IFAU – INSTITUTE FOR
LABOUR MARKET POLICY
EVALUATION

Insured by the partner?

Martin Olsson
Peter Skogman Thoursie

WORKING PAPER 2010:3

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

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

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

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

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

ISSN 1651-1166

Insured by the partner?*

by

Martin Olsson[a] and Peter Skogman Thoursie[©]

March 5, 2010

Abstract

This paper investigates whether the partner's social insurance coverage affects spousal labor supply. Using a reform which increased the sickness insurance coverage for non-government workers, the spousal elasticity of sick days with respect to the partner's benefit is estimated to 0.4. Additional analysis indicates that the partner's insurance coverage is partly affecting spousal labor supply through an insurance effect and the overall effect is particularly large among low income families. Joint leisure is not found to have an effect on the overall effect. We conclude that spouses pool their supply of labor. Thus if policy evaluations ignore spousal interactions they will underestimate the effect.

Keywords: Spousal labor supply, spill-over, social insurance programs
JEL-codes: H31, J22

* The authors are grateful for comments by Peter Fredriksson, Ethan Kaplan, Rafael Lalive, Mårten Palme, Lars Persson, Per Pettersson-Lidbom, Emilia Simeonova and Johan Vikström, as well as participants at the 2009 SUDSWEc conference in Uppsala.

[a] Department of Economics, Stockholm University, S-106 91 Stockholm, Sweden; Institute for Labor Market Policy (IFAU) and Research Institute of Industrial Economics (IFN); e-mail: martin.olsson@ne.su.se.

[©] The Institute for Labour Market Policy Evaluation (IFAU), S-751 20 Uppsala, Sweden; e-mail: peter.thoursie@ifau.uu.se

Table of contents

1 Introduction

All OECD countries (apart from South Korea) have some form of insurance which compensates workers for loss of wages caused by temporary non-work related illness. In most European countries this system is universal and often referred to as sickness insurance or temporary disability insurance (TDI). The total payment of benefits is often substantial and constitutes more than 1 percent of GDP in many countries (OECD, Social Expenditure Data Base).[1] Despite the economic significance of TDI programs, only a handful of studies have focused on the relationship between the insurance coverage and labor supply.[2] As regards spill-over effects within couples, there has been no study on how spousal labor supply is affected by the partner's TDI.[3]

In this article we assess if a higher benefit level in the TDI spills over to spousal labor supply, by exploiting a change in the Swedish TDI system in December, 1987. This reform increased the benefit level for workers in the private and the local government sector. At the same time, central government workers were unaffected. Using data on a representative longitudinal sample of 3.3 percent of the Swedish population where spouses in the government sector are matched with complete individual sickness history for the period 1986 to 1990, we estimate the causal spill-over effect with a difference-in-differences estimator (DD). Treatment is defined on the basis of whether the partner to a spouse is a non-government worker or not. In contrast to most other DD-studies, we do not rely on a control group consisting of individuals from other sectors or regions, since both treated and control are spouses working in the government sector.

Results show that a higher benefit level for the partner did not affect a spouse's probability to start a new sick spell, but prolonged ongoing spells with on average 4 percent.

---

[1] In addition there are five US States, including California, which have a TDI system. For more information about TDI, see the information provided by the Social Security Administration in the US (http://www.ssa.gov/policy/docs/progdesc/sspus/tempdib.pdf), and Kerns (1997).

[2] A higher benefit level is generally found to decrease individual labor supply through increased sick reporting (Johansson and Palme 1996, 2002, 2005, Henrekson and Persson 2004, Pettersson-Lidbom and Skogman Thoursie 2008).

[3] There is a related literature dealing with early retirement decisions within the family. See for example, Baker (2002), Hesselius (2004), Kapur and Rogowski (2007), and Gustman and Steinmeier (2004). Moreover, worker absence and productivity are found to depend on the productivity and absence level of peers at work (see e.g.,

For local government workers, where the size of the change in benefit is known, the spousal elasticity of sick days with respect to the partner's benefit is estimated to 0.4. This can be compared to labor supply estimates from other social insurance programs such as the Unemployment insurance and the Workers' compensations, where own labor supply elasticities range between 0.5 to 1 (Krueger and Meyer 2002). Furthermore, Pettersson-Lidbom and Skogman Thoursie (2008) find a large direct effect on the total number of sick days for the same reform that we are examining in this paper. Back-on-the-envelope calculations suggest that our estimated spill-over effect adds at least another 2 percent to the total reform effect and as much as 18 percent of the total reform effect stems from spill-over effects. So, by ignoring spill-over effects, an evaluation of labor supply effects among married workers from changed insurance coverage will underestimate the total effect on individual behavior.

