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Economic incentives and long-term sickness absence:
the indirect effect of replacement rates on absence behavior

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WORKING PAPER 2015:17

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ISSN 1651-1166

Economic incentives and long-term sickness absence: the indirect effect of replacement rates on absence behavior[a]

by

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31st August, 2015

Abstract

Reductions in SI replacement levels has been a widely used instrument to lower sickness absence rates. The idea is that increasing the direct cost of absence would lower the absence rate. This paper explores a reform to the compulsory Swedish SI that meant that the replacement rate varied over the sickness absence spell. The reform reduced the replacement rate from 90 percent of foregone earnings to 65 percent during the 3 first days of a sickness absence spell and to 80 percent for days 4 – 90. From day 91 and onwards the rate remained at 90 percent. I show that the reform had, beside the previously shown direct effect, also an indirect effect on sickness absence behavior. The indirect effect stems from an increased cost of returning to work "to soon" and having to return on sick leave, this time with a lower replacement rate. I find that the indirect effect significantly reduces the probability to end an absence spell, creating a locking-in effect in sickness absence.

Keywords: Absenteeism, Sickness Insurance, Natural Experiment
JEL-codes: H51, I18, J22

[a]I would like to thank Matti Sarvimäki, Per Johansson, Johan Vikström, Erik Grönqvist, Marcus Eliason, Erika Lindahl and seminar participants at IFAU and Universidad ICESI for helpful comments.
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1 Introduction

Sickness insurance (SI) and disability insurance (DI) represent a large share of public expenditure in the industrialized world. In 2008 the average cost of sickness and disability benefits among OECD countries was estimated to be 2 percent of GDP. Sickness benefits accounted for almost half of this cost. This is almost three times more than was spent on unemployment benefits (OECD, 2009). The cost of sickness benefits is mostly due to long-term absence spells. For Sweden, 51 percent of ongoing sickness absence spells in December 2012 lasted more than 90 days (Social Insurance Agency, 2013). In addition to the direct cost of long-term sickness absence, a majority of disability claimants enter the system after a long sickness absence spell (OECD, 2009). There is considerable evidence showing that economic incentives matter for the take up rates of social insurance (e.g. Meyer et al., 1995; Curington, 1994; Johansson and Palme, 1996, 2002, 2005; Henrekson and Persson, 2004; Ziebarth and Karlsson, 2014; Pettersson-Lidbom and Thoursie, 2013). By now we know that high replacement rates increase take up rates of SI and the duration of short-term absence. However, less is known about the impact of replacement rates on long-term absenteeism. Two exceptions to this are Ziebarth (2013) and Hesselius and Persson (2007). In Ziebarth (2013), the author shows that a reduction of the replacement rate in Germany had no effect on long-term absenteeism. For Sweden, Hesselius and Persson (2007) show that an increase in the replacement rate for absence spells lasting longer than 90 days increased the duration of spells.

In order to add to this rather short list, I utilize a reform of the compulsory Swedish SI scheme that occurred at the beginning of 1991. The reform reduced the replacement level in the SI scheme from 90 percent of foregone earnings to 65 percent for the 3 first days of a sickness absence spell (short spells) and to 80 percent for day 4 – 90 (medium spells). From day 91 and onwards (long spells) the rate remained at 90 percent. The reason for the unusual incentive structure was a desire to cut public spending without punishing severely sick individuals. The reform also addressed the claimant's total compensation level. Prior to the reform, most workers on the Swedish labor market are covered by supplementary sickness compensation through their collective bargaining agreement. In general, the additional insurance replaced about 10 percent of foregone earnings. Following the reform any extra compensation after the 90th day led to an equivalent reduction in SI benefits. The new insurance scheme only affected new ab-

sence spells.[1] Individuals on ongoing sickness absence were able to remain in the old insurance scheme.

The reform had two separate effects on individual sickness absence behavior: First, a *direct* effect and secondly, an *indirect* effect. The direct effect stems from changes in the direct cost of entering into the sickness absence scheme (work-to-absence transitions), or continuing an absence spell (absence-to-work transitions). If economic incentives affect the sick-leave decision, I would expect fewer work-to-absence transitions while absence-to-work will increase for short (1–3 days) and medium-length (4 – 90) absence spells following the reform. The indirect, on the other hand, effect stems from changes to the indirect cost of returning to work. The indirect cost is caused by the risk of returning to work and having to start a new absence spell with a lower replacement rate. Again, if economic incentives play a role in the sick-leave decision, this implies that the indirect effect of the reform would lead to a reduced absence-to-work transition for medium (4 – 90 days) and long-term (more than 90 days) absence spells.

As shown above, the direct and indirect costs varies during the absence spell. For medium-length spells the costs even move in different directions. This shows how important it is to account for both these costs when evaluating the impact of a reform of this kind. If only the direct costs are included when estimating the reform effects on absence behavior, the results may be biased. Using detailed data on the complete account of all sickness absence spells during the period, I am able to estimate the *indirect* effect of the reform on sickness absence behavior. Emphasis will be on medium and long sickness absence spells.

