruption in the comparison group is 'other destinations'; 42 percent of the spells end in this category, compared to 22 percent among the treated. Other destinations include e.g. education and part time unemployment (without UI benefits). Among these, the latter is the most common destination in the comparison group.

[28] Most previous Swedish studies have used HÄNDEL to measure unemployment duration. This data is less appropriate here, since there is no consistent way of handling individuals who transfer to SI benefits in this register. Short sickness spells are likely to be unnoticed in HÄNDEL, whereas a person who gets SI benefits for a longer time period either leaves the register at some point during the sickness period, or is moved to the category 'others registered'. The PES generally has less frequent contact with individuals in this category, which means that the information on unemployment duration is likely to be less accurate also for these individuals.

Table 1 Spell characteristics

	Treatment group	Comparison group
Number of unemployment spells	3 368	11 567
Number of individuals	2 696	8 408
Proportion of ind. with multiple spells	0.19	0.25
Days left until UI benefit expiration (in the beginning of the spell)	224	202
Proportion of spells lasting longer than		
10 weeks	0.67	0.61
20 weeks	0.44	0.40
30 weeks	0.31	0.28
40 weeks	0.22	0.21
50 weeks	0.16	0.14
Average spell length (weeks)	23.7	21.8
Proportion of spells ending in		
Employment	0.35	0.21
Labor market programs	0.13	0.13
Other destinations♦	0.22	0.42
Unknown destination	0.19	0.14
Censored after 60 weeks or due to end of study	0.11	0.10

Note: The sample consists of the individuals in the LINDA-database who began an unemployment period with income-related UI benefits during the period 2002-12-01 – 2003-12-31. ♦Other destinations include e.g. education and part time unemployment (without UI benefits).

From *Table 2* which presents descriptive covariate statistics, we can see that the unemployed in the comparison group, on average, are younger, less educated, and have more young children compared to the treated. The proportion of women is also higher in this group, as is the proportion of immigrants from non-OECD countries. There are also some differences in type of education between the two groups. The differences in observed characteristics are a natural consequence of having defined treatment status based on the pre-unemployment wage.

Table 2 Sample characteristics (means)

	Treatment group	Comparison group
Female	0.28	0.61
Age	38.8	35.0
Immigrant: OECD	0.05	0.04
Immigrant: other	0.06	0.13
Married	0.46	0.44
Presence of children<18	0.38	0.46
Length of education		
Upper secondary education	0.87	0.85
Post-secondary education	0.34	0.21
Missing	0.00	0.00
Type of education		
General	0.20	0.29
Pedagogic, teacher education	0.03	0.05
Humanities, arts	0.04	0.06
Social sciences, law, trade, admin.	0.17	0.16
Science, mathematics, computer science	0.04	0.02
Technical, manufacturing	0.38	0.17
Agriculture, forestry, veterinary	0.02	0.02
Health care, social work	0.04	0.12
Service	0.04	0.08
Missing/unknown	0.03	0.04
Pre-unemployment wage (month), SEK*	25 552	15 968
Number of individuals	2 696	8 408

Note: The sample consists of the individuals in the LINDA-database who began an unemployment period with income-related UI benefits during the period 2002-12-01 – 2003-12-31. * denotes average among spells, rather than among individuals.

Figure 4 below shows how the flow from UI to SI benefits evolves over time for the sample of unemployed workers. *Figure 5* shows the evolution of the share of the unemployed finding employment. The shares in both figures are calculated for time intervals of two weeks, and separately for the treatment and the comparison group. Here, we see a first indication of how the reform affected sickness absence and job finding among unemployed workers. The sick report rate is higher for the comparison group for most of the time period. Around the time of the reform it decreases for both groups, a pattern which is in line with a common finding in the Swedish literature on sickness insurance, namely that sick report rates tend to decline in the summer (see e.g. Larsson 2006, and Johansson and Palme 2005). After the summer, the flow to SI benefits returns almost to the pre-reform level for the comparisons group, while it remains on a somewhat lower level for the treated. This pattern is thus consistent with the reform having a negative effect on sickness absence among those unemployed who were affected by the reform, as is found by Larsson and Runeson (2007). Regarding the job finding rate on the other hand, there is no indication that transitions to employment increased for the treated relative to the comparisons after the reform. When interpreting these figures it is important to note that they do not adjust for any of the potentially important differences between the two groups. Perhaps most importantly, they do not account for the lengths of the unemployment spells.[29] Separating the reform effect from the effect of unemployment duration is a crucial part of the identification strategy, which we turn to next.

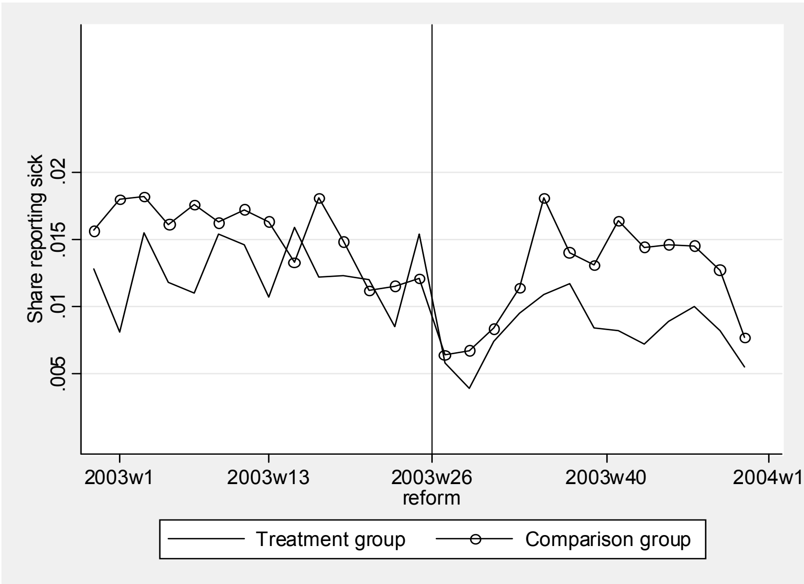

Figure 4 Share of the UI recipients reporting sick (per two-week interval), before and after the reform

