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Moral hazard among the sick and unemployed:
evidence from a Swedish social insurance reform

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Moral hazard among the sick and unemployed: evidence from a Swedish social insurance reform♥

by

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Abstract

This paper looks at a specific type of moral hazard that arises in the interplay between two large public insurance systems in Sweden, namely the sickness insurance (SI) and the unemployment insurance (UI). Moral hazard can arise from the benefit size structure as for some unemployed persons, benefits from the SI are higher than benefits from the UI. We use a reform of the SI system that came in force 1 July, 2003, to identify the effect of economic incentives arising from the different benefit sizes. Our results from a duration analysis show clearly that the higher the benefits, the larger the probability of reporting sick.

Keywords: Unemployment insurance, sickness insurance, health, duration analysis, discrete hazard models.
JEL-Code: C41, J64, J65, H55, I18

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1 Introduction

Moral hazard is a common problem associated with insurance which arises when the behavior of an insured person is affected by how the insurance is constructed. The empirical literature on moral hazard within insurance systems is extensive. However, most studies consider one insurance system at a time. Moral hazard that arises in the interplay between various social insurance programs is a largely unexplored research area, as pointed out by Krueger and Meyer (2002) in their *Handbook of Public Economics* chapter on labour supply effects of social insurance.

This paper looks at a specific type of moral hazard that arises in the interplay between two large public insurance systems in Sweden, namely the sickness insurance (SI) and the unemployment insurance (UI). More specifically, we address the question of whether differences in benefit generosity affect the use of SI benefits among unemployed persons. In Sweden it is possible for the unemployed to report sick and receive SI benefits (even for a short period). This rule is based on the idea that job search is comparable to work. In order to be eligible for UI benefits, an unemployed person should actively search for jobs and be able to accept a job offer at short notice. Unemployed persons who loose their work (search) capacity due to sickness should therefore receive benefits from the SI rather than the UI.

There are at least two sources of moral hazard in this context. First, UI benefits are limited to 300 work days, whereas SI benefits in principle can be received forever. By reporting sick an unemployed person can postpone the UI expiration date. A previous study from Sweden (Larsson, 2006) shows that the probability of reporting sick among the unemployed increases drastically as the UI expiration date approaches. Henningsen (2006) finds the same pattern in Norway, where the institutional setting is similar to Sweden. However, whether these results are due to economic incentives or to actual health deterioration caused by stress remains to be explored.[1]

Second, moral hazard can arise from the benefit size structure. For some unemployed persons, benefits from the SI are higher than benefits from the UI.

[1] Several empirical studies indicate that exit rates from unemployment to employment increase as workers approach the benefit expiration date. Evidence from the United States is reported by Moffitt (1985), Meyer (1990), and Katz and Meyer (1990). Swedish evidence is found in Carling *et al* (1996).

Both benefits are determined by the worker's pre-unemployment wage, the replacement rate being approximately the same, whereas the cap – i.e. the maximum amount – for most periods has been higher in the SI than in the UI system. Thus, the high-wage unemployed workers have been able to receive higher benefits from the SI than from the UI. In the early 2000s, SI benefits could be up to 20 percent higher than maximum UI benefits. For unemployed persons who have received UI benefits for 100 days, the UI benefit cap drops by approximately 7 percent, implying that the SI benefits could be nearly 30 percent higher for such 'long-term' unemployed persons. Larsson (2006) looks into this potential source of moral hazard as well and finds that the difference in benefits seems to increase the probability of reporting sick.

In this study, we use a reform of the SI system that came into force on 1 July, 2003 to identify the effect of economic incentives arising from the different benefit sizes. Basically, the purpose of the reform was to eliminate the difference in benefits by lowering the SI benefit cap to the same level as the UI benefit cap during the first 100 days of unemployment. We would expect sickness absence to decrease due to the reform as the benefits from the SI no longer exceed the benefits from the UI.

We identify the effect of economic incentives using the fact that the reform affected various groups of unemployed persons differently and at different durations of unemployment. First, the reduction of the SI benefit cap affected only those who had a previous wage above the new, lower cap. Persons with a lower previous wage can be used as a comparison group. Second, as workers become unemployed at different dates, the reform affected them at different durations of unemployment. This enables us to separate the reform effect any effects that occur at a specific point of time in unemployment. Finally, our data contains repeated unemployment spells, allowing us to test for unobserved individual heterogeneity.

Our results suggest strong negative effects on the incidence of sickness absence. Due to the lowered benefit cap, the incidence of sick reports was reduced by about 36 percent more among the treated compared to the comparison group. As the average drop in benefits in our sample was roughly 9 percent, we estimate an elasticity of sick reports with respect to sickness benefits of about 3.9. The result is very robust across various specifications.

The remainder of this paper is organised as follows: section 2 presents the central features of Sweden's UI and SI systems; section 3 discusses identification issues; section 4 presents the data; section 5 shows the empirical

results; section 6 discusses the economic significance of the reform and concludes.

2 Unemployment and sickness insurance in Sweden[2]

SI and UI form an integral part of the compulsory public social insurance in Sweden. Benefits from the public social insurance are income-related and for the most part financed by taxes. The system, being a part of the Swedish welfare state, can be characterized as general rather than selective. That is, most citizens are comprised by the system, and the degree of means testing is low. Moreover, the Swedish system is often perceived as generous with high replacement rates by international standards.

2.1 Description of the unemployment insurance

The unemployment insurance system provides income-related compensation for a maximum period of 60 weeks. During 2003 the replacement ratio was 80 percent up to a cap, approximately equal to the mean wage of a Swedish worker. For income-related benefits, the unemployed person has to fulfill three conditions[3]:

- *The basic condition* that the unemployed person is available for vacant jobs. In practice this means that he has to be registered at the public employment office as a job seeker and that he is willing to accept a job.
- *The membership condition* that the unemployed person has been a member of a UI fund for at least twelve months prior to unemployment. Membership is voluntary.
- *The working condition* that the unemployed has worked at least six months during the last twelve months preceding unemployment.

If the unemployed person has been a member of a UI fund for a shorter period than a year but still fulfills the other two conditions, he is entitled to the fixed lower compensation.

The UI is administered by 36 unemployment insurance funds representing workers from different occupational groups. All together, the UI funds have

[2] This chapter describes the systems as they were in 2003.
[3] For a detailed description, see e.g. www.aea.se.

approximately 3.8 million members, corresponding to 85 percent of the work force and 65 percent of the adult population. The funds are formally independent, but they must be officially approved by the state and follow common regulations in order to receive a grant from the state. Until lately, the main source of finance for the UI benefits has been the state grant, the remaining part being financed by membership fees.