There are various theoretical explanations why the insurance coverage for the partner indirectly affects spousal labor supply.[4] First, for couples with a common household budget, an increase in the partner's benefit level reduces the cost of future unexpected events such as increased sickness absence by the partner. This *insurance effect* would unambiguously increase spousal sick reporting, regardless of whether or not the partner reacts to the reform.[5] Second, the partner's benefit level might also affect household income and change spousal sick reporting through an *income effect*. Even if absenteeism is a normal good, the income effect with less than full compensation is, however, a priori unknown because the increased benefit level could be offset by a reduced income through the partner's reduced labor supply (an income effect can also occur if the partner receives lower future wages due to an increased sick reporting).

In the presence of direct effects on the partner's sick reporting behavior, there are additional reasons why spousal sick reporting might change. If spouses are substitutes in household production, spousal absenteeism can decrease if the partner carries out more

---

Bandiera et al. 2005, 2007, Mas and Moretti 2009 and Hesselius et al. 2009). To study social interaction between spouses compared to between co-workers has the advantage that couples more clearly define the peers of interest.

[4] We will use the terms labor supply and sick reporting interchangeably in this paper.

[5] This is related to the added-worker effect in the unemployment insurance literature where the incentive for the spouse to increase labor supply when the partner becomes unemployed diminishes with the partner's unemployment benefit level (Ashenfelter 1980, Heckman and MaCurdy 1980, and Lundberg 1988). Cullen and Gruber (2000) find that the labor supply of wives to unemployed husbands is affected by the husbands' unemployment insurance.

household work when staying home. On the other hand, if couples have a demand for joint leisure, an increased benefit level for one of them might enhance such behavior. Taking care of the partner who is ill and fairness (i.e. if one partner stays home from work, then the other also wants to stay home) are other, but perhaps less obvious, explanations belonging to this joint leisure hypothesis.

To empirically disentangle these types of work disincentives effects has been proven difficult (Autor and Duggan 2007). For example, eliminating an income effect by controlling for the partner's income relies on a heroic assumption since the partner's income is affected by the reform itself. It is tempting to eliminate the joint-leisure hypothesis by estimating the spill-over effect on a sub-sample of couples who never had any overlapping sick cases to check if the spill-over effect is still present. Once again, this would rely on a sample potentially selected based on the outcome variable. However, the problem is likely to be of a second order since almost all sick cases for the spouse – 91 percent – never overlapped with the partners'. With this discussion in mind we try to separate out an insurance effect from the total spill-over effect. Explicitly stating the assumptions required for this to have a causal interpretation, we tentatively conclude that at least a part of the estimated spill-over effect consists of an insurance effect.

The rest of the paper is organized as follows. In Section 2 we discuss the reform and in Section 3 we describe the data. In Section 4 graphical results are presented and the identification strategy is discussed. Results are presented in Section 5. Finally we conclude in Section 6.

2 The reform

The basic idea with a TDI is to compensate for the economic loss at times of non-worked related temporary sickness or disability. As of today, Sweden has a compulsory publicly administered TDI program and it is funded primarily through a payroll tax levied on employers. It covers all workers whose employers pay the payroll tax. The employer pays sick pay (80 percent of previous earnings up to a ceiling) from day 2 to day 14. From the 15th day of sickness the insurance scheme provides sickness benefits. For the majority of workers, collective agreements top-up the replacement rate from the public system. Thus, to compute the potential benefit replacement rate of an individual worker one must take into account both the TDI benefits and the paid sick leave from the collective agreements. A physician's certificate is required from the eighth day which, in practice, gives the worker full discretion of claiming benefits the first seven days. The reform we will use in this paper is from December 1, 1987. Therefore we will describe more in detail how the insurance system worked before and after that date.