Two studies (Johansson and Palme, 2002, 2005) have previously evaluated the reform. Both papers use a small sample of blue collar workers. The first study (Johansson and Palme, 2002) finds that the reform reduced work-to-absence transitions and prolonged absence durations, i.e. the reduced replacement levels led to fewer but longer absence spells. The second study (Johansson and Palme, 2005) shows that the work-to-absence transition rate decreased due to the reform and that the outflow from sickness absence increased for short absence spells (1 – 3 days), while it decreased for long absence spells (longer than 90 days). For medium-length absence spells (4 – 90 days), the estimated effect is insignificant. However, Johansson and Palme (2005) are unable to separate the direct and indirect effect on medium absence

[1] For the remainder of the paper, unless otherwise stated, absence and absence behavior refer to sickness absence.

spells. The insignificant estimate is thus the combined direct and indirect effect of the reform.[2] Furthermore, in their analysis of the effects within the duration (i.e. the effects of the reform between day 4 and 89 from day 90 onwards) Johansson and Palme (2005) were not able to properly take into account the compositional changes in the population of sickness absentees after the reform. In other words, since the new SI scheme influences the individual's decision to begin a spell of sickness absence, the population on sick leave after the reform will have on average poorer health than the population on sick leave before the reform.

The main contribution of this study is that I improve the identification strategy in Johansson and Palme, thereby allowing me to estimate the indirect impact of the reform. To this end, I use a random sample of all Swedish employees during the period in question. To identify the impact I utilize the fact that the new insurance scheme only affected absence spells that started after the reform was implemented. By only looking at individuals that started an absence spell prior to the reform (i.e. individuals that were unaffected by the reform at the time they began their sickness absence spell) I am able to fully control for the compositional effects of the reform and to identify the indirect effect. The reason for this is that the direct effect of the reform only affects new absence spells, ongoing absence spells are unaffected. The indirect effect of the reform, on the other hand, affects absence spells initiated in the post-reform period as well as ongoing absence spells. Thus, by only using absence spells from the pre-reform period, I am able to identify the indirect effect on absence behavior.

Since I have data on the universe of Swedish employees during the period in question, I am also able to extend the work by Johansson and Palme. The richer data set allows me to obtain more precise estimates of the reform's indirect effect on medium and long absence spells and to perform a heterogeneity analysis of the differences in effects between white collar workers and blue collar workers.

I find that the indirect effect of the reform reduced absence-to-work transitions both among medium as well as long-term absentees. The results are in accordance with the theoretical predictions. For medium absence spells, the reform reduces the probability to exit an absence spell by on average 17 percent. The corresponding effect on long absence spells is a 10-percent decrease. In comparison with previous evaluations of the reform, the effect on long-term absence

[2]This is not an issue for the long absence spells. For these Johansson and Palme identify and estimate the indirect effect.

is in line with Johansson and Palme (2005). However, unlike Johansson and Palme, I am also able to show that the indirect effect of the reform on medium absence spells is substantial and even greater than for the longer spells.

I also find that the indirect effect on absence-to-work transitions is stronger among white collar workers than among blue collar workers. The difference could probably be explained by the fact that white collar workers to a greater extent were covered by supplementary sickness compensation through their collective bargaining agreement before the reform but not after. This meant that the cost of returning to work was amplified for white collar workers. Furthermore, I find that male claimants react more strongly to the reform than women. This is especially true for medium-term absenteeism. This result supports previous findings that men, on average, tend to react more strongly to economic incentives in an SI scheme (see e.g. Henrekson and Persson, 2004; Johansson and Palme, 1996; Ziebarth and Karlsson, 2014). Finally, I find some indications that a claimant's sickness absence history increases the indirect effect, at least among individuals with long absence spells in the past. This could be explained by a perceived a high risk of actually encountering the increase in the cost of returning to work (i.e. having another sickness absence)

The paper is structured as follows: Section 2 discusses the views of economists on health and sickness absence. It also reviews previous research. Section 3 constitutes a description of the aspects of the Swedish sickness insurance system relevant to this study, including the 1991 reform used in the analysis. Section 4 provides stylized theoretical predictions of the reform's effect on absence behavior. Section 5 describes the data and the sampling method used. It also explains the empirical strategy. Section 6 presents the main results. Section 7 presents the heterogeneous effects for absence-to-work transitions. Finally, Section 8 concludes the paper.

2 Health, sickness absence and economic incentives

Ill health and sickness absence are clearly closely related. Ill health is the reason for most sickness absence spells, but not all health problems lead to sickness absence. Assume that an individual's health ranges from very poor to very good. If the health status is below a certain level it is impossible for any individual to fulfill a work task. Economic incentives have no effect on the absence behavior of this group. At the other end of the spectrum, if an individual's health is over a certain level, (s)he would not qualify as ill and no doctor would

provide him/her with a certificate for sick leave.[3] Between these two clear-cut cases there are a number of borderline cases where it is possible for an employee to attend work but also to report in sick. If the individual can choose between being on sickness absence or being at work and is indifferent between the two alternatives (given their current health status, the SI replacement level, work tasks, etc.), a change in the replacement level could affect the decision whether to go to work or not (Holmlund, 1991).