Note: The shares are computed as (number of UI recipients reporting sick within an interval)/(average number of UI recipients each week in that interval). The shares are computed for the individuals in the LINDA-database who began an unemployment period with income-related UI benefits during the period 2002-12-01 – 2003-12-31.

[29] Not accounting for spell length means that the composition of the two groups with regard to unemployment duration will change over time in divergent ways, since the workers in the different groups leave unemployment at different rates. This may cause the difference between the hazard rate for the treated and the comparisons to change over time.

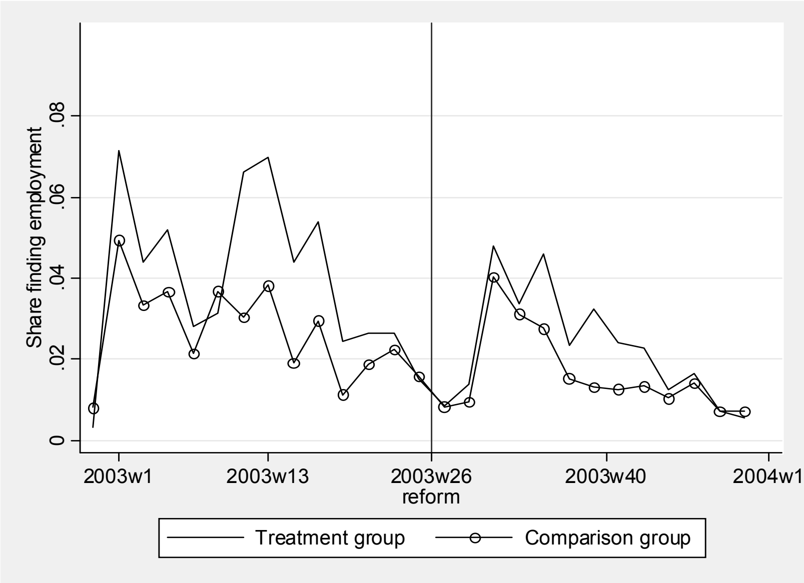

Figure 5 Share of the unemployed finding employment (per two-week interval), before and after the reform

Note: The shares are computed as (number of unemployed persons finding a job within an interval)/(average number of unemployed persons each week in that interval). The shares are computed for the individuals in the LINDA-database who began an unemployment period with income-related UI benefits during the period 2002-12-01 – 2003-12-31.

6 Empirical results

6.1 Transitions to sickness insurance

For the reform in July 2003 to have an effect on the job finding rate among unemployed workers, it must first of all have an effect on sickness absence. I therefore begin by examining how the reform affected the incidence of sick reports among the UI recipients, that is, the transition rate from UI to SI benefits. Hence, I replicate the results in Larsson and Runeson (2007), though with a somewhat different sample[30].[31, 32]

Table 3 presents results for Cox regression models as described in *Section 4*, but where the outcome variable is sick leave rather than employment. The table shows the

[30] The most important difference is that I use weekly, rather than daily, data.
[31] Since I allow individuals to return to UI benefits after a sickness period, I here follow Larsson and Runeson (2007) and exclude UI spells starting after July 1, 2003. This is to avoid changes in the sample composition that are caused by the reform. If the reform also affects the duration of the SI spells, it may affect the composition of UI recipients through its effect on the hazard rate from SI back to UI.
[32] In this analysis, I do not impose the restriction that there must be a three-week break in the UI benefit payments for a spell to end. Instead, a sickness period of *any* length or an interruption in the UI benefit payments which is longer than one calendar week defines the end of a UI period.

estimated reform effect for four different specifications, estimated with partial maximum likelihood[33]. A table with all parameter estimates can be found in the Appendix. Column (1) shows the results for a model that only includes a dummy for the reform date, a dummy for treatment status, and an interaction variable called the 'cap reform effect'. The latter captures the effect of the reduced SI benefit cap, and is thus the variable of interest. In column (2) I present results for a model to which I have added indicators for the month of entry into unemployment, the number of days left until the UI benefits expire, the local labor market, as well the following individual characteristics: sex, age, level of education (4 categories), type of education (10 categories), immigrant background, marital status, presence of children younger than 18 and pre-unemployment wage. The last two columns show results for two stratified models, where the month of entry into unemployment and the local labor market are used as stratification units.[34] As mentioned in *Section 4*, these models should be less sensitive to compositional changes in unobserved factors as the reform effect is identified solely by comparing individuals beginning their unemployment period during the same month (column 3), as well as in the same local labor market (column 4).