The aim of the 1987 reform was to increase the benefit replacement rate to 90 percent up to a given income cap for spells lasting less than a week (see, e.g., Proposition 1986/87:69 and Ds S 1986:8).[6] The reason for the change was that some type of workers only received a relatively small fraction of their previous income if they were only sick for a short period. This fact was considered to be unfair by policymakers (SOU 1981:22 and SOU 1983:48) and in December 18, 1986, the government decided to increase the replacement rate for short-term disability. In addition, the one-day-waiting period was abolished. The new TDI law came into force December 1, 1987.

To understand how the reform affected the benefit levels for different categories of workers we need to elaborate on the Swedish labor market. At that time, the workers where basically employed in three different sectors: workers employed by the *central government* (16 percent), workers employed by the *local government*s (39 percent), and workers employed in the private sector (45 percent). All workers except for central government workers were affected by the reform. For workers in the private sector we are unable to compute the exact replacement rate change due to the lack of information about their job characteristics and their collective agreements.[7] Central government workers were not affected by the reform since the central government took advantage of the Social Security Act (1962:381) which made it possible for an employer (the central government in this case) to provide paid sick leave to its workers while at the same time the TDI benefits that the workers were entitled to were paid out to the employer instead.

[6] The changes occurred during the first week of a sick case so the difference in total compensation before and after the reform is decreasing for each day that a spell exceeds the 7th day. To phrase it more "economically" – asymptotically the reform did not change the compensation for a sickness spell (but perhaps luckily, as Angrist and Pischke (2009) points out, "real life does not play out in asymptotia"; Angrist and Pischke 2009, p.209).

[7] Due to the pre-reform rules of TDI, the replacement rate for a blue-collar worker in the private sector could depend on a number of factors such as whether she worked part time or full time, whether she had irregular working hours, whether she was a shift worker etc. (see the government report Ds S 1986:8). White-collar workers in the private sector received benefits from the public system but they were also entitled to paid-sick leave from their employers as

As a result, the cash sickness benefits for central governments workers were 92 percent of current earnings both before and after the reform. In addition, the cash benefits were paid from the very first day of temporary sickness so in contrast to the TDI program there was no waiting period for central government workers.

Local government workers also received TDI benefits from the public system but in addition they received paid sick leave from their employers as a result of collective agreements between the unions and the employer's federations. Consequently, the calculation of the effective replacement rate must take into account both sick leave pay and TDI benefits. Before the reform, the effective wage replacement rate was 90 percent from the first day of temporary disability since the local government workers received paid sick leave from their employers in addition to the TDI. After the reform the local government workers received full wage replacement (100 percent) from the first day due to the change in the TDI system together with a simultaneous change in collective agreements. Thus, the percentage change in the replacement rate for local government workers was 11 percent.

Everyone in the working population in Sweden received a letter from the Swedish Social Insurance Agency (previously known as the National Insurance Board) a couple of months before December 1, 1987, which provided detailed information about the reform. The letter also stated that all workers were required to provide information about their number of working days per year in order for them to get the benefits. The reform was also extensively covered in the media: both by the public television and by all the newspaper. Consequently, the reform was very well-known and, therefore, we should expect noticeable labor supply effects.

3 Data description

Information on sickness absence is collected from the Swedish National Insurance Board and covers start and end dates for all spells in Sweden for the period 1986-1990. The sickness data are matched with information from LINDA, a comprehensive and

a result of collective agreements between the unions and the employers. Unfortunately, we are unable to compute a replacement rate for white-collar workers in the private sector due to the complexity of collective agreements.

representative data set covering 3.3 percent of the Swedish population.[8] LINDA contains information on a sampled person's household members and the marital status of each of them. We define two persons to be a married couple if both are reported as married and registered in the same household. We limit the study to married persons employed by the central government aged 20–64 with a yearly income above SEK 6000.[9] Information whether the spouse is a wife or a husband, lives in an urban area and the age is also gathered from LINDA. Treated are defined as spouses married to *non-governmental* workers (directly affected by the reform) and spouses married to *governmental* workers (not affected the reform) are used as controls. The sample consists of 89,773 married persons, who in total have 167,714 sick spells.