Most of the previous economic research on sickness absence has therefore focused on the economic incentives of the sickness insurance scheme and other potential determinants of sickness absence such as cyclical fluctuations and employment protection legislation. From this we know that the unemployment rate and absenteeism are negatively correlated. This is due both to changes in the composition of the labor force as well as behavioral responses (see e.g. Arai and Thoursie, 2005; Askildsen et al., 2005). Another result is that workers increase their sick leave usage when full employment protection is provided (Lindbeck et al., 2006; Ichino and Riphahn, 2005; Riphahn, 2004). There is also considerable evidence suggesting that an increased generosity of sickness benefits tends to increase absence rates. For the United States, Meyer et al. (1995) show that workers' compensation for work-related injuries leading to temporary work absence led to an increase in absence duration in the 1980s. Curington (1994) found mixed results on work absence behavior following a number of legislative changes on the benefit levels from 1964 – 1983. Johansson and Palme (1996) empirically revealed that the direct cost of being absent has a negative impact on work absence for a sample of male blue-collar workers. Henrekson and Persson (2004) uses aggregated Swedish data to study a number of legislative changes in the replacement rate to show that economic incentives have a strong impact on absence behavior. Pettersson-Lidbom and Thoursie (2013) show that an increase in Swedish benefits levels in 1987 led to an increase in absence.

Theoretical research on this topic is rarer. Some examples are Ehrenberg (1970), Barmby et al. (1994), Coles and Treble (1996) and Engström and Holmlund (2007). All four studies show that higher unemployment benefits increase absenteeism among employed workers.

There are fewer studies when it comes to long-term sickness absence and locking-in effects. In a study by Ziebarth (2013), the author assessed a reduction in the replacement rate in the German health insurance system. The results suggest that the reform had no effect on long-term

[3] This is based on the assumption that the physician is able to observe the individual's health status.

absenteeism on the average population. Ziebarth, on the other hand, found some heterogeneity in the effects and a small but significant decrease in absence durations for the poor and for middle-aged full-time employees. Hesselius and Persson (2007) evaluated a policy change to the Swedish SI scheme in 1998. After the reform, insurance claimants were allowed 10 percentage points additional compensation from collective agreements between day 91 and 360 of an absence spell. The results indicate that absence spells of at least 91 days increased by 4.7 days on average.

3 Swedish sickness insurance and the reform

Sweden's sickness insurance system replaces income for workers who cannot perform their usual work tasks because of temporary illness. The SI scheme is financed through payroll taxes on wages and covers all employees whose employers pay payroll tax. The scheme is administered by the Swedish Social Insurance Agency (SIA).

The state's monitoring of the insurance is very lax, especially during the first seven days of an absence spell. During this period it is up to the worker to decide whether (s)he is ill and to what extent this warrants an absence from work. From the eighth day of the absence spell, a medical certificate from a physician is needed. A physician is only supposed to write a certificate for an individual who is in such poor health that (s)he is unable to fulfill his/her ordinary work tasks. However, even with a medical certificate, there is some evidence indicating that patients may exert a strong influence over their sickness absence since they are able to make their physician issue a certificate even though he/she would not normally recommend sickness absence for the patient in question (e.g. Svärdsudd, 2000; Englund, 2008). Based on the information on the medical certificate, the local Social Insurance Office makes a decision whether to sick-list an individual or not. In other words, it is the local Social Insurance Office that monitors the insurance and prevents abuse of the system.

Both the proportion of earnings paid to the worker by the SI scheme, and the employer's responsibility for sickness benefits have changed on several occasions during the last few decades. The reform I am examining was implemented on March 1st 1991.

This reform changed the replacement level for all insured workers. The scheme changed

from a uniform compensation level of 90 percent of foregone earnings[4] from the first day of a sick spell onwards, to a scheme where the level depended on the claimant's absence duration. More precisely, after March 1st 1991 the replacement rate of the SI was reduced to 65 percent for the first three days of an absence spell and to 80 percent from day four to day 90. After 90 days the replacement rate returned to 90 percent of foregone earnings.[5] The reform applied to new spells only, not retroactively to ongoing spells. For this group, replacement rates remained at 90 percent. Besides changing the foregone earnings compensated through the SI scheme, the reform also addressed the claimant's total compensation level. Most workers on the Swedish labor market are covered by supplementary sickness compensation through their collective bargaining agreement.[6] In general, this additional insurance replaced about 10 percent of the foregone earnings (i.e. a significant number of workers had a complete coverage rate).[7] Following the 1991 reform, any extra compensation after the 90th day of sickness absence led to an equivalent reduction in SI benefits.

The significant changes to the SI scheme were one of several proposed budget cuts proposed by the Swedish government at the beginning of 1991. The suggested cuts were a response to the deep economic crisis in Sweden at the time. The unorthodox design of the new SI scheme was motivated by a desire to reduce public spending without economically punishing the sickest.

The compensation levels stayed the same until April 1993 when the replacement rate for absence of more than 90 days decreased to 80 percent. At the same time a waiting period of one day was introduced into the SI scheme.

4 Theoretical predictions

This section describes how the reform could theoretically have affected sickness absence behavior on two margins: work-to-absence transition and absence-to-work transition. I follow

[4]The insurance only replaces earnings up to the social security ceiling of 7.5 price base amounts. The price base amount is a measure set by the Swedish Government a year at a time. The amount is calculated based on changes in the consumer price index. The price base amount has various uses, including ensuring that sickness benefits, study support, etc., do not decline in value because of an increase in the general price level. In 1991, about 7 percent of the labor force had labor earnings above the social security ceiling.

[5]The compensation was still limited by the social security ceiling of 7.5 price base amounts. In 1991 this amounted to SEK 32,200, approximately EUR 3,300.