The UI funds work closely with the local public employment offices, especially in controlling whether the unemployed person fulfills the rules concerning job search. The unemployed person has to meet his employment officer regularly and he is obliged to apply for any job the officer assigns him. If not, the employment officer must write a report to the UI fund, which then decides on a suitable sanction. In short, either the unemployed person is suspended from the UI, or his benefits are reduced. These sanctions are time-limited or permanent, depending on if the person has violated the rules before, and the expected duration of the employment he refuses to accept.

UI benefits are time-limited to 300 workdays, corresponding to 60 weeks. These benefit days can be received either continuously or with breaks in the unemployment period. If working long enough – basically at least six months – during a break, a person can qualify for a new period of 300 days.

The UI benefit amounts were changed by the new Government from January 2007. The description below concerns the rules during 2003. The income-related UI benefits were 80 percent of the worker's average earnings during the last six months of work, with a lower and an upper limit. Figure 1 illustrates. The fixed basic amount of SEK 7,040 (≈ € 750)[4] per month constituted the minimum, corresponding to 80 percent of a monthly wage of SEK 8,800. The upper limit varied depending on how long the person had been unemployed. During the first 100 days of unemployment, the maximum benefits were 80 percent of a monthly wage of SEK 20,075. After the first 100 days, the cap was reduced to 80 percent of SEK 18,700.[5]

The first five days of involuntary unemployment are uncompensated. If the unemployment is voluntary – i.e. if the person has left his job without a valid reason or if he has been laid off because of improper behavior – the uncompensated period is up to 45 benefit days.

[4] Exchange rate April, 2006.
[5] These amounts were constant between July 1, 2002 and December 31, 2006.

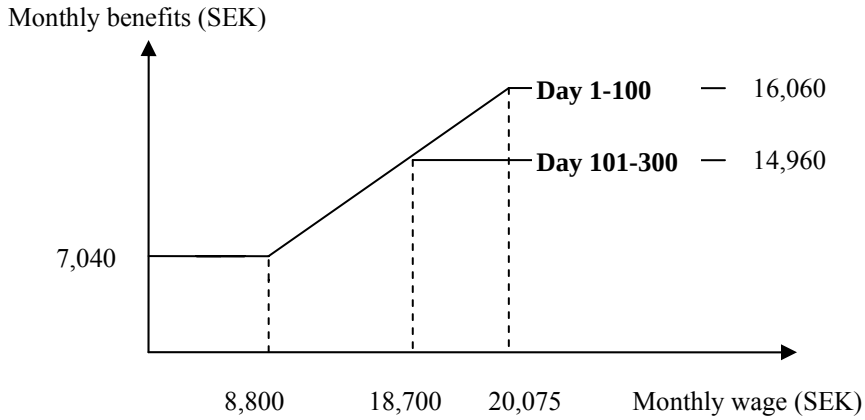

Figure 1 UI benefits in 2003

2.2 Description of the sickness insurance

The purpose of the SI is to provide economic maintenance when the worker is too sick to work and support himself. Benefits are income-related and there is no formal time-limit. In recent years, the replacement ratio has been around 80 percent. Just like the UI benefits system, the SI system contains a benefit cap.

All employed workers are automatically covered by the SI. Students and unemployed workers are also eligible for SI benefits as long as they fulfill certain conditions. An unemployed person must be registered at a local employment office as a job seeker. The size of a person's SI benefits is not based on his UI benefits but on his wage before unemployment. Thus, unemployed persons without any employment history do not receive SI benefits.

The SI is administered by the Swedish Social Insurance Agency and financed by payroll taxes. The first day of sickness is uncompensated. Employers are responsible for the employees' sickness compensation during the following 13 days of sickness, a period which was extended to 20 days between July 1, 2003 and December 31, 2004; after that the Social Insurance Agency takes over. For unemployed persons, the Social Insurance Agency is responsible for the sick pay from day two.[6]

[6] This asymmetry in rules has important implications for data and thus for our study. The data from the Swedish Social Insurance Agency includes all sick spells for unemployed persons,

The SI system contains some control instruments to prevent unjustified use of the insurance. After reporting sick by contacting either his employer (employed workers) or the Social Insurance Agency (non-employed), the person must visit a doctor within seven days of sickness in order to receive additional compensation after the first week. Again after four weeks, a doctor's certificate must be provided to the SI authorities.

A reform in 1 July 2003 changed the marginal replacement rate in two ways, the effect being different for employed and unemployed workers. Figure 2 illustrates the case for an unemployed worker. The size of the SI benefits depends on the person's wage prior to the sick period. For unemployed workers, it is based on the wage prior to unemployment. Before the reform, the replacement rate was 80 percent of the previous (pre-unemployment) wage. The minimum wage for receiving any SI benefits was SEK 775 per month, and the maximum SEK 24,125 per month. In other words, SI benefits varied between SEK 620 and SEK 19,300 per month.[7] The reform implied two changes: First, it reduced the marginal replacement rate to 77.6 percent. This 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 corresponds to the maximum monthly UI benefits.

whereas sick-spells shorter than or equal to the two (or three) weeks during which the employer is responsible are not included. Thus, we cannot use employed workers as a comparison group.

[7] Not accounting for the first uncompensated day.

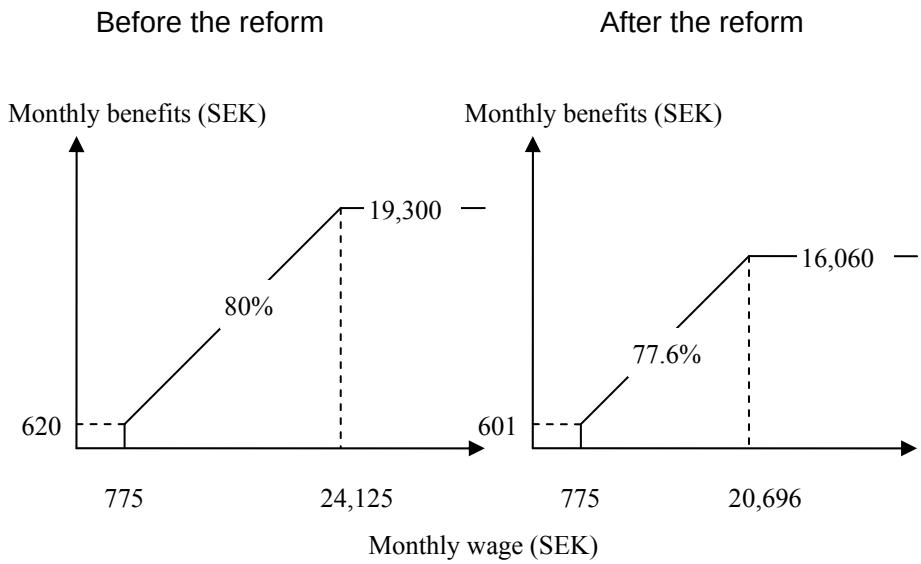

Figure 2 SI benefits for unemployed workers, before and after the reform in July, 2003.