Summary statistics for the total sample are presented in Panel A of *Table 1*. Panel B provides summary statistics for individuals with positive spells.[10] On both panels the sample compositions between the groups are similar except that wives tend to more common in the control group. No evidence is found for any relative change in group composition over time between the groups; this holds for both the individual sample and the sample with positive spells. Further, the sickness incidence seems not to have been affected by the reform, while the duration seems to have been affected. Spouses to partners who received a higher benefit level increased their average duration with 0.36 days. Importantly, we cannot find any evidence that the relative increase in average duration in the treatment group is a result of a compositional effect rather than a reaction to the reform.

[8] See Edin and Fredriksson (2000) for a general description of LINDA.
[9] Workers with less than SEK 6000 cannot claim benefits from the TDI.
[10] Sick spells are right-censored at 50 days. The reason is that spells with long duration amount to a very small part of the entire distribution with only 2.4 percent lasting more than 50 days. The relative generosity of the Swedish TDI during the late 1980's makes some long spells appear more like cases for the disability insurance rather than for the temporarily disability insurance (the longest case that started during our analyzed period lasted 992 days). When calculating group-time averages, these long durations add up to a large share of the total number of compensated sickness days, even though they only represent a very small part of the total distribution of sickness cases.

Table 1. Summary statistics: Average values for treatment and control group before and after the reform

	Individual Sample				
	Treatment		*Control*		
	After	Before	After	Before	DD
Panel A	(1)	(2)	(3)	(4)	(1)-(2)-[(3-(4)]
Yearly incidence of a sick spell	1.87	1.77	2.04	1.94	-0.00
	(2.24)	(2.11)	(2.28)	(2.17)	(0.03)
Share with at least one spell per year	0.65	0.64	0.68	0.67	0.00
	(0.78)	(0.48)	(0.47)	(0.47)	(0.01)
Family size	3.44	3.45	3.36	3.34	0.02
	(1.13)	(1.08)	(1.08)	(1.09)	(0.01)
Wives	0.34	0.34	0.50	0.50	0.00
	(0.47)	(0.47)	(0.50)	(0.50)	(0.01)
Urban	0.35	0.36	0.38	0.37	-0.01
	(0.48)	(0.48)	(0.48)	(0.48)	(0.01)
Age	43.52	43.18	43.45	43.72	0.07
	(8.69)	(8.82)	(8.94)	(8.67)	(0.11)
Individuals	*40,483*	*28,604*	*12,008*	*8,678*	

	Positive Spells				
	Treatment		*Control*		
	After	Before	After	Before	DD
Panel B	(1)	(2)	(3)	(4)	(1)-(2)-[(3-(4)]
Duration	5.90	5.82	5.83	6.13	0.36
	(9.55)	(9.52)	(9.52)	(9.97)	(0.14)
Family size	3.44	3.44	3.36	3.36	-0.00
	(1.11)	(1.06)	(1.09)	(1.07)	(0.02)
Wives	0.43	0.42	0.58	0.59	0.01
	(0.49)	(0.49)	(0.49)	(0.49)	(0.01)
Urban	0.39	0.41	0.40	0.42	0.00
	(0.49)	(0.49)	(0.49)	(0.49)	(0.01)
Age	42.52	42.05	42.77	42.49	0.18
	(8.67)	(8.80)	(8.63)	(9.17)	(0.18)
Spells	*75,731*	*50,650*	*24,519*	*16,814*	

Notes: Columns (1)–(4) report standard deviations in parentheses. For the DD-estimates, OLS standard errors clustered on individuals are displayed in parentheses.

4 Graphical analysis and identification strategy

The goal of this paper is to identify whether the insurance coverage in the social insurance system triggers indirect effects. In order to do so, we compare sick reporting for central government workers who are married with partners who received an increased benefit level with those government workers whose partner did not receive an increase after the 1987 reform. Sick reporting can only take non-negative values and can therefore be analyzed in two ways. First, one can estimate the reform effect on the probability that a spousal sick spell occurs. Second, given that a spousal sick spell has occurred we might be interested in the reform effect on the duration of such a sick spell. It is straightforward to estimate the probability that a sick spell occurs provided that spouses married to governmental workers constitutes a valid control group. Estimating the effect on duration is problematic, however, if there is a reform effect on the occurrence of a sick spell. The reason is that spouses who start a sick spell due to the reform are potentially selected and might have a differential sick reporting behavior over time than the control for other reasons than the reform. Analyzing the indirect reform effect on duration is only possible if the reform has no effect on the incidence of spousal sick reporting. We therefore begin the analysis by estimating the change in probability that a sickness spell exceeds a given length between the treated and control group, before and after the reform, based on the following linear probability difference-in-differences (DD) model:

$$P(Y_{igt} \geq s) = \alpha + \lambda_t + \pi D_{gt} + \delta_s(D_{gt} \times Post_t) + u_{igt} \qquad (1)$$

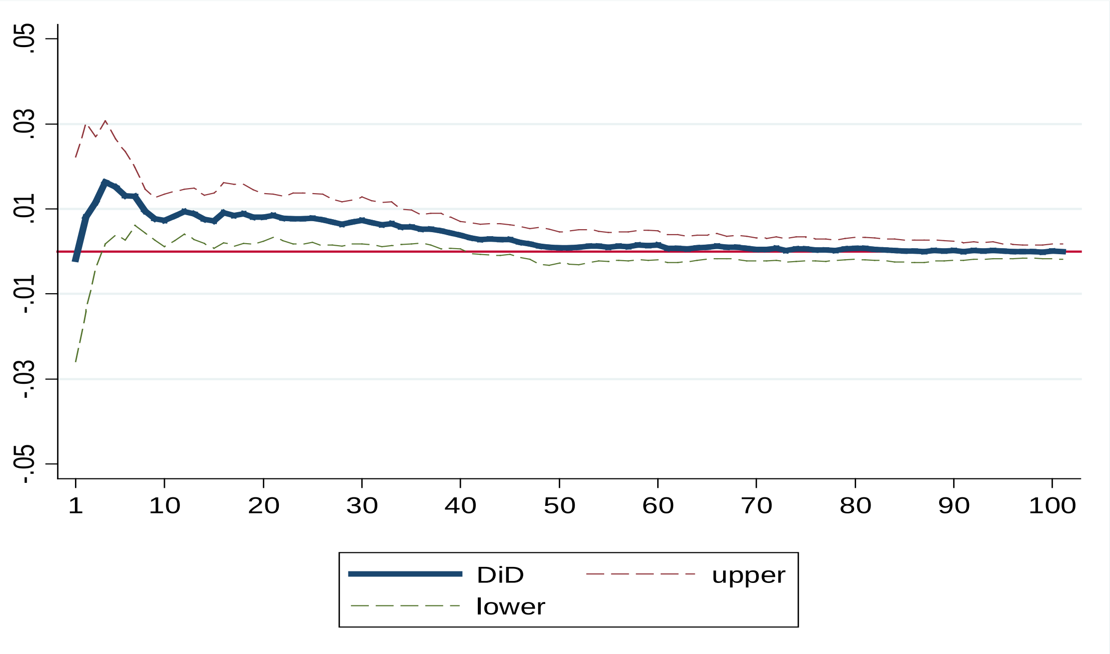
where Y_{igt} is a binary variable taking the value 1 if individual i belonging to group g ($g=1$ if treated) at time t has a sick spell of s days or longer ($s=1, 2, \ldots, 100$). Time effects are represented by λ_t (t is here defined as half-years), D_{gt} is a dummy variable taking the value 1 if the individual is married to a non-governmental worker (treated) and 0 otherwise, and $Post_t$ is a dummy variable taking the value 1 from December 1987. The variable of interest is the interaction between the treatment dummy variable and the post-reform dummy, where δ_s measures the average reform effect on the probability that sick spells are at least s days on average. Note that this DD estimate is obtained for each length of a sick spell.[11]

[11] This is the same as to estimate the change in 1-CDF.

Figure 1 shows all estimates for durations between 1 to 100 days combined. The figure reveals two important insights. The first insight is that the reform had no impact on the incidence, that is, the probability to start a new sick spell did not change in the treatment group relative the comparison group as a reaction to the reform. This is seen at length equal one where the DD estimate is not statistically different from zero. The second insight is that the reform affected the duration of ongoing spells up to around 40 days, where a clear pattern is seen; the effect is decreasing in magnitude from day three and vanishes at around the 40th day. This suggests that the effect of the partner's benefit level diminishes with the length of the spouse's sick cases.