[6]The employee does not need to be a member of a union to be entitled to the supplementary compensation, as long as the employer has signed a collective bargaining agreement all employees are automatically covered.

[7]Since sickness insurance benefits were taxed differently compared to labor income this actually meant that people earned more while they stayed at home on sickness absence.

Johansson and Palme (2005) and define the *direct* cost of being absent from work (i.e. exit a work spell or continue an absence spell) as the percentage share of earnings not replaced by the SI scheme. The *indirect cost*, the cost of *returning* to work, i.e. exiting an absence spell and risking having to begin a new absence spell, is defined as the difference between the share of earnings not replaced by the SI scheme during an ongoing spell and the corresponding share of a new spell after returning to work.

Table 1 summarizes how these costs differed before and after the reform, how they varied over the length of an absence spell and whether the spell was ongoing at the time of the reform or if it started afterwards.

Table 1: Summary of the direct and indirect costs on sickness absence associated with the reform. For for ongoing and new absence spells, divided by absence spell length.

	Direct cost			*Indirect cost*		
Spell duration	Pre-reform	Post-reform	Diff	Pre-reform	Post-reform	Diff
Ongoing spells						
1 – 3 days	10 %	10 %	0 %	0 %	25 %	25 %
4 – 90 days	10 %	10 %	0 %	0 %	25 %	25 %
91 – days	10 %	10 %	0 %	0 %	25 %	25 %
New spells						
1 – 3 days	10 %	35 %	25 %	0 %	0 %	0 %
4 – 90 days	10 %	20 %	10 %	0 %	15 %	15 %
91 – days	10 %	10 %	0 %	0 %	25 %	25 %

Note:The direct cost is defined as the as the percentage share of earnings not replaced by the SI scheme. The indirect cost is defined as the difference between the share of earnings not replaced by the SI scheme during an ongoing spell and the corresponding share of a new spell after returning to work.

Table 2 show how the direct and indirect costs theoretically could affect sickness absence behavior. For ongoing spells the direct cost remained unchanged at 10 percent. The claimant kept receiving 90 percent of foregone earnings throughout the absence spell. Therefore, the reform should not have had any direct effect on absence-to-work transitions for this group. But, since the indirect costs increased for ongoing absence spells there would be an expected indirect effect as well. If economic incentives affect the sick-leave decision, the increased indirect cost should lead to a decrease in absence-to-work transitions. For new absence spells, i.e. spells started after the reform, the increased direct costs of absence spells of less than 90 days should have a negative impact on work-to-absence transitions. However, if indirect costs are also taken into account, the theoretical predictions become more ambiguous and depend on the length of the spell. For short absence spells, less than 4 days, the effect is unambiguously negative. For medium absence spells (4 – 90 days) the direct and indirect effect move in opposite directions,

thus making the total effect impossible to predict. Longer absence spells (more than 90 days) are expected to increase due to the increased indirect costs.

Table 2: Summary of the direct, indirect and total effects of the reform on sickness absence for ongoing and new absence spells. Divided by absence spell length.

	Direct Effects	Indirect Effects	Total Effects
Ongoing spells			
1 – 3 days	0	+	+
4 – 90 days	0	+	+
91 – days	0	+	+
New spells			
1 – 3 days	-	0	-
4 – 90 days	-	+	±
91 – days	0	+	+

As shown, the direct effect and the indirect effect on absence-to-work transitions move in opposite directions, and vary during the absence spell. The a priori effect of the reform on absence-to-work transitions is thus unclear. In the empirical analysis I will focus solely on the indirect effect of the reform on ongoing absence spells.[8] The reason for only looking at ongoing spells is that it allows me to isolate the indirect effect of the reform on sickness absence behavior.

5 Data and empirical strategy

The data used in this paper are created from population-wide registers: the Sickness Benefit Register and the LOUISE database. The LOUISE database is administered by Statistics Sweden and covers the entire Swedish population aged 16 – 64. The database includes background variables such as age, gender, number of children in the household, marital status, area of occupation, and annual labor earnings. The Sickness Benefit Register is administered by the Swedish Social Insurance Agency (SIA) and is an event data base with information on sickness insurance benefit payments for every individual who has been sick and received sickness insurance benefits. The register records the start and end date for every sickness absence spell in Sweden between 1987 – 1991 where the individual was entitled to sickness benefits from the social insurance system. During the period in question, the SIA was responsible for benefit

[8] For ongoing spells, the indirect effect equals the total effect since the reform has no direct effect on these spells.

payments from the first day of absence onwards.[9] In addition to the start and end dates, the register contains information on daily benefit amounts, employment status at the beginning of each spell, and whether the absence is full time or part time.