3 Identification strategy

The fundamental research question of interest is how the size of economic compensation affects sickness absence. The reform that reduced the SI benefit cap serves as an ideal tool for identification. First, it divides the unemployed population into *treated* and *non-treated* as it only affected persons with previous wages above the UI benefit cap. Second, as workers become unemployed at different dates, the reform affected them at different durations of unemployment. Thus, the reform effect can be separated from any effects that occur at a specific point of time in unemployment. Finally, our data contains repeated unemployment spells, allowing us to test for unobserved individual heterogeneity.

Let us look more closely into how the reform affected the difference between SI and UI benefits for various types of unemployed persons.[8] Recall that the difference depended on i) the previous, pre-unemployment wage, and ii) whether the unemployed person had received UI benefits for less or more than 100 days. Figure 3 illustrates the case of an unemployed person who has not passed the 100-day limit, i.e. before the UI benefit cap drops.

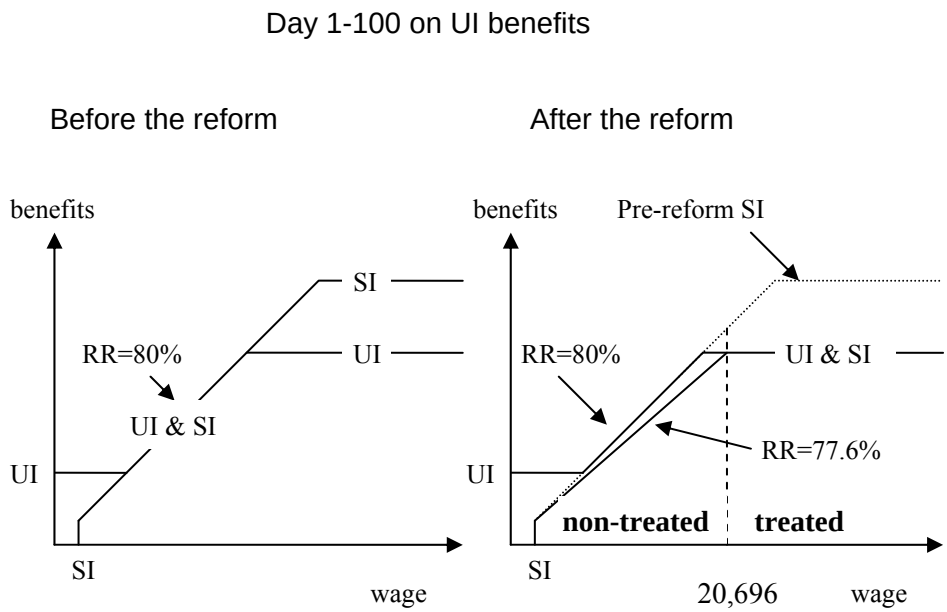

Figure 3 The change in relative benefit size due to the reform, during the first 100 UI benefit days

The reform changed the SI benefits for everybody, as the marginal replacement rate was reduced from 80 to 77.6 percent. Thus, the relative SI benefits (as compared with UI benefits) were reduced for all unemployed persons. However, up to the previous wage of SEK 20,696 the change was relatively small and, more importantly, equal to all.[9] These are the non-treated or com-

[8] Lack of data on short sick spells for employed persons prevents us from using the employed as an additional comparison group.

[9] The reform reduced the SI benefits with 3 percent for all unemployed persons with a previous wage up to SEK 20,696. Persons with very low previous earnings are an exception, as the reform

parison persons. For unemployed persons with a previous wage above that level, the treated, the reform implied a reduction of SI benefits that varied from 3 to almost 17 percent.

Figure 4 illustrates the case for an unemployed person who has passed the first 100 UI benefit days. The pattern is somewhat different as the UI benefit cap is now lower, implying that even after the reform, benefits from the SI are higher than benefits from the UI for high-wage unemployed persons. But the effect of the reform on the benefit *difference* 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 dividing line being at SEK 20,696.

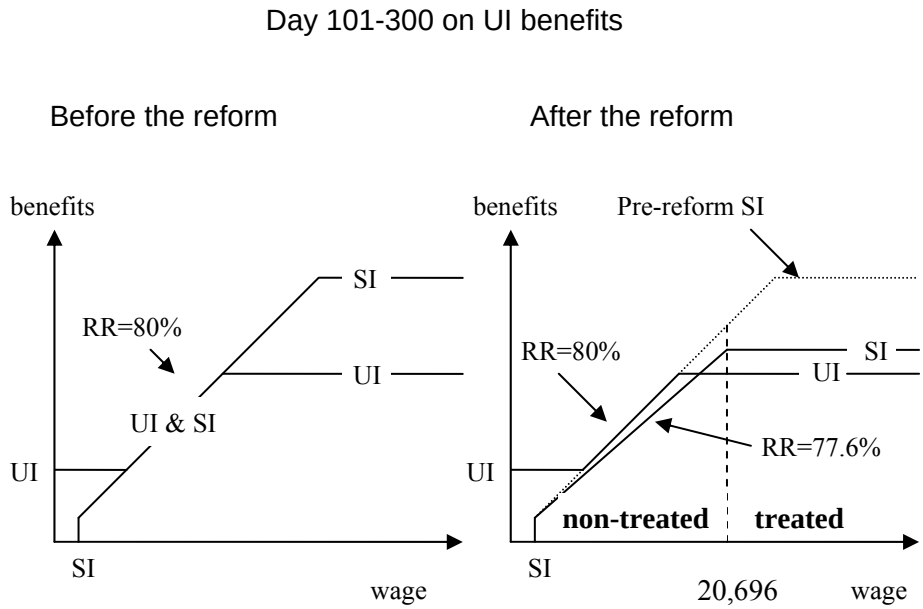

Figure 4 The change in relative benefit size due to the reform, after the first 100 UI benefit days

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also implied a marginal reduction of the minimum wage for SI eligibility; from SEK 620 to SEK 601. Hence, persons in this income group got eligible for SI benefits and thus experienced a benefit increase. However, in our data there are no observations in this income interval.

We will analyze the behavioral response to the change in compensation size in terms of the conditional incidence of sickness absence. That is, the incidence of sickness absence at time *t*, conditional on remaining unemployed up until time point *t*.[10] Our identification strategy exploits two features of the policy change. The first feature is the fact that the population can be divided into treated and non-treated and the second relates to the timing of the reform.