Why do we observe an effect for the duration and not for the incidence? An explanation could be that the employee might perceive starting a new sick spell as a worse signal to the employer compared to continue an ongoing spell with an additional day, implying less promotion and career opportunities. This is the case if it is more costly for the employer to adjust work plans and find a substitute for a new sick case than for a person who has already been on sick leave for a while.

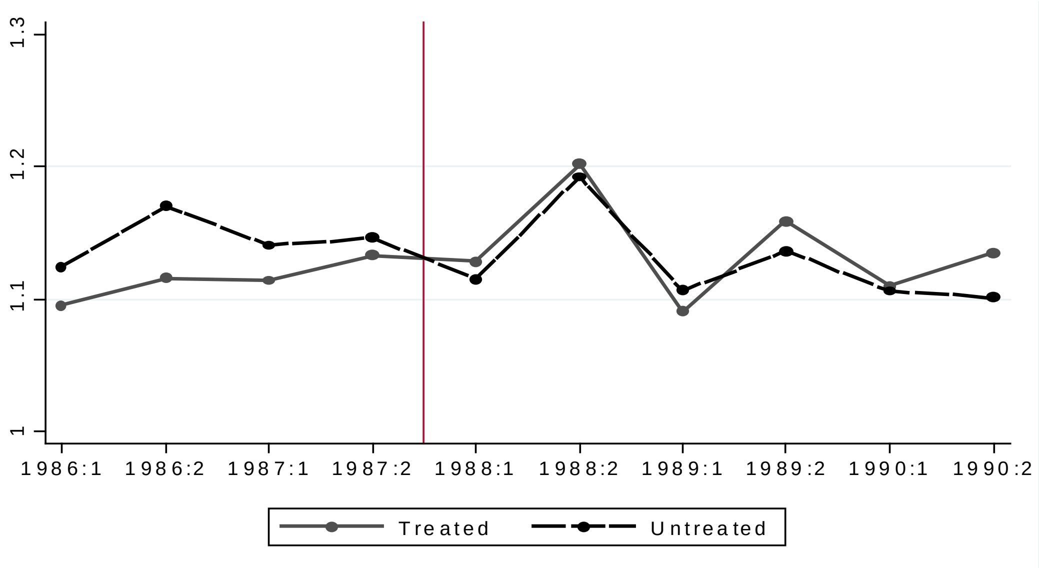
Figure 1. Estimated change in 1-CDF with a 95 percent interval. Half year data



Note: The figure shows the change in probability that a sickness spell exceeds a given length (on the x-axis) between the treated and control group before and after the reform. Standard errors are clustered on a time-group level.

On the basis of the above results we explore the effect on duration more in detail. The half year development of average length for sick cases in the treatment and the control group are shown in *Figure 2,* measured in logarithmic form. During 1986 and 1987, the two years before the reform, central government workers married to other central government workers (the control group) had on average longer spell lengths than central government workers married to non-central government workers (the treatment group). Even if average durations are different between the groups, the trends are very similar up until 1987 which supports the parallel trend assumption.[12] Furthermore, average duration is relatively low the first half year while for the second half it is relatively high, revealing a seasonal pattern. In the years 1988 to 1990 the seasonal pattern is sustained but now with a big difference: the treatment group, that before the reform had an on average shorter spell length, now have a longer spell length than the control group. This shift is striking and indicates that an increased insurance coverage affects spousal labor supply negatively.

Figure 2. Average log duration: Comparing treatment and control group for the period 1986-1990



[12] Another issue, discussed by Athey and Imbens (2006), is that the DD-approach can be sensitive to the functional form assumption when treated end controls have different outcome levels prior to the reform. Unless the time trend is zero for the two groups, the parallel trend assumption can never hold for both levels and logs. Since there seems to be no trends for treated and controls we also examine the development of average duration in levels for the treatment and control group. The results are robust to this change.