We first note that the coefficient for the reform date dummy is negative and significant in all four specifications, indicating that there was a general decrease in sickness absence around the time of the reform. This variable should partially be picking up the effect of the general 3 percent reduction in SI benefits but also the effect of other changes around July 1, 2003, such as seasonal variation in sickness absence. The estimate for the 'cap reform effect' is also statistically significant in all specifications and quite large; it suggests that the reduced SI benefit cap lowered the incidence of sick reports among the treated with 31-33 percent[35]. Moreover, this estimate is very stable across the various specifications. Hence, in line with Larsson and Runeson

[33] Ties are handled using the exact method in SAS, see DeLong *et al* (1994), and Kalbfleisch and Prentice (1980).
[34] These models are estimated with a stratified partial maximum likelihood estimator; see e.g. van den Berg (2001), section 6.
[35] The percentage effect is obtained by 100*(exp(β)-1), where β is the parameter of interest.

(2007), these results suggest that the reform had a strong negative effect on the transition rate to SI among the UI recipients.[36]

Table 3 Estimated effects on the transition rate to sickness insurance

	(1)	(2)	(3)	(4)
Post reform (*t*) (D_t^{reform})	-0.263***	-0.195*	-0.517**	-0.431*
	(0.086)	(0.107)	(0.255)	(0.257)
Previous wage>20,696 (D^T)	-0.139	0.121	0.119	0.128
	(0.091)	(0.106)	(0.107)	(0.108)
Cap reform effect (*t*) $(D_t^{reform} * D^T)$	-0.377**	-0.390**	-0.397**	-0.377**
	(0.159)	(0.159)	(0.159)	(0.164)
Month of entry into unemployment	No	Yes	-	-
No of days until UI-expiration, in the beginning of the spell (six categories)	No	Yes	Yes	Yes
All other covariates included	No	Yes	Yes	Yes
Stratification by month of entry	No	No	Yes	Yes
Stratification by local labor market	No	No	No	Yes
-2 Log likelihood	14,819	14,607	13,524	9,303
No of observations	12,748	12,746	12,746	12,746
No of strata	-	-	7	153

Note: Estimation using (stratified) partial maximum likelihood estimator. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (*t*) denotes time-varying variable. The following covariates are included in column (2)-(4): sex, age, age^2, immigrant background, marital status, presence of children younger than 18, level of education, type of education, ln(pre-unemployment wage), and indicators for the local labor market.

6.2 Transitions to employment

Let us now move on to analyze whether the reduced sick report rate among the treated also translated into a higher rate of job finding. The results for the Cox regression models are reported in *Table 4* . As in the previous section, I start by presenting results for a model that only includes a dummy for the reform date, a dummy for the treatment group and an interaction variable – the 'cap reform effect' – which captures the effect of the reduced SI benefit cap on the treated population (column 1). The point estimate for the cap reform effect is close to zero in this model and it is not statistically significant. Adding covariates to the model does not alter this finding; the estimate for the cap reform effect is close to zero and non-significant also in the second specification, which includes all covariates. A table with all parameter estimates can be found in the Appendix.

[36] The size of the effect is very similar to the one found in Larsson and Runeson (2007). Their preferred estimate for the 'cap reform effect' implies a 36 percent reduction of the incidence of sick reports. This effect is very robust across various sensitivity tests. They also study the effect on sickness duration, but find no such effects.

In the last two specifications, I have stratified on the month of inflow into unemployment (column 3), as well as on the local labor market (column 4). This means that the baseline hazard is allowed to differ across months of entry, and across local labor markets. The variation which identifies the reform effect in these specifications thus comes from when, within a given month, a person entered unemployment. These models should be less sensitive to seasonal (column 3), as well as regional (column 4), variations in labor market conditions. A further implication of this approach is that only unemployment spells that start before the reform are used to identify the reform effect (since there is no within-month variation in the time-varying reform variable for spells beginning after the reform). While the estimate for the cap reform effect does not change much as I stratify on the entry month, it becomes more negative when I also stratify on the local labor market. However, it is still very far from being significantly different from zero. In sum, I find no evidence suggesting that the reduced sick report rate in the treatment group affected the transition rate to employment.

Regarding the other variables, we note that the estimate for the post reform dummy is negative and significant in all four specifications. This result indicates a general decrease in transitions to employment around the time of the reform (as is suggested in *Figure 5*). We also note that the dummy for the treatment group is positive and significant, showing that the job finding rate in general is higher for the unemployed with previously high wages.

Table 4 Estimated effects on the transition rate to employment

	(1)	(2)	(3)	(4)
Post reform (*t*) (D_t^{reform})	-0.156***	-0.300***	-0.739***	-0.617***
	(0.046)	(0.073)	(0.233)	(0.237)
Previous wage>20,696 (D^T)	0.422***	0.209***	0.197***	0.235***
	(0.066)	(0.076)	(0.076)	(0.078)
Cap reform effect (*t*) $(D_t^{reform} * D^T)$	0.007	-0.007	0.000	-0.069
	(0.078)	(0.079)	(0.079)	(0.082)
No of days until UI-expiration, in the beginning of the spell (six categories)	No	Yes	Yes	Yes
Month of entry into unemployment	No	Yes	-	-
All other covariates included	No	Yes	Yes	Yes
Stratification by month of entry	No	No	Yes	Yes
Stratification by local labor market	No	No	No	Yes
-2 Log likelihood	39,382	38,670	35,964	25,669
No of observations	14,935	14,932	14,932	14,932
No of strata	-	-	11	239

Note: Estimation using (stratified) partial maximum likelihood estimator. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (*t*) denotes time-varying variable. The following covariates are included in column (2)-(4): sex, age, age^2, immigrant background, marital status, presence of children younger than 18, level of education, type of education, ln(pre-unemployment wage), and indicators for the local labor market.