The timing feature arises when we use duration data and have a fixed reform date. As workers become unemployed at different dates, the reform affects them at different durations of unemployment. We use this variation to separate the reform effect from any effects that occur at a specific point of time in unemployment. This is done by comparing the evolution of hazard rates into sickness for people who experience the reform at different stages of their unemployment period. For example, the unemployed who experience the reform 30 days into their unemployment spell are compared with those who did not experience the reform during their first 30 days of unemployment.

This strategy enables us to identify the effect of the reform *date*. It is possible that other changes in the environment occurred around the time of the reform affecting transitions out of unemployment. In order to separate the effect of the compensation size from such factors, we compare the reform-date effect for the treated and the non-treated. If the effect is larger for the treated, who experienced a larger cut in the replacement rate, we have evidence of responsiveness to economic incentives. Hence, the policy change we use to identify the behavioral response to the compensation size is not the entire reduction in SI benefits due to the reform in July 2003, 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 identified as long as we believe that other changes in the environment occurred around the time of the reform.

To estimate the effect of the policy change, we use a discrete time Cox regression model. The advantage of imposing this semi-parametric structure instead of estimating fully non-parametric hazard rates is that we can control for some potentially important confounders, such as the time of inflow into unemployment. The baseline specification to be estimated can be written as:

[10] In what follows, we will simply refer to this as 'incidence'.

$$(1) \qquad \lambda(t) = \lambda_0(t)\exp\{f(x, z(t), \Omega) + \delta D_t^{July03} + \gamma T + \beta D_t^{July03} T\},$$

where λ_0 is the base-line hazard rate, i.e. the pre-reform hazard into sickness (for an individual with the value zero on all covariates). $f(.)$ is a function of the time-invariant covariates x and the time-varying covariates $z(t)$. D_t^{July03} is a time-varying dummy variable, where $D_t^{July03} = 0$ prior to July 2003 and $D_t^{July03} = 1$ thereafter. T is a dummy for the treatment group, were T=0 if the previous wage is below SEK 20,696 and T=1 for wages above that. The effect of the reduction in the SI benefit cap is obtained by comparing the difference in hazard rates for the treatment and the comparison groups before and after the 1st of July 2003. The effect of the policy change is given by the coefficient of the interaction variable, β.

The underlying assumption behind this 'difference-in-difference' approach is that everything else that changed around the time of the reform affected the treated and the comparisons similarly. It is thus important to check whether our estimates are affected by compositional changes in unobserved factors. For example, seasonal patterns may differ over the length of the unemployment spell. Moreover, the effects may differ across local labor markets or across individuals due to some unobserved heterogeneity. To check the robustness of our results to such heterogeneity, we estimate stratified models where the week of inflow into unemployment and the local labor market are used as stratification units. Access to data on repeated unemployment spells also allows us to stratify on the individual to control for unobserved individual heterogeneity.

4 Data

We combine data from a few different sources for the empirical analysis. The database *ASTAT*, originating from the unemployment insurance funds and the *Sickness Benefit Register* (*SFR*) from the Social Insurance Agency constitute the two main sources. These two datasets are a part of *LINDA*, which is a register-based longitudinal database that includes about 3 percent of the Swedish population.[11] LINDA additionally contains several demographic variables collected from e.g. tax registers.

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

ASTAT contains information on benefit payments for all unemployed persons who have been entitled to either basic-amount or income-related UI 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. Each week ASTAT registers the number of benefit days received, together with information on benefit amounts and the number of days left until a person's UI benefits expire. For unemployed with income-related benefits the database also includes information on the previous wage.

SFR contains information on SI benefit payments for all people who have been sick and entitled to such benefits, hence both employed and unemployed persons. For employed workers, however, sick spells shorter than or equal to the employers' responsibility period are not included in the data. For each sick spell, SFR records the start and end date, the income on which the benefits are based, and if benefits were given on a full or part-time basis. The SI benefits 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, covering about 83 percent[12] of the sick spells starting in 2003.

Using ASTAT as the data source for unemployment spells means that the condition for being defined as unemployed is to receive funding from the UI. This implies that participants in labor market programs and people who are registered at the public employment office as unemployed but who are not qualified for UI benefits[13] are not included in our sample. The main reason for excluding these groups is that we neither have information on their benefits (if any) nor on their previous wage, which we need in order to know their SI compensation in case of sickness.[14] Since the previous wage is unknown also for unemployed who are only entitled to the basic-amount of UI benefits, we also exclude this group.

[12] About 89 percent if we also count those periods where regular benefits for illness were given together with one of the other benefit types.
[13] That is, people who have not fulfilled the *working condition* (see section 2.1).
[14] Data on participants in labor market programs can be obtained from the database *Händel*, which is also included in LINDA. However, since Händel neither includes information on benefits nor on previous wages, this data is not very helpful for our purposes.

4.1 Sampling and descriptive statistics

We construct our sample by selecting all individuals who began an unemployment period with income-related UI benefits during the period 1 December 2002 – 30 June 2003 (i.e. up until the reform date). Unemployment spells beginning after the reform are left out in order to avoid changes in the sample composition caused by the reform.[15] The rationale for not sampling before December 2002 is that the wage information is incomplete before this point in time[16]. An unemployment period is considered to begin when a person who have not received UI benefits during the last 7 days, starts to receive benefits.

Each unemployment spell beginning during the sampling period is followed until it ends, or at most, until the end of 2003. A transition to SI benefits or an interruption in the UI benefit payments for more than one week defines the end of an unemployment period. If a person who has transferred to the SI later returns to the UI system, a new unemployment period starts. For simplicity, we make no distinction between different types of SI benefits or between full and part-time sick leave. That is, we regard all SI periods the same. If an UI period ends for some other reason than sickness, e.g. because the person finds a job or starts a labor market program, the spell is treated as censored.

Our sampling procedure results in a sample of 10,845 individuals. For about 36 percent of them, the data includes multiple unemployment spells[17]. Table 1 and Table **2** below present some descriptive statistics. Table 1 gives statistics on the incidence and the duration of sick spells separately for the treatment and the comparison group. We see that the sick report rate is lower among the treated (8.5 %) than the comparison persons (9.9 %). Sick spells are slightly shorter among the treated, as well, whereas their UI spells are considerably longer. The latter can be due to a lower sick report rate, which implies fewer interruptions in unemployment and thus fewer but longer UI spells.

[15] If the reform also affects the duration of SI spells, it may affect the composition of the unemployed population through its effect on the hazard rate from sickness back to unemployment. UI spells beginning after the reform are, however, included in parts of the sensitivity analysis when the individual is used as stratification unit (see section 5.3.2).