5.1 Empirical strategy for estimating the indirect effects

To investigate the indirect effect on absence behavior, I utilize the fact that the compensation level only changed for spells initiated after March 1st, 1991. The sample is thus constructed from the pre-reform period only. The sampling strategy allows me to handle the compositional effects created by the reform.[10]

In order to obtain the sample used in the estimations, I select a random sample of five percent of all new sickness absence spells in January and February 1991. I define the study group as individuals who started their absence spell during this period, i.e. just before the reform, but who exited it *after* the reform had been implemented (i.e. after March 1st, 1991). The comparison group, on the other hand, consists of individuals who both started and exited an absence spell in January or February 1991. The sample strategy allows me to compare absence-to-work transitions shortly before the reform with absence-to-work transitions shortly after the reform. Spells are censored after 365 days. I restrict the sample to individuals aged between 20 and 63. To account for seasonal effects, a corresponding sample of new sickness absence spells in January and February 1990 is also included in the analysis. The importance of seasonal effects is illustrated in *Figure 1*. The figure shows the day-by-day work-to-absence transition for the period 1990 – 1992. The dashed, horizontal line marks the reform date. *Figure 1* clearly reveals a seasonal pattern in work absence. Not accounting for these patterns would distort the estimates. Including spells from 1990 in the analysis also allows me to identify the effect on long absence spells. The final sample consists of 195,000 absence spells.

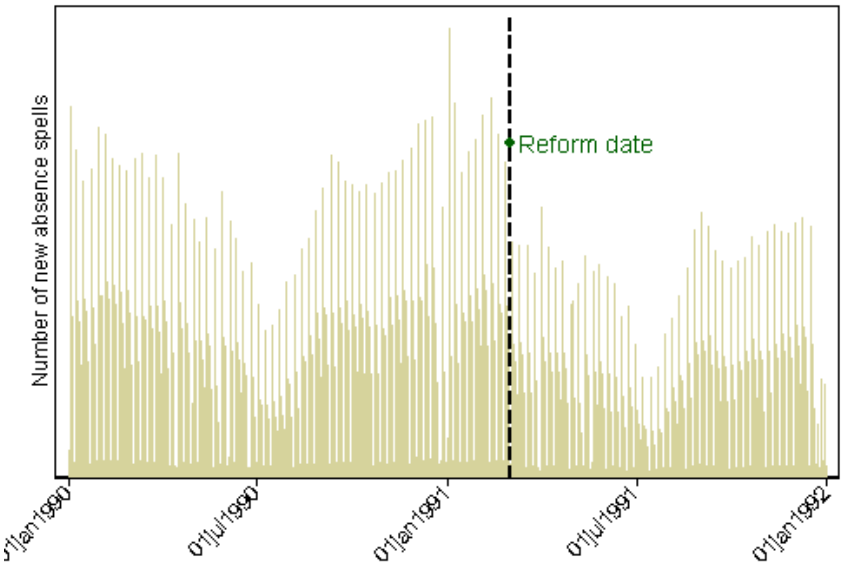
It is also worth noting that *Figure 1* shows that there is a distinct drop in the transition rate around the time of the reform. This drop did not occur around the same time in previous years. I see this as the first sign of the reform affecting absence behavior.

In order to analyze the absence-to-work transitions, I estimate a piecewise constant Cox-

[9] As from January 1st 1992, responsibility for the SI scheme during the first 10 calendar days was transferred to the employer. Short-term sickness spells for employed workers are therefore no longer registered by the SIA.

[10] Even though it is unlikely, there could still be a behavioral response to the reform using this sample. The reform was announced about two weeks prior to the implementation, potentially influencing people to start a sickness absence before March 1st 1991. I test this hypothesis by only using spells started before February 10th. The results are robust.

Figure 1: Day-by-day work-to-absence transitions.



proportional hazard (CPH) model. This strategy exploits the fact that the absence spell can be divided into three intervals, each having a different replacement rate, and thus entailing a different cost of returning to work. I let the impact of the reform vary over the two pre-specified time intervals: 4 – 90 days (medium) and more than 90 days (long).

To estimate the indirect effect on medium and long-term sickness absence, I use the following model specification:

$$h_{it}(M_{it}, R_{it}, I_{it}^{4-90}, I_{it}^{90+}, X_{it}, t) = h_0(t) exp(\beta_0 X_{it} + \beta_1 M_{it} + \beta_2 R_{it} I_{it}^{4-90} + \beta_3 R_{it}^{90+})$$

where $h_0(t)$ is the baseline hazard. X_{it} is a vector of individual specific covariates, M is a dummy variable taking the value one if the absence spell lasted beyond March 1st, 1990 or 1991 and zero otherwise. R is an indicator of whether the spell is affected by the reform, i.e. lasted beyond March 1991, or not. I_{it}^{4-90} and I_{it}^{90+} indicates whether the spell had lasted 4 – 90 days or more than 90 days at day t, respectively. The parameters of interest, β_2, and β_3 are the indirect effects for 4 – 90 and longer than 90-day durations respectively. If the reform had the impact outlined in Section 4 (i.e. absence-to-work transitions decreased as the indirect cost of absence increased) the β's should be less than one, indicating a decrease in the hazard rate and prolonged absence spells.

If there is an indirect effect on day 4 – 90 of an absence spell such that the outflow from

absence is reduced, the estimated effect of β_3 will be attenuated towards one. The reason for this attenuation effect is simply that the comparison population remaining after day 90 has poorer health than the remaining study population (affected by the economic incentives).[11]

A complication with the analysis is the recession that hit Sweden during the early 1990s. During the fall of 1991, the Swedish unemployment rate increased considerably. There is a literature that indicates that there is a relationship between the unemployment rate and the absence rate (see e.g. Arai and Thoursie, 2005; Askildsen et al., 2005). I take this into account by using quarterly data from Statistics Sweden on the local labor market unemployment rates in January and February 1990 and 1991, respectively.[12]

In order to assess the robustness of the results, I will also perform a number of placebo regressions, i.e. move the reform one year back in time. These results are presented in Section 6.2.