Even in the absence of a significant average treatment effect, there may be an effect on the job finding rate for sub-groups of unemployed workers. Previous studies on sickness absence among the unemployed, find that the flow to SI increases as the UI expiration date approaches (see e.g. Larsson 2006). A similar pattern is visible in my results, see *Table A1* in the Appendix. Moreover, Larsson and Runeson (2007) find that the decline in sick reports due to the July 2003-reform was largest among those with relatively few UI days left. To check whether there perhaps is an effect on the job finding rate for those who are close to benefit expiration, I have re-estimated the model only including the sub-sample with less than 150 remaining UI days (in the beginning of their unemployment period). However, the estimate for the cap reform effect is insignificant also for this group.

6.3 Sensitivity analyses

The treatment and comparison group are indeed heterogeneous in several respects as treatment status is defined based on pre-unemployment earnings. One concern is whether the estimate of the cap reform effect is biased due to divergent trends in labor market opportunities for the two groups. For instance, if the labor market opportunities worsened for the high-wage relative to the low-wage unemployed during the time period studied, this may bias the estimate of the cap reform effect downwards. This could then explain why we do not observe any effect of the cap reform on the transition rate to employment. A similar problem may arise if the labor market opportunities worsened more for the high-wage than for the low-wage unemployed around July 2003, due to different seasonal patterns. I perform several sensitivity analyses in order to test the robustness of my results in these respects.

I start by examining employment-to-population rates for different educational groups during the relevant period (2002-2004), see *Figure 6*. Since the average level of education is higher among the treated, this figure should give an indication of how the labor market opportunities developed for the two groups during this time period. *Figure 6* gives no support for that these opportunities worsened for the treated relative to the controls; it rather suggests the reverse.

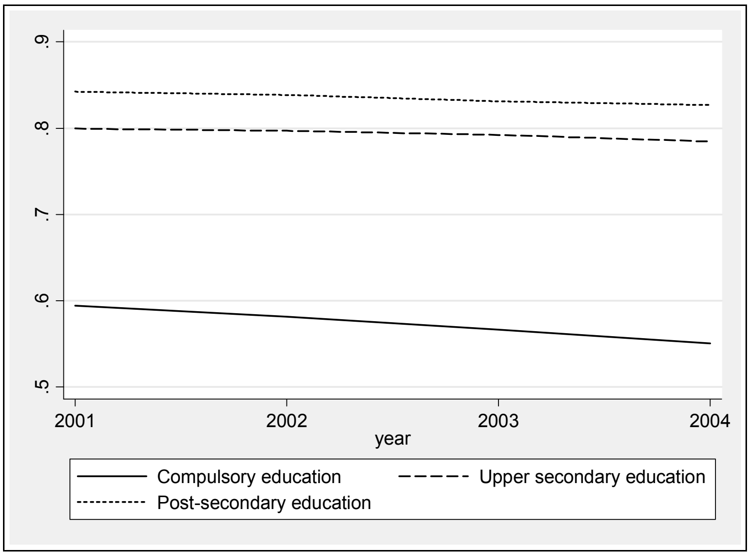

Figure 6 Employment-to-population rates for different educational groups, 2001-2004 (annual averages)

Note: Calculated for persons aged 16-64. Source: Labour Force Surveys, Statistics Sweden.

The employment-to-population rates are only available as annual averages. As discussed above, the result could also be biased due to divergent seasonal patterns in labor market opportunities for workers with different wages. In order to examine this possibility, I first re-estimate the model for different sub-samples which are more similar than the treated and comparisons in the baseline model, in terms of the pre-unemployment wage. I also test whether there could be different seasonal patterns for the two groups around the time of the reform, by analyzing the effects of a hypothetical reform supposed to have occurred on the same date the year after the actual reform.[37]

To reduce heterogeneity between the two groups, I successively exclude individuals with the 10 percent, 30 percent, 50 percent and 70 percent lowest pre-unemployment wages in the comparison group.[38] Column (2)-(5) in *Table 5* present the results from this exercise. For ease of comparison, the first column of the table reproduces my main results (shown in column 2, *Table 4*). The point estimate for the cap reform effect remains close to zero in all these regressions and the estimate is far from being statistically significant. Hence, limiting heterogeneity between the two groups in this way does not affect my findings.