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

[17] In the stratified analysis that also includes UI spells beginning after the reform (see section 5.3.2) there are multiple UI spells for about 49 percent of the individuals.

Table 1 Descriptive spell statistics

	Treatment group	Comparison group
No. of ind. with a UI spell	2,165	8,680
No. of ind. with an SI spell (%)	184 (8.5)	855 (9.9)
No. of UI spells	3,369	16,990
No. of transition to SI benefits (%)	228 (6.8)	1,012 (6.0)
Average spell length (days)		
UI benefits	54.6	35.7
SI benefits	51.3	53.6
No SI spells lasting:		
1 days	2	10
2-7 days	93	396
8-28 days	50	218
29-89 days	45	191
>90 days	38	197
No censored SI spells	30	143

Note: The sample consists of all individuals in the LINDA-database who began an unemployment period with income-related UI benefits during the period 2002-12-01 – 2003-06-30.

From Table 2 we can see that the individuals in the comparison group are, on average, younger, less educated, and have more young children compared to the treated. In general, they have fewer days left until their UI benefits expire in the beginning of the unemployment period. The proportion of women is also higher in the comparison group, as is the proportion of immigrants from non-OECD countries.

If we instead compare the sample of unemployed persons who report sick to the total sample of unemployed persons, we see that the proportion of women is lager among the sick, as is the average age and the proportion that is married. Also worth noting is that the sick individuals are closer to UI benefit expiration as compared to the total sample of unemployed persons.

Table 2 Descriptive covariate statistics (means)

	Sample of unemployed		Sample of sick	
	Treatment	Comparison	Treatment	Comparison
Female	0.27	0.63	0.38	0.68
Age	40.9	37.2	44.0	40.4
Education: High school	0.85	0.83	0.86	0.79
Education: Post high school	0.31	0.19	0.31	0.14
Immigrant: OECD	0.05	0.04	0.07	0.05
Immigrant: other	0.06	0.13	0.05	0.15
Married	0.47	0.46	0.53	0.52
Presence of children<18	0.35	0.44	0.34	0.50
Days left until UI benefit expiration (in the beginning of the UI spell)*	208.2	185.2	193.0	177.7
Average (previous) wage*	25,293	15,538	25,664	15,441
No. of individuals	2,165	8,680	184	855

Note: The sample consists of all individuals in the LINDA-database who began an unemployment period with income-related UI benefits during the period 2002-12-01 – 2003-06-30. Statistics marked by * are averages among spells. The other statistics are averages among individuals.

In Figure 5 we show the (smoothed) weekly inflow into sickness separately for the treatment and the comparison group. The inflow rate is here defined as the number of sick reports each week among UI recipients, divided by the total number of UI recipients that week. The picture should give a crude 'difference-in-difference' estimate of the effect of the policy change. We see that the difference between treated and the non-treated after the reform is larger than pre-reform difference. Hence, based upon these non-parametric estimates, the policy change seems to have been effective in decreasing sickness absence among the UI recipients. However, these empirical inflow rates do not control for any of the potentially important differences between the two groups, something which we will do in the next section.

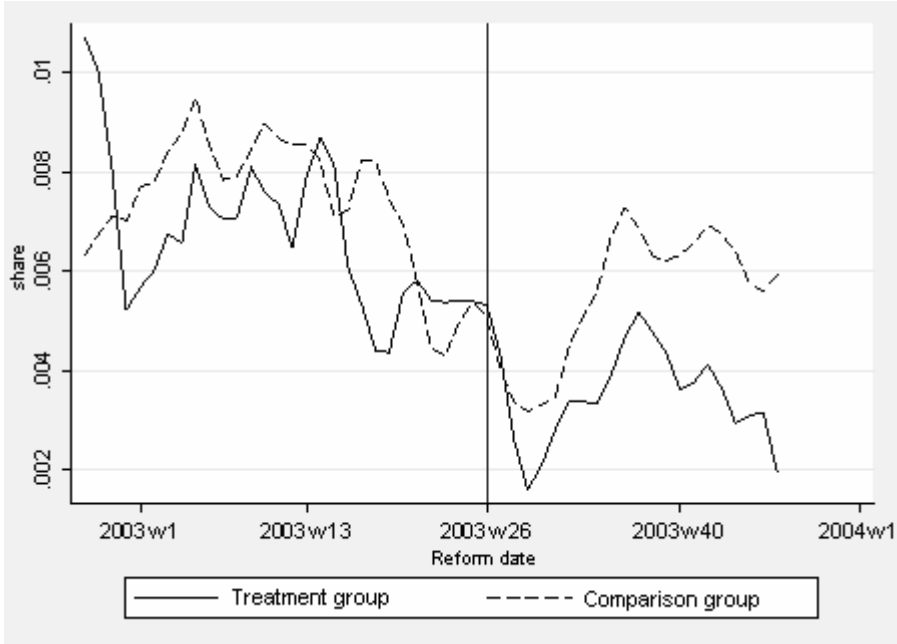

Figure 5 Weekly inflow into SI benefits among UI recipients before and after the reform, separately for the treatment and the comparison group

Note: The inflow rate is smoothed by running-line least-squares, bandwidth 0.1.

5 Empirical results

5.1 Incidence of sickness absence

The results for the Cox regression model are reported in Table 3, which consists of five different specifications estimated by partial maximum likelihood in discrete time.[18] Let us begin with column (1) which presents the results for a model that only includes a dummy for the reform date, a dummy for the treatment group, and an interaction variable called the 'cap reform effect'. The latter captures the effect of the reduced SI benefit cap on the treated population and is thus the parameter of main interest. The estimate for the cap reform

[18] See Cox (1972), Kalbfleisch and Prentice (1980), and Lancaster (1990).

effect is statistically significant and quite strong; it suggests that the reform reduced the incidence of sick reports among the treated by 33.7 percent.[19]

Column (2)-(5) present results for some further specifications of the model in which we control for a number of covariates (which are discussed below). In essence, including covariates does not change the result concerning the cap reform effect; the coefficient estimate increases slightly and remains statistically significant in all specifications. The estimated parameter in the regression including all covariates (column 5) is -0.451, which suggests that the reduced SI benefit cap lowered the transition rate to sickness absence with about 36 percent in the treated population.

Among the other variables, we notice that the coefficient for the reform date dummy is negative and significant, hence indicating a general decrease in sickness absence among the unemployed 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 of other changes in the environment occurring around July 1, 2003. The parameter estimate for the reform date dummy decreases substantially when we control for the month of inflow to unemployment (column 4 and 5). We will return to this point in the sensitivity analysis (section 5.3.1), where we stratify on the week of inflow to unemployment.