6 Results

6.1 Main effects

As stated in Section 4, the increase in the cost of returning to work could lower the absence-to-work transition rate, i.e. individuals on sickness absence would prolong their absence spell because of the risk of having to start a new absence spell and receive a lower compensation level. If this were the case, the reform would create a locking-in effect in the insurance.

Table 3 shows the estimated indirect effect of the reform on absence-to-work transitions. I use four different model specifications in order to assess the robustness of the results. The first column presents the baseline model, controlling only for seasonal effects. The model specification in column (2) adds a vector of individual specific covariates and county fixed effects. Finally, in columns (3) and (4) I also control for the local labor market unemployment rate. The first row of the table reports the estimated impact on medium-length absence spells (4 – 90 days). The second row reports the same impact on long-term sickness absence spells (more than 90 days).

Looking at the table, we see that the estimated coefficients are stable regardless of the model

[11] This is known as a dynamic selection problem in the literature (see e.g. van den Berg, 2001).

[12] I define the local labor market as the county of residence at the beginning of an absence spell. Sweden consisted of 24 counties during this period.

Table 3: Cox proportional hazard model estimates of the indirect effect on absence-to-work transitions

	(1)	(2)	(3)	(4)
Day 4 – 90	0.833**	0.834**	0.829**	0.829**
	(0.014)	(0.015)	(0.015)	(0.015)
Day 91 –	0.922*	0.904*	0.899*	0.898*
	(0.038)	(0.039)	(0.039)	(0.039)
County	No	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Unemployment	No	No	Yes	Yes
Unemployment^2	No	No	No	Yes
N	287,659	275,828	275,828	275,828

Note: Hazard ratios. The baseline hazard is specified as piecewise constant. Unemployment is measured at the county level. Controls include dummies for the level of education, age, sector of employment (2 digits), and gender. Absence spells are censored after 365 days. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$

specification. Let us start with the impact on medium spells. In the baseline specification, column (1), the estimated coefficient is 0.833, a 16.7 percent decrease in the hazard. The estimated coefficient diminishes slightly in column (2). When also including individual specific covariates, the reform reduces the probability to exit an absence spell by, on average, 16.6 percent. The indirect effect increases slightly to around 17.1 percent when controls for local labor market unemployment rates are included in the model in columns (3) and (4). All the estimated hazard ratios for medium-length absence spells are statistically significant at the one-percent level.

Let us now move on to the indirect effect on long absence spells which are spells of more than 90 days. Again, the estimated coefficients are fairly stable for all four model specifications. Using the baseline specification, the estimated coefficient is 0.922, a 7.78 percent reduction to the hazard. The second model specification returns an estimate of 0.904. For the most flexible model specifications – columns (3) and (4) – the estimated indirect effect again rises slightly (0.899 and 0.898). The indirect effect of the reform reduces the probability of exiting an absence spell of 91 days or longer by, on average, 10.1 percent when indicators related to the local labor market unemployment rate are included in the model. All estimated hazard ratios on the long absence spells are statistically significant at the five-percent level.

The main message from *Table 3* is that the increase in the cost of returning to work caused a decrease in the absence-to-work transition. This is true for both medium and long absence

spells. The results are coherent with the theoretical predictions of Section 4, when the cost of returning to work increases, absence-to-work transitions decreases. The indirect effects for both medium and long absence spells are statistically significant at the five-percent level.

When comparing the size of the estimates, *Table 3* shows that the indirect effect seems to be relatively smaller for the longest absence spell. The indirect effect on the probability of ending an absence spell after day 90 is weaker compared to the 4 – 90 day interval. A reason behind the weaker effect could be the dynamic selection described in Section 6.1. The reduced outflow between day 4 and 90 should attenuate the indirect effect on absence spells longer than 90 days towards one. The fact that I, in spite of this attenuation bias, find a reduction in absence-to-work transitions for absence spells lasting longer than 90 days, strengthens the argument that indirect effects have an impact on absence behavior.

Additionally, economic incentives may have a more significant role for medium absence spells compared to longer ones. As health deficiencies lie behind most sickness absence spells, it is plausible to assume that the health of individuals with long absence spells is poorer than for those with shorter ones. Given this, the possibility to adapt behavior to a new insurance scheme might be more limited for this group.

6.2 Placebo effects

This section provide placebo estimates for absence-to-work transitions. I do this by moving the reform one year back in time (i.e. before the actual reform took place). If such placebo effects emerge as significant, it would indicate that the previously estimated effects do not represent an effect of the actual reform. In the estimations I use a sample of five percent of all new sickness absence spells in January and February 1989 and January and February 1990. The study group now consists of individuals who exited their absence spells *after* March 1st, 1990.

Table 4 presents the placebo effects from the same four model specifications used in Section 6.1. By simply looking at the point estimates, the placebo regression suggests that there are small negative pre-treatment effects for medium absence spells, and small positive pre-treatment effects for long absence spells. All estimates are, however, insignificant at the five-percent level. The one exception is for medium absence spells in column 2, where the estimated coefficient is significant at the five-percent level.