Table 5 Effects of excluding workers with the lowest previous wages

	(1)	(2)	(3)	(4)	(5)
% of comparison group excluded	0%	10%	30%	50%	70%
Average previous wage (month)					
Comparison group	15,968	16,680	17,573	18,340	19,149
Treatment group	25 552	25 552	25 552	25 552	25 552
Post reform (*t*) (D_t^{reform})	-0.300***	-0.320***	-0.334***	-0.398***	-0.396***
	(0.073)	(0.075)	(0.082)	(0.092)	(0.109)
Previous wage>20,696 (D^T)	0.209***	0.224***	0.261***	0.265***	0.251***
	(0.076)	(0.080)	(0.083)	(0.085)	(0.091)
Cap reform effect (*t*) $(D_t^{ref} * D^T)$	-0.007	-0.003	-0.006	0.008	0.000
	(0.079)	(0.080)	(0.083)	(0.087)	(0.097)
All covariates included	Yes	Yes	Yes	Yes	Yes
-2 Log likelihood	38,670	36,727	32,145	27,186	21,353
No of observations	14,932	13,777	11,462	9,151	6,838

Note: Estimation with partial maximum likelihood. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (*t*) denotes time-varying variable. The following covariates are included: sex, age, age^2, immigrant background, marital status, presence of children younger than 18, level of education, type of education, ln(pre-unemployment wage), number of days until UI-expiration, indicators for the month of entry into unemployment and the local labor market.

[37] Due to lack of data on pre-unemployment wages for 2002, I cannot perform the same analysis for the year before the reform.

[38] Ideally, we may also want to exclude individuals with the highest previous wages among the treated. However, by doing this we would at the same time reduce the average amount of 'treatment' received in the treatment group. Recall that the percentage decrease in SI benefits varied among the treated depending on the pre-unemployment wage; the higher the wage, the larger the percentage reduction in benefits. The results from such an exercise would thus be difficult to interpret.

To study the effect of a hypothetical reform the year after the actual reform, I construct a new sample of unemployment spells, following the same sampling procedure but instead including spells beginning during the period 1 December 2003 – 30 December 2004. In case this imaginary reform gives rise to a significant estimate for the 'cap reform effect', this would indicate that the seasonal patterns may indeed differ for the two groups around this time of the year. If there is no significant 'effect' of the hypothetical reform either, divergent seasonal patterns in labor market opportunities seems less likely to be a problem. The results for this exercise are shown in *Table 6*, column (2). The first column of the table reproduces my main results for the actual reform (shown in column 2, *Table 4*). The estimate for the hypothetical cap reform is more negative than the estimate for the actual reform, however it is far from being significantly different from zero. Hence, there is no evidence of different seasonal patterns for the two groups around July 1 the year after the reform.

Table 6 Estimated effects of a hypothetical reform July 1, 2004

	(1) Actual reform (July 1, 2003)	(2) Hypothetical reform (July 1, 2004)
Post reform (*t*) (D_t^{reform})	-0.300***	-0.303***
	(0.073)	(0.074)
Previous wage>20,696 (D^T)	0.209***	0.346***
	(0.076)	(0.070)
Cap reform effect (*t*) $(D_t^{reform} * D^T)$	-0.007	-0.056
	(0.079)	(0.073)
No of days until UI-expiration (six cat.)	Yes	Yes
Month of entry into unemployment	Yes	Yes
All other covariates included	Yes	Yes
-2 Log likelihood	38,670	40,136
No of observations	14,932	16,160

Note: Estimation with partial maximum likelihood. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (*t*) denotes time-varying variable. The following covariates are included: sex, age, age^2, immigrant background, marital status, presence of children younger than 18, level of education, type of education, ln(pre-unemployment wage), and indicators for local labor market.

In addition to the sensitivity analyses presented above, I have checked whether the results are sensitive to how unemployment spells that end for 'unknown reasons' (which may well be employment) are treated. To do this, I have re-estimated the model treating these spell as ending in employment. I have also estimated a model that account for (persistent) unobserved individual heterogeneity, by using the fact that I have multiple unemployment spells for a part of the sample.[39] The findings are qualitatively the same in these models.

7 Concluding remarks

This paper studies the effects of a reform which substantially reduced the economic incentives for unemployed persons to transfer from the unemployment insurance (UI) to the sickness insurance (SI). While there is evidence that this reform effectively lowered the incidence of sick reports among the unemployed affected, I find no evidence suggesting that the reduced sick report rate in turn affected the transition rate to employment. Hence, for those who reduced their sick report rate due to the reform, spending more time in the UI rather than the SI did not seem to shorten the time out of employment.

Should we then conclude that the interplay between these two insurances does not have any economic significance, and that it does not matter whether there perhaps is excess use of the SI among the unemployed? Probably not. First of all, making sure that the insurance systems are used in the way intended is likely to be important per se. If the citizens have the perception that the benefits are misused this could undermine the legitimacy for the social insurance system. Second, we should note that the study in this paper is a partial equilibrium analysis, and that the results do not indicate how the reform affected total employment. Reduced SI benefits for the unemployed may affect employment through other channels than the one studied here, e.g. it may affect transitions to other benefit systems as they become relatively more attractive. Early retirement pension is one example.

Finally and perhaps most importantly, the fact that I do not find any effect on the job finding rate can have two different explanations, which in turn will have very different

[39] To check whether the results are affected by compositional changes in unobserved factors, I have estimated a model where I stratify on the individual. That is, the reform effect is identified using within individual variation. This method may not be ideal since it only uses a small sub-sample of the unemployed workers (only 24 percent of the individuals in the sample have repeated unemployment spells), as well as rests on the assumption that the unobserved individual characteristics are fixed across spells. However, the results are qualitatively the same. These results are not reported, but are available upon request from the author.

policy implications. First, it could be due to that, for those affected by the reform, search effort did not differ depending on receiving benefits from the UI or the SI (given their health status). This would then indicate that monitoring in at least one of the insurance systems is insufficient. If these unemployed persons did not search actively in *either* system, this would suggest insufficient monitoring in the UI, as active search is a formal requirement for receiving UI benefits. If they in fact searched actively in *both* systems, this would instead indicate insufficient monitoring in the SI, as the SI is intended for those who have lost their work (search) capacity due to sickness.