We also note that the incidence of sickness absence is significantly lower during the first 100 days of unemployment than later in the UI period. Hence, the probability of reporting sick seems to increase as the UI expiration date approaches, which is in accordance with the findings of Larsson (2006). However, as the last three specifications reveal, the hazard does not seem to be monotonically increasing as the expiration date comes closer. It is highest right before the expiration date, but reaches another peak right after the 100 UI day limit has passed, i.e. at the time when the UI benefits are reduced relative to the SI for many UI recipients.

Some of the demographic variables also obtain statistically significant parameter estimates. Being older is associated with a higher transition rate to sickness absence, and women have considerably higher transition rates than men – the difference being almost 45 percent. This large discrepancy has motivated us to also estimate the model separately for men and women. These estimations give in general less precise estimates (not reported), as should be expected, but

[19] The percentage effect is obtained by $100*(\exp(\beta)-1)$, where β is the parameter of interest.

the cap reform effect is still significant at the five percent level for the male population. The cap reform effect is however not found to be significantly different between men and women[20].

Among the other results presented in Table 3, we note that a post-high school education is associated with a significantly lower transition rate into sickness absence than is an education below the high school level. Moreover, the sick report rate appears to be significantly higher for those who have children living at home.

[20] The hypothesis of equal effects is tested by including an interaction term between the cap reform effect and the female dummy in the regression including both men and women.

Table 3 Estimated effects on the incidence of sickness absence

	(1)	(2)	(3)	(4)	(5)
Past July 2003 (t) $\left(D_t^{July03}\right)$	-0.420***	-0.442***	-0.442***	-0.201*	-0.233**
	(0.088)	(0.088)	(0.088)	(0.111)	(0.110)
Previous wage>20,696 (D^T)	-0.208**	-0.145*	-0.129	-0.127	-0.018
	(0.084)	(0.084)	(0.084)	(0.084)	(0.092)
Cap reform effect (t) $\left(D_t^{July03} * D^T\right)$	-0.411**	-0.433**	-0.427**	-0.439**	-0.451**
	(0.185)	(0.185)	(0.185)	(0.185)	(0.186)
Before 100 UI days limit (t)		-0.525***			
(=300-201 days until UI-expiration)		(0.064)			
300-251 days until UI-exp. (t)♣			-0.508***	-0.516***	-0.454***
			(0.103)	(0.103)	(0.104)
250-201 days until UI-exp. (t)♣			-0.355***	-0.356***	-0.320***
			(0.105)	(0.105)	(0.106)
200-151 days until UI-exp. (t)♣			-0.102	-0.103	-0.076
			(0.108)	(0.108)	(0.109)
150-101 days until UI-exp. (t)♣			-0.200**	-0.198**	-0.164*
			(0.095)	(0.095)	(0.096)
100-51 days until UI-exp. (t)♣			-0.202**	-0.200**	-0.180*
			(0.100)	(0.100)	(0.100)
Inflow into unemployment (month)	No	No	No	Yes	Yes
Female					0.371***
					(0.064)
Age					0.117***
					(0.021)
Age^2					-0.001***
					(0.000)
Immigrant: OECD					-0.043
					(0.132)
Immigrant: other					-0.096
					(0.093)
Education: High school					0.017
					(0.076)
Education: Post high school					-0.266***
					(0.081)
Married					-0.017
					(0.068)
Presence of children<18					0.154**
					(0.075)
ln (previous wage)					-0.015
					(0.046)
County dummies					Yes
-2 Log likelihood	17,453	17,383	17,365	17,340	17,163
No of observations	20,359	20,359	20,359	20,359	20,339

Note: Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (t) denotes time-varying variable.♣Reference is 50-1 days until UI-expiration.

5.2 Heterogeneous effects

The size of the decrease in SI benefits due to the reform depends on the person's previous wage – the higher the wage, the larger the percentage reduction in benefits. We would thus expect the largest response to the reform to be found among those with the highest previous wages. In Table 4, column (2), we present results from a regression in which we have separated the treatment group (T) into two groups: those with a previous wage ranging between SEK 20,696–24,125 and who experienced a 3–16.8 percent cut in benefits (T1); and those with a previous wage above SEK 24,125, who experienced a benefit cut of 16.8 percent (T2). For reasons of comparison, column (1) reproduces the average effect, i.e. Table 4, column (5).

As expected, it is in the high wage group (T2) that we find the largest responsiveness to the reform: the cap reform effect is statistically significant and suggests a 47 percent decrease in sick reports due to the reduced benefit ceiling. No statistically significant effect is found for the middle wage group (T1).

In column (3) we have instead divided T into two groups based upon the number of days left until the UI expiration date. More specifically, we investigate whether the cap reform effect is different for those who have 200–300 UI days left (have *not* passed the 100 day limit), compared to those who have less than 200 UI days left (have passed the 100 day limit). Recall that passing the 100 days-limit implies a drop of the UI benefit cap by approximately 7 percent, as is shown in Figure 1. This created an even larger discrepancy between the SI and UI benefits before the reform and still creates a small difference in benefits after the reform for high wage unemployed persons. We see that the cap reform effect only appears significant for the 'after 100 days group', that is for the unemployed closest to UI expiration.

Finally, column (4) shows results when T is split up both along the wage and the expiration date dimension. As should be expected (given the results above), it is among the unemployed in the highest wage group and with relatively few UI days left that the responsiveness to the reform is strongest. Hence, the effect of the reduced benefit cap is largest in the group which before the reform had the largest incentives to report sick – those whose SI benefits were substantially higher than the UI benefits and those closest to the UI expiration date.