Besides the fact that all estimates but one are insignificant, the estimates are also smaller com-

Table 4: Cox proportional hazard model estimates of the placebo effect on absence-to-work transitions

	(1)	(2)	(3)	(4)
Day 4 – 90	0.980	0.963*	0.967	0.983
	(0.017)	(0.017)	(0.017)	(0.018)
Day 91 –	1.038	0.998	1.003	1.020
	(0.043)	(0.042)	(0.043)	(0.043)
County	No	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Unemployment	No	No	Yes	Yes
Unemployment^2	No	No	No	Yes
N	268,847	252,436	252,436	252,436

Note: Hazard ratios. The baseline hazard is specified as piecewise constant. Unemployment is measured at the county level. Controls include dummies for the level of education, age, and gender. Absence spells are censored after 365 days. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$

pared to the main results. With this in mind, I see no reason to change my conclusions in Section 6.

7 Heterogeneous effects

So far in the analysis I have assumed that the indirect effects had an impact on everyone in the same way. Clearly this is a restrictive assumption. In this section I therefore divide the sample by area of occupation (blue collar / white collar), gender, and by sickness absence history to see if there are heterogeneous reform effects in either of these dimensions.

7.1 Heterogeneous effects by area of occupation

Previous evaluations of the 1991 reform (Johansson and Palme, 2002, 2005) have focused solely on blue collar workers. The effect of the reform on white collar workers is thus unknown. This section, therefore, divides the indirect effect between blue *and* white collar workers. Blue collar workers are defined as individuals with less than three years of upper secondary education, while workers with three years of upper secondary education or more are defined as white collar workers.

I use the most flexible model specification, controlling for seasonal effects, a vector of individual covariates, the county of residence and the local labor market unemployment rate, and run separate regression for white and blue collar workers, respectively. The results are

shown in *Table 5*.

The estimates indicate that the indirect effect of the reform was greater among white collar workers (the difference is, however, not statistically significant). This result is not surprising. During the period, white collar workers were to a greater extent covered by supplementary sickness insurance through their collective bargaining agreements. In general, this additional insurance paid out 10 percent of foregone earnings on top of the regular SI replacement rate. In other words, white collar workers had a complete SI cover rate prior to the reform. Since the reform also addressed the claimant's total compensation level, any extra compensation over 90 percent of foregone earnings led to an equivalent reduction in SI benefits, the cost of returning to work was thus higher for white collar workers.

Table 5: Cox proportional hazard model estimates of the effect on absence-to-work transitions, by area of occupation

	(1) Blue collar	(2) White collar
Day 4 – 90	0.848** (0.018)	0.787** (0.027)
Day 91 –	0.930 (0.0470)	0.832* (0.0693)
N	191,139	84,689

Note: Hazard ratios. The baseline hazard is specified as piecewise constant. The models include controls for the local labor market unemployment rate (first- and second order) and individual covariates (dummies for the level of education, age, sector of employment (2 digits), county of residence, and gender). Absence spells are censored after 365 days. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$

7.2 Heterogeneous effects by gender

It is a well known fact that there is a gender gap in sickness absence. Women are, on average, more absent from work for health reasons than men (see e.g. Paringer, 1983; Broström et al., 2002; Mastekaasa and Olsen, 1998; Angelov et al., 2013). However, men have been found to react more strongly to changes to the SI replacement levels (see e.g. Henrekson and Persson, 2004; Johansson and Palme, 1996; Ziebarth and Karlsson, 2014). In light of this, it is interesting to examine whether the reform effects differ between men and women.

To investigate whether there are heterogeneous responses according to gender, I run separate regressions for men and women. *Table 6* presents the estimated indirect effects by gender. I use the most flexible model specification, seasonal effects, a vector of individual covariates, the county, and the local labor market unemployment rate. It is clear from the table that, for

medium length absence spells, the estimated indirect effect differs according to gender. Men reduced their probability to exit an absence spell by approximately 19 percent. The corresponding estimate for women is 14 percent. The estimates are significant at the one-percent level. For the longest absence spells, the point estimates indicate that men and women were affected to the same extent by the reform. However, neither of the estimates is statistically significant at the five-percent level.

Table 6: Cox proportional hazard model estimates of the effect on absence-to-work transitions, by gender

	(1) Men	(2) Women
Day 4 – 90	0.811** (0.019)	0.860** (0.023)
Day 91 –	0.912 (0.0492)	0.883 (0.0640)
N	150,788	125,040

Note: Hazard ratios. The baseline hazard is specified as piecewise constant. The models include controls for the local labor market unemployment rate (first- and second order) and individual covariates (dummies for the level of education, age, sector of employment (2 digits), county of residence, and gender). Absence spells are censored after 365 days. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$

To summarize, the results show that the indirect effect of the reform on absence-to-work transitions is greater for male workers compared to female workers. This difference is particularly evident in the medium length absence spells. The results are in line with previous research and lend support to the idea that men react more strongly to economic incentives and changes to the SI replacement levels compared to women.

7.3 Heterogeneous effects by sickness absence history

Since the increased cost of returning to work hinges on the assumption of a return to sickness absence, the claimants' sickness absence history may have an impact on the reform effect. It is plausible to assume that individuals with a history of regular and/or long sickness absence spells have, on average, poorer health. Given this, these individuals will likely perceive a high risk of actually encountering the increase in the cost of returning to work (i.e. having another sickness absence). If this is the case, I should find a bigger indirect effect among claimants with a history of sickness absence. Besides an increase in the risk of re-entering absence, individuals with a history of sickness absence may also have acquired a greater understanding of how the SI system works and therefore have a better knowledge of what the cost of returning to work

is. Again, this would suggest a greater indirect effect for those who have utilized the SI scheme in the past.