Second, it is possible that spending more time in the UI in fact *did* increase search effort, but that more active search still did not result in faster transitions to employment for this particular group. If this is the case, it would indicate that those who changed their sickness absence behavior due to the reform belong to a group with weak attachment to the labor market. An interesting topic for future research would be to use data on individual search behavior in order to discriminate between these two explanations.

References

Abbring, J., G. van den Berg and J.C. van Ours (2005), "The effect of unemployment insurance sanctions on the transition rate from unemployment to employment", *Economic Journal* 115, 602-630.

Bring, J. and K. Carling (2000), "Attrition and Misclassification of Drop-outs in the Analysis of Unemployment Duration", *Journal of Official Statistics*, vol 16, no 4, 321-330.

Brown, S. and J. Sessions (1996), "The Economics of Absence: Theory and Evidence", *Journal of Economic Surveys* 10, 23-53.

Carling, K., B. Holmlund and A. Vejsiu (2001), "Do Benefit Cuts Boost Job Finding? Swedish Evidence from the 1990s", *Economic Journal* 111, 766-790.

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

DeLong, D.M., G.H. Guirguis and Y.C. So (1994), "Efficient Computation of Subset Selection Probabilities with Application to Cox Regression", *Biometrika*, 81, 607-611.

Edin, P-A. and P. Fredriksson (2000), "LINDA – Longitudinal Individual Data for Sweden", Working Paper 2000:19, Department of Economics, Uppsala University.

Engström, P. and B. Holmlund (2007), "Worker Absenteeism in Search Equilibrium", *Scandinavian Journal of Economics*, vol 109, 439-467.

European Economic Advisory Group (2007), "Report on the European Economy 2007", Ifo Institute for Economic Research.

Fortin, B. and P. Lanoie (1992), "Substitution between Unemployment Insurance and Workers' Compensation", *Journal of Public Economics* 49, 287-312.

Henningsen, M. (2006), ”Moving between Welfare Payments. The Case of Sickness Insurance for the Unemployed”, Memorandum no 04/2006, Department of Economics, University of Oslo.

Holmlund, B. (2005), “Sickness Absence, Search Unemployment and Social Insurance”. Revised version of Working Paper 2004:6, Department of Economics, Uppsala University.

Johansson, P. and M. Palme (2005), “Moral hazard and sickness insurance”, *Journal of Public Economics* 89, 1879-1890.

Kalbfleisch, J.D. and R.L. Prentice (1980), *The Statistical Analysis of Failure Time Data*, New York: John Wiley & Sons, Inc.

Karlström, A., M. Palme and I. Svensson (2006), ”The Employment Effect of Stricter Rules for Eligibility for DI: Evidence from a Natural Experiment in Sweden”, mimeo, Stockholm University.

Katz, L. and B. Meyer (1990), “The Impact of the Potential Duration of Unemployment Benefits on the Duration of Unemployment”, *Journal of Public Economics* 41, 45-72.

Krueger, A. and B. Meyer (2002), “Labor Supply Effects of Social Insurance”, in A. Auerbach and M. Feldstein (ed.), *Handbook of Public Economics,* vol 4, North-Holland.

Lalive, R., J.C. van Ours and J. Zweimüller (2005), “The effect of benefit sanctions on the duration of unemployment”, *Journal of the European Economic Association*, vol 3(6), 1386-1417.

Larsson, L. (2004), “Harmonizing unemployment and sickness insurance: Why (not)?”, *Swedish Economic Policy Review*, vol 11, no 1, 151-188.

Larsson, L. (2006), “Sick of being unemployed? Interactions between unemployment and sickness insurance”, *Scandinavian Journal of Economics*, vol 108, 97-113.

Larsson, L. and C. Runeson (2007), “Moral hazard among the sick and unemployed: Evidence from a Swedish social insurance reform”, Working Paper 2007:8, IFAU.

Meyer, B. (1990), “Unemployment Insurance and Unemployment Spells”, *Econometrica* 58, 757-782.

Moffitt, R. (1985), “Unemployment Insurance and the Distribution of Unemployment Spells”, *Journal of Econometrics* 28, 85-101.

OECD (2006), *Economic Survey of Finland*, vol 2006/5, Paris.

Pellizzari, M. (2006), “Unemployment duration and the interactions between unemployment insurance and social assistance”, *Labor Economics* 13, 773-798

SFU Socialförsäkringsutredningen (2007), ”Arbetslösa som blir sjuka och sjuka som inte blir arbetslösa”, Samtal om socialförsäkring nr 16.

Van den Berg, G. (2001), ”Duration Models: Specification, Identification, and Multiple Durations”, in Heckman J. and E. Leamer (ed.) *Handbook of Econometrics*, vol 5, North Holland, Amsterdam.