Table 4 Interacting the cap reform effect with previous wage and duration until UI benefit expiration

	(1)	(2)	(3)	(4)
Past July 2003 effect $\left(D_t^{July03}\right)$	-0.233**	-0.234**	-0.236**	-0.237**
	(0.110)	(0.111)	(0.111)	(0.111)
Previous wage>20,696 (D^T)	-0.018		-0.018	
	(0.092)		(0.092)	
Middle wage (20,696-24,125) (D^{T1})		-0.017		-0.016
		(0.108)		(0.108)
High wage (>24,125) (D^{T2})		-0.027		-0.027
		0.136		(0.136)
Cap reform effect $\left(D_t^{July03} * D^T\right)$	-0.451**			
	(0.186)			
Cap reform effect*middle wage $\left(D_t^{July03} * D^{T1}\right)$		-0.316		
		(0.228)		
Cap reform effect*high wage $\left(D_t^{July03} * D^{T2}\right)$		-0.643**		
		(0.288)		
Cap reform*before 100d-limit $\left(D_t^{July03} * D^T * before\ 100\ days\right)$			-0.337	
			(0.339)	
Cap reform*after 100d-limit $\left(D_t^{July03} * D^T * after\ 100\ days\right)$			-0.484**	
			(0.205)	
Cap ref*bef 100d-limit*middle wage $\left(D_t^{July03} * D^{T1} * before\ 100\ days\right)$				-0.011
				(0.402)
Cap ref*after 100d-limit *middle wage $\left(D_t^{July03} * D^{T1} * after\ 100\ days\right)$				-0.405
				(0.255)
Cap ref*bef 100d-limit*high wage $\left(D_t^{July03} * D^{T2} * before\ 100\ days\right)$				-0.835
				(0.598)
Cap ref*after 100d-limit*high wage $\left(D_t^{July03} * D^{T2} * after\ 100\ days\right)$				-0.594*
				(0.313)
Six categories for # days until UI benefit expiration (t)	Yes	Yes	Yes	Yes
Inflow into unemployment (month)	Yes	Yes	Yes	Yes
All other covariates included	Yes	Yes	Yes	Yes
-2 Log likelihood	17,163	17,162	17,163	17,161
No of observations	20,339	20,339	20,339	20,339

Note: Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (t) denotes time-varying variable.

5.3 Sensitivity analysis

5.3.1 Effects of the time of inflow into unemployment

The effect of the reduced benefit cap is partly identified by comparing the evolution of hazard rates into sickness for people who experienced the reform at different lengths of unemployment. This means most importantly that we compare people based upon when they became unemployed. If there is (health) heterogeneity among the unemployed with respect to the time of unemployment this could potentially affect our results. For instance, it may matter if such heterogeneity differs over time in divergent ways for the treatment and the comparison group.

In order to check whether this type of heterogeneity affects our findings, we perform stratified analyses using a stratified partial maximum likelihood estimator (see e.g. van den Berg 2002, section 6). By stratifying on the week of inflow into unemployment, the reform effect is now identified solely by the comparison of individuals beginning their unemployment period during the same week. The results from this analysis are presented in Table 5, column (2). In column (3) we show results when the week of inflow into unemployment *and* the local labor market (county) are used as stratification units. This allows there to be heterogeneity with respect to inflow week that differs between different local labor markets. The estimate for the 'cap reform effect' is very similar to that obtained earlier (shown in column 1) for both regressions. Hence, heterogeneity with respect to the time of inflow into unemployment does not seem to distort our findings.

Table 5 Estimated effects on incidence, using the week of inflow into unemployment and the local labor market (county) as stratification units

	(1) Main results	(2) Stratification by week of inflow	(3) Stratification by week of inflow *and* local labor market
Past July 2003 (t) $\left(D_t^{July03}\right)$	-0.233**	-0.809***	-0.966***
	(0.110)	(0.253)	(0.263)
Income>20,696 (D^T)	-0.018	-0.011	-0.014
	(0.092)	(0.092)	(0.097)
Cap reform effect (t) $\left(D_t^{July03} * D^T\right)$	-0.451**	-0.497***	-0.503**
	(0.186)	(0.187)	(0.203)
Six categories for # days until UI benefit expiration (t)	Yes	Yes	Yes
Inflow into unemployment (month)	Yes	No	No
All other covariates included	Yes	Yes	Yes
Stratification by week of inflow	No	Yes	Yes
Stratification by local labor market	No	No	Yes
-2 Log likelihood	17,163	12,700	6,578
No of observations	20,339	20,339	20,339
No of strata	-	31	673

Note: Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (t) denotes time-varying variable.

5.3.2 Effects of persistent individual heterogeneity

There may of course be individual heterogeneity due to other factors than time and local labor market. One example relates to the outflow to employment. If people who find jobs on average have better (or worse) health than those who remain unemployed, this may cause the composition of our sample regarding health status to change over time. Divergent labor market opportunities for the treated and the comparisons may then imply different compositional changes regarding health status in the two groups, which in turn may affect our estimates.

In order to improve on this part, we use the fact that we have multiple unemployment spells for about half (49 percent) of the individuals in our sample (if we also sample unemployment spells beginning after the reform) and estimate the model only using within individual variation. Hence, we re-estimate the model using the stratified partial maximum likelihood estimator, but this

time we use the individual as the stratification unit.[21] The estimates produced by this approach are robust with respect to individual heterogeneity that is persistent over time.[22] The obvious drawback with this method is that we now identify the cap reform effect using only a selected sample of the unemployed.[23]

The results from this robustness check are presented in Table 6, column (2). We see that the cap reform effect is similar in size to the effect obtained earlier. The estimate is somewhat less precise, but still significantly different from zero at the five percent level. Hence, our previous results seem robust with respect to this type of compositional change.

Table 6 Estimated effects on incidence, using the individual as a stratification unit

	(1) Main results	(2) Stratification by individual
Past July 2003 (t) $\left(D_t^{July03}\right)$	-0.233**	-0.338*
	(0.110)	(0.175)
Income>20,696 (D^T)	-0.018	0.913
	(0.092)	(0.587)
Cap reform effect (t) $\left(D_t^{July03} * D^T\right)$	-0.451**	-0.497**
	(0.186)	(0.245)
Six categories for # days until UI benefit expiration (t)	Yes	Yes
Inflow into unemployment (month)	Yes	Yes
All other covariates included	Yes	-
-2 Log likelihood	17,163	2,158
No of observations	20,339	35,044
No of strata	-	14,525

Note: Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (t) denotes time-varying variable.

[21] A similar approach has been used in previous empirical studies; see e.g. Johansson & Palme (2004) and Lindeboom & Kerkhofs (2002), though the latter uses the workplace rather than the individual as a stratification unit.

[22] This method has the additional advantage of allowing for dependence between observations for the same individual. In our main analysis, we pool the observations without taking such possible dependence into account, which may result in underestimated standard errors.

[23] This method needs at least two UI spells for each individual to identify the reform effect, out of which at least one exists before the reform and one after. Moreover, individuals with one uncensored and one censored spell are not used *if* the censored spell is shorter than the uncensored spell.

5.3.3 Pre-treatment effects

The reduction of the SI benefit ceiling was announced already in April 2003, soon after a debate had arisen on the harmonization of the SI and UI system. Hence, it is possible that there would be a change of behavior regarding sickness absence among the high-wage unemployed during the months prior to the reform. In order to examine the existence of such 'pre-treatment' effects, we have re-estimated the model with a hypothetical reform in the beginning of June 2003, as well as in the beginning of May, April and March of the same year. Table 7 presents the results from this analysis.