I test whether sickness absence history has an impact on the indirect reform effect by running separate regressions by the number of sick days in the two previous years. I divide the data into quartiles in order to make the groups equal in size.[13] The estimates are presented in *Table 7*.

Table 7: Cox proportional hazard model estimates of the effect on absence-to-work transitions, by the number of days on sick leave in the two previous years.

	0 - 11	12 - 28	29 - 65	66 -
Day 4 - 90	0.837**	0.866**	0.863**	0.840**
	(0.032)	(0.033)	(0.031)	(0.027)
Day 91-	1.007	0.991	0.946	0.851**
	(0.110)	(0.118)	(0.096)	(0.051)
N	71,113	68,584	67,321	68,810

Note: Hazard ratios. The baseline hazard is specified as piecewise constant. The models include controls for the local labor market unemployment rate (first- and second order) and individual covariates (dummies for the level of education, age, sector of employment (2 digits), county of residence, and gender). Absence spells are censored after 365 days. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$

Looking at *Table 7*, I see no strong correlation between sickness absence history and behavioral responses to the increased indirect cost. The estimated effects only varies between 13 and 16 percent (all estimates are statistically significant at the one percent level). When it comes to long absence spells, however, there is some evidence suggesting a stronger indirect effect among long-term absentees. The effect goes from zero among those with the fewest sickdays (0 – 11) to a statistically significant effect of 15 percent among those with the most sickdays (more than 61 days) in the previous two years.

8 Concluding remarks

The presence of moral hazards presents policy makers with a delicate problem: how do you balance the advantageous income-distribution properties of a generous sickness insurance system with the disadvantageous behavioral responses to such a system? In recent years, reductions in SI replacement levels have been widely used as a way of lowering absence rates. The cuts are justified by the assumption that economic incentives affect an individual's sick-leave decision.

[13] I have also run similar regressions using the number of absence spells instead of sick days as a measure of sickness absence history. Using this measure, I find no connection between sickness absence history and the indirect effect.

Increasing the cost of being absent would thus lower the absence rate.

In this paper, I have examined a reform of the Swedish SI that introduced differentiated replacement rates in the insurance scheme. Prior to the reform, the SI replaced 90 percent of foregone earnings from the first day of absence an onwards. Following the reform, the replacement level were reduced to 65 percent during the first three days of an absence spells and to 80 percent for days 4 – 90. From day 91 onwards the rate remained at 90 percent. The reason behind maintaining the compensation level for spells of more than 90 days was to avoid negative economic effects on an already disadvantaged group.

I show that the reform had an indirect effect on absence behavior. The indirect effect stems from the fact that the indirect cost (i.e. the cost of returning to work) increased due to the risk of having to start a new absence spell and receive the lower replacement rate. Using detailed data on the complete account of all sickness absence spells during the period, I am able to identify and estimate the indirect effect on absence-to-work transitions by comparing absence behavior before and after the reform.

The empirical findings confirm previous research concerning economic incentives and absence behavior; the absence-to-work transitions declined significantly after the reform. The indirect cost of returning to work seems to play an important role in the decision to end an absence spell. However, I find that the indirect effect was greater for medium-length absence spells compared to longer ones. Given that individuals on long-term sickness absence are more severely ill, this could be interpreted as evidence of economic incentives playing a limited role when it comes to influencing long-term absence behavior.

I also find some indications of heterogeneity in the results. White collar workers' reaction appears to be stronger than that of blue collar workers and male claimants seem to react more strongly to the reform than women (none of these differences are however statistically significant). The somewhat stronger effect among white collar workers adds knowledge to the literature since previous evaluations of the reform focused solely on blue collar workers. That men seem to have reacted slightly stronger than women to the reform is in line with findings that men, on average, tend to react stronger to economic incentives in the SI scheme (see e.g. Henrekson and Persson, 2004; Johansson and Palme, 1996; Ziebarth and Karlsson, 2014). I also find indications that the indirect effect increases with previous sickness absence; at least among previous long-term absentees. This could be due to an increased perceived risk of hav-

ing another sickness absence among this group.

To conclude, the introduction of differentiated replacement levels to the Swedish sickness insurance scheme increased the direct cost of sickness absence during the first 90 days of an absence spell. However, the reform also increased the indirect costs associated with a return to work. In this study, I show that these indirect costs affected absence behavior and that people on sick leave , on average, prolonged their absences following the reform. This result does not only show that, in line with economic theory, people on sick leave react to economic incentives but also the importance of taking potential indirect effects into account when designing and evaluating social insurance schemes. The Swedish SI scheme no longer utilizes differentiated replacement levels, instead the first day of a sickness absence period is a qualifying day. The qualifying day creates indirect costs similar to those studied in this paper. One significant difference however is that there is now a relapse rule. This means that if the employee has begun to work again after being reported sick and falls ill again within five calendar days, the new sickness absence period counts as a continuation of the previous one. This creates a SI scheme with direct costs associated with starting a sickness absence period, but without indirect costs associated with ending a sickness absence period and return to work.

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