Appendix

Table A1 Estimated effects on the transition rate to sickness insurance

	(1)	(2)	(3)	(4)
Post reform (*t*) (D_t^{reform})	-0.263***	-0.195*	-0.517**	-0.431*
	(0.086)	(0.107)	(0.255)	(0.257)
Previous wage>20,696 (D^T)	-0.139	0.121	0.119	0.128
	(0.091)	(0.106)	(0.107)	(0.108)
Cap reform effect (*t*) $(D_t^{reform} * D^T)$	-0.377**	-0.390**	-0.397**	-0.377**
	(0.159)	(0.159)	(0.159)	(0.164)
Days until UI benefit expiration♣				
(*Ref*: 50-1 days until UI-expiration)				
300-251 days until UI-exp.		-0.288***	-0.280***	-0.250***
		(0.092)	(0.092)	(0.094)
250-201 days until UI-exp.		-0.251**	-0.243**	-0.203*
		(0.111)	(0.111)	(0.113)
200-151 days until UI-exp.		-0.075	-0.062	-0.017
		(0.114)	(0.114)	(0.117)
150-101 days until UI-exp.		0.038	0.049	0.096
		(0.115)	(0.115)	(0.118)
100-51 days until UI-exp.		0.102	0.110	0.127
		(0.117)	(0.117)	(0.119)
Female		0.202***	0.213***	0.224***
		(0.070)	(0.070)	(0.072)
Age		0.077***	0.078***	0.075***
		(0.024)	(0.024)	(0.024)
Age^2		-0.001**	-0.001**	-0.001**
		(0.000)	(0.000)	(0.000)
Immigrant: OECD		-0.031	-0.042	-0.053
		(0.128)	(0.128)	(0.131)
Immigrant: other		0.035	0.039	0.064
		(0.088)	(0.088)	(0.089)
Married		-0.119*	-0.118*	-0.127*
		(0.068)	(0.068)	(0.069)
Presence of children<18		0.209***	0.209***	0.201***
		(0.074)	(0.074)	(0.075)
Level of education:				
(*Ref*: compulsory school)				
Upper secondary education		-0.176	-0.172	-0.174
		(0.110)	(0.110)	(0.112)
Post-secondary education		-0.273***	-0.264***	-0.248***
		(0.094)	(0.094)	(0.096)
Missing		-0.816	-0.824	-0.692
		(1.020)	(1.021)	(1.030)
Type of education (10 categories)	No	Yes	Yes	Yes
ln(pre-unemployment wage)		-0.295***	-0.298***	-0.311***
		(0.106)	(0.107)	(0.107)
Month of entry into unemployment	No	Yes	-	-
Dummies for local labor market (county)	No	Yes	Yes	-
Stratification by month of entry	No	No	Yes	Yes
Stratification by local labor market	No	No	No	Yes
-2 Log likelihood	14,819	14,607	13,524	9,303
No of observations	12,748	12,746	12,746	12,746
No of strata	-	-	7	153

Note: Estimation using (stratified) partial maximum likelihood estimator. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (t) denotes time-varying variable.♣Measured in the beginning of the unemployment spell.

Table A2 Estimated effects on the transition rate to employment

	(1)	(2)	(3)	(4)
Post reform (*t*) (D_t^{reform})	-0.156***	-0.300***	-0.739***	-0.617***
	(0.046)	(0.073)	(0.233)	(0.237)
Previous wage>20,696 (D^T)	0.422***	0.209***	0.197***	0.235***
	(0.066)	(0.076)	(0.076)	(0.078)
Cap reform effect (*t*) $(D_t^{reform} * D^T)$	0.007	-0.007	0.000	-0.069
	(0.078)	(0.079)	(0.079)	(0.082)
Days until UI benefit expiration♣				
(*Ref*: 50-1 days until UI-expiration)				
300-251 days until UI-exp.		0.218***	0.219***	0.209***
		(0.067)	(0.067)	(0.068)
250-201 days until UI-exp.		0.147*	0.147*	0.146*
		(0.076)	(0.076)	(0.077)
200-151 days until UI-exp.		0.169**	0.158**	0.145*
		(0.080)	(0.080)	(0.081)
150-101 days until UI-exp.		0.114	0.106	0.088
		(0.084)	(0.084)	(0.085)
100-51 days until UI-exp.		0.002	-0.007	-0.003
		(0.090)	(0.090)	(0.091)
Female		-0.269***	-0.261***	-0.261***
		(0.041)	(0.041)	(0.042)
Age		0.048***	0.048***	0.049***
		(0.013)	(0.013)	(0.013)
Age^2		-0.001***	-0.001***	-0.001***
		(0.000)	(0.000)	(0.000)
Immigrant: OECD		-0.176**	-0.186**	-0.175**
		(0.088)	(0.088)	(0.089)
Immigrant: other		-0.620***	-0.621***	-0.612***
		(0.067)	(0.067)	(0.068)
Married		0.097**	0.095**	0.103**
		(0.047)	(0.047)	(0.048)
Presence of children<18		-0.122**	-0.118**	-0.130***
		(0.047)	(0.048)	(0.048)
Level of education:				
(*Ref*: compulsory school)				
Upper secondary education		-0.201***	-0.195***	-0.182***
		(0.069)	(0.069)	(0.070)
Post-secondary education		0.060	0.062	0.057
		(0.049)	(0.049)	(0.050)
Missing		0.422	0.418	0.453
		(0.398)	(0.398)	(0.407)
Type of education (10 categories)	No	Yes	Yes	Yes
ln(pre-unemployment wage)		0.125	0.125	0.131*
		(0.079)	(0.079)	(0.079)
Month of entry into unemployment	No	Yes	-	-
Dummies for local labor market (county)	No	Yes	Yes	-
Stratification by month of entry	No	No	Yes	Yes
Stratification by local labor market	No	No	No	Yes
-2 Log likelihood	39,382	38,670	35,964	25,669
No of observations	14,935	14,932	14,932	14,932
No of strata	-	-	11	239

Note: Estimation using (stratified) partial maximum likelihood estimator. Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (*t*) denotes time-varying variable.♣Measured in the beginning of the unemployment spell.