We see that the point estimate for the 'cap reform effect' during the pre-reform period is only 22–46 percent of the point estimate for the actual reform effect and is never significantly different from zero. We conclude that there is no evidence of anticipatory behavior during the months preceding the reform.

Table 7 Pre-treatment effects

	Reform:	Hypothetical reforms:			
	July, 1st	June, 1st	May, 1st	April, 1st	Mars, 1st
Past July 2003 effect $\left(D_t^{July03}\right)$	-0.233**	-0.404***	-0.478***	-0.286***	-0.105
	(0.110)	(0.112)	(0.108)	(0.106)	(0.108)
Income>20,696 (D^T)	-0.018	-0.052	-0.061	-0.063	-0.020
	(0.092)	(0.098)	(0.105)	(0.122)	(0.142)
Cap reform effect $\left(D_t^{July03} * D^T\right)$	-0.451**	-0.209	-0.145	-0.101	-0.141
	(0.186)	(0.160)	(0.151)	(0.152)	(0.164)
Six categories for # days until UI benefit expiration (t)	Yes	Yes	Yes	Yes	Yes
Inflow into unemployment (month)	Yes	Yes	Yes	Yes	Yes
All other covariates included	Yes	Yes	Yes	Yes	Yes
-2 Log likelihood	17,163	17,159	17,153	17,168	17,175
No of observations	20,339	20,339	20,339	20,339	20,339

Note: Standard errors in parentheses. */**/*** denotes significance at the 10/5/1 percent levels respectively. (t) denotes time-varying variable.

5.4 Duration of sickness absence

Reduced economic compensation in case of sickness could be expected to affect not only the probability of reporting sick, but also the duration of sickness absence. Given that an individual transfers to the SI system, it seems reasonable to expect his or her sickness period to be shorter (on average) after the

reform compared to what it would have been if the compensation size had remained unchanged.

However, given that the reform had a strong effect on the incidence, we would not necessarily expect it to reduce the length of the observed sickness spells. If the reduction of the incidence is (mainly) due to reduced moral hazard, we would expect the average health of the treated population on SI benefits to be worse after the reform than before. Thus, the average duration of the SI spells should not necessarily decrease. In other words, the threshold for a few days' sick period due to minor illness is higher after the reform, thereby increasing the average length of SI periods.

When estimating the effect of the reform on the hazard rate out of sickness, we get no significant estimates (not reported), neither for the reform date nor for the reduced SI benefit cap.[24] Hence, the two counteracting effects seem to balance each other out, leaving the average duration of sick spells unchanged.

6 Discussion

Our results suggest a strong behavioral response to changes within the sickness insurance among the unemployed population. Using a reform within the sickness insurance that only affected some of the population – the treated – we estimate that the incidence of sick reports was reduced by about 36 percent more in this group compared to the comparison group that was not affected by the reform.

Up to this point we have made inference regarding the total behavioral response to the cap reform, without relating it to the magnitude of benefit reduction. In order to say something about the *economic* or *policy* significance of our estimates, we use the estimates from section 5.1 to calculate the elasticity of the sick report rate with respect to SI benefits. Futhermore, this makes it possible to compare our results with results from previous studies on sickness benefits.

The elasticity measure used is given by: $\hat{e} = (1 - \exp(\hat{\beta}) / \Delta)$, where Δ is the percentage decrease in benefits due to the reduced SI benefit cap (i.e. the reduction on top of the general 3 percent reduction due to the reduced repla-

[24] To do this we have created an additional dataset by following the sub-sample of unemployment spells that has ended in sickness, until they end, or at most, until the end of 2003. The effect of the reform on the duration is then estimated using model (1), as specified in section 3.

cement rate). The decrease in benefits is computed for each individual based on the factual difference between his or her old and new SI benefits. On average, SI benefits were reduced by 9.3 percent in our sample. The estimated elasticity is therefore 3.9.

Previous studies that estimate the effect of economic compensation on absence incidence among employed workers report lower elasticities: just below 1 in Johansson and Palme (2005) and 1.72 or 2.45 in Pettersson-Lidbom and Skogman Thoursie (2006), depending on whether monthy or weekly data is used. These estimates can be interpreted within the traditional labour supply framework as elasticities between leisure and consumption. Whether our estimate can be compared with them thus depends on whether unemployment in this context is assumed to be leisure or work, which in turn is not clear. However, it seems plausible that unemployed persons are more sensitive to changes in the SI compensation size than employed persons.

Our estimate is high even if compared to previous results concerning unemployed workers. Larsson (2004) and (2006) use data from the late 1990s and report an elasticity of around 1–1.5.[25] Whether this difference is due to a different time period or a different identification strategy is difficult to say.

One aspect that might affect our elasticity estimate concerns supplementary compensation for sickness and unemployment. The most common type of such benefits is insurance schemes regulated by collective agreements between unions and employers' organizations. These agreements vary across sectors and in some sectors even across firms. In general, they contain supplementary benefits above the cap for high-wage workers, implying that the denominator – the percentage decrease in benefits due to the reform – is overestimated and the elasticity underestimated. How much the elasticity is biased is difficult to estimate as we do not have data on which scheme the individual is covered by, and due to variation across schemes. For the identification of the cap reform effect however, the supplementary schemes do not pose any problem. Even though they imply that some high-wage unemployed receive considerably higher total benefits from the SI than from the UI even after the reform, they do not change the fact that the reform affected the treatment group and the comparison group differently.

To conclude, our results show evidence of moral hazard in the Swedish sickness insurance system. In fact, the moral hazard revealed by our study may

[25] The papers do not include an estimate of the elasticity but the results can be used to calculate it.

be of two kinds: First, assume that the drop in SI benefits made (relatively) healthy unemployed persons refrain from reporting sick. In that case, our results suggest that the reform *decreased* moral hazard within the sickness insurance.

Second, it could of course be the case that the drop in benefits made some truly sick persons refrain from reporting sick when this no longer was economically advantageous. Without access to health information we cannot determine this with certainty. If this is the case, our results actually suggest that the reform *increased* moral hazard within the unemployment insurance system. Active job seach is a formal requirement for UI eligibility, and unemployed persons who are too sick to apply for jobs should receive benefits the SI instead. The difficulty of determining whether the reform actually decreased moral hazard illustrates the importance of taking into account the whole social insurance system when designing reforms.

Economic incentives seem to be important for the use of sickness insurance among the unemployed. This in turn raises the question of whether interactions between the SI and the UI also matter for the job finding rate; does being financed by the UI rather than the SI matter for transitions to employment? Investigating the effects of the reform in July 2003 on the job finding rate is thus an interesting topic for further research.

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