Publication series published by the Institute for Labour Market Policy Evaluation (IFAU) – latest issues

Rapporter/Reports

2008:1 de Luna Xavier, Anders Forslund and Linus Liljeberg ”Effekter av yrkesinriktad arbetsmarknadsutbildning för deltagare under perioden 2002–04”

2008:2 Johansson Per and Sophie Langenskiöld ”Ett alternativt program för äldre långtids-arbetslösa – utvärdering av Arbetstorget för erfarna”

2008:3 Hallberg Daniel ”Hur påverkar konjunktursvängningar förtida tjänstepensionering?”

2008:4 Dahlberg Matz and Eva Mörk ”Valår och den kommunala politiken”

2008:5 Engström Per, Patrik Hesselius, Bertil Holmlund and Patric Tirmén ”Hur fungerar arbetsförmedlingens anvisningar av lediga platser?”

2008:6 Nilsson J Peter “De långsiktiga konsekvenserna av alkoholkonsumtion under graviditeten”

2008:7 Alexius Annika and Bertil Holmlund ”Penningpolitiken och den svenska arbetslösheten”

2008:8 Anderzén Ingrid, Ingrid Demmelmaier, Ann-Sophie Hansson, Per Johansson, Erica Lindahl and Ulrika Winblad ”Samverkan i Resursteam: effekter på organisation, hälsa och sjukskrivning”

2008:9 Lundin Daniela and Linus Liljeberg ”Arbetsförmedlingens arbete med nystartsjobben”

2008:10 Hytti Helka and Laura Hartman ”Integration vs kompensation – välfärdsstrategier kring arbetsoförmåga i Sverige och Finland”

2008:11 Hesselius Patrik, Per Johansson and Johan Vikström ”Påverkas individen av omgivningens sjukfrånvaro?”

2008:12 Fredriksson Peter and Martin Söderström ”Vilken effekt har arbetslöshetsersättningen på regional arbetslöshet?”

2008:13 Lundin Martin ”Kommunerna och arbetsmarknadspolitiken”

2008:14 Dahlberg Matz, Heléne Lundqvist and Eva Mörk ”Hur fördelas ökade generella statsbidrag mellan personal i olika kommunala sektorer?”

2008:15 Hall Caroline ”Påverkades arbetslöshetstiden av sänkningen av de arbetslösas sjukpenning?”

Working papers

2008:1 Albrecht James, Gerard van den Berg and Susan Vroman “The aggregate labor market effects of the Swedish knowledge lift programme”

2008:2 Hallberg Daniel “Economic fluctuations and retirement of older employees”

2008:3 Dahlberg Matz and Eva Mörk “Is there an election cycle in public employment? Separating time effects from election year effects”

2008:4 Nilsson J Peter ”Does a pint a day affect your child’s pay? The effect of prenatal alcohol exposure on adult outcomes”

2008:5 Alexius Annika and Bertil Holmlund “Monetary policy and Swedish unemployment fluctuations”

2008:6 Costa Dias Monica, Hidehiko Ichimura and Gerard van den Berg ”The matching method for treatment evaluation with selective participation and ineligibles”

2008:7 Richardson Katarina and Gerard J. van den Berg “Duration dependence versus unobserved heterogeneity in treatment effects: Swedish labor market training and the transition rate to employment”

2008:8 Hesselius Patrik, Per Johansson and Johan Vikström “Monitoring and norms in sickness insurance: empirical evidence from a natural experiment”

2008:9 Verho Jouko, “Scars of recession: the long-term costs of the Finnish economic crisis”

2008:10 Andersen Torben M. and Lars Haagen Pedersen “Distribution and labour market incentives in the welfare state – Danish experiences”

2008:11 Waldfogel Jane “Welfare reforms and child well-being in the US and UK”

2008:12 Brewer Mike “Welfare reform in the UK: 1997–2007”

2008:13 Moffitt Robert “Welfare reform: the US experience”

2008:14 Meyer Bruce D. “The US earned income tax credit, its effects, and possible reforms”

2008:15 Fredriksson Peter and Martin Söderström “Do unemployment benefits increase unemployment? New evidence on an old question”

2008:16 van den Berg Gerard J., Gabriele Doblhammer-Reiter and Kaare Christensen ”Being born under adverse economic conditions leads to a higher cardiovascular mortality rate later in life – evidence based on individuals born at different stages of the business cycle”

2008:17 Dahlberg Matz, Heléne Lundqvist and Eva Mörk “Intergovernmental grants and bureaucratic power”

2008:18 Hall Caroline “Do interactions between unemployment insurance and sickness insurance affect transitions to employment?”

Dissertation series

2007:1 Lundin Martin “The conditions for multi-level governance: implementation, politics and cooperation in Swedish active labor market policy”

2007:2 Edmark Karin “Interactions among Swedish local governments”

2008:1 Andersson Christian “Teachers and Student outcomes: evidence using Swedish data”