The next question is whether the graphical results hold if we apply a statistical analysis to test whether the effect is significant. Using individual cross-sectional data for the period 1986-1990, the following linear difference-in-differences (DD) model can be formulated:

$$Y_{igt} = \alpha + \lambda_t + \beta X_{it} + \pi D_{gt} + \delta(D_{gt} \times Post_t) + \varepsilon_{igt} \qquad (2)$$

where Y_{igt} is sick duration for individual i belonging to group g ($g=1$ if treated) at time t. The vector X_{it} includes individual characteristics (family size, age and two binary variables indicating if the spouse is a wife and whether living in an urban area). Although summary statistics in *Table 1* did not indicate any compositional changes over time for the groups, these variables are still included in order to control for any observed changes in compositions between the two groups, and in order to improve precision of the reform effect. Once again, the variable of interest is the interaction between the treatment dummy variable and the post reform dummy where δ measures the average reform effect on spousal duration.

One main concern when estimating the effect with a regression model are intra-class correlation potentially leading to false inference. When individuals within certain groups are correlated, OLS standard errors might be grossly understated if the regressor of interest varies only at the group level (Moulton 1986). This means that OLS standard errors from equation (2) are downward biased if observations within treatment-time are correlated. To solve this problem we model correlation within group-time by assuming that the error term also consists of a group-time error, such that $\varepsilon_{igt} = \upsilon_{igt} + \eta_{gt}$, where η_{gt} is a random error component specific to group g in time t (both errors are assumed to be homoscedastic and η_{gt} are uncorrelated across group-years). Using clustered standard errors is not appropriate in this case since that would require a large number of group-times. Instead, we apply the two-step approach suggested by Donald and Lang (2007). In the first step we construct covariate adjusted group-year effects by estimating:

$$Y_{igt} = \mu_{gt} + bX_{it} + \upsilon_{igt} \qquad (3)$$

where $m_{gt} = \lambda_t + \alpha D_{gt} + \delta(D_{gt} \times Post_t) + \eta_{gt}$. The estimated group-time effects, $\hat{m}_{gt,}$ are group-time means of the outcome adjusted for individual variables. In the second step, we regress these estimated group-time effects on all variables that vary at the group and time levels using the following equation:

$$\hat{m}_{gt} = \alpha + \lambda_t + \pi D_{gt} + \delta(D_{gt} \times Post_t) + u_{gt} \tag{4}$$

where $u_{gt} = \eta_{gt} + (\hat{m}_{gt} - m_{gt})$. Since this equation is formulated at the group-time level, correlated errors within group-years are taken into account. As pointed out by Donald and Lang (2007), homoscedasticity of u_{gt} is a natural assumption when the number of observations in each group is large, which is true in our case. This point demonstrates that in many circumstances, the most efficient estimator is the unweighted OLS estimator. When estimating the treatment effect we difference equation (4) across spouses married to government and non-government workers, respectively, and run OLS on the following equation:

$$\Delta \hat{m}_t = \pi + \delta Post_t + \Delta u_t \tag{5}$$

where $\Delta \hat{m}_t = \hat{m}_{1t} - \hat{m}_{0t}$ (Δu_t is analogously defined and assumed to be independent and identically distributed). This estimation is based on T time observations and is equivalent to a group fixed-effect model.

The nature of the sickness data allows us to estimate equation (5) with different time intervals.[13] The above figures and the estimation of equation (1) relied on half year data, but in order to get more degrees of freedom we would like to use a more disaggregated level. At the same time we do not want the result to depend on this choice. Hence, data on different time intervals is used to estimate equation (5); yearly, half yearly, quarterly and monthly. The most conservative is yearly data, using variation from only 5 obser-

[13] But recall that data from LINDA varies annually.

vations, with half yearly variation we end up with 10 observations, with quarterly 20 observations, and with monthly 60 observations.[14]

5 Results

Results from estimations based on equation (5) are shown in *Table 2*. All models reveal that the sickness duration for married central government workers increased on average with 4 percent as a reaction to the partner's increased replacement rate. Conditioning on age, whether the spouse is a wife, living in an urban area and household size does hardly change the estimated reform effect. That the reform effect is highly significant for all models and changes remarkably little when utilizing different time intervals or controlling for covariates imply that the estimate can be interpreted as causal.[15]

The largest treatment effect is estimated with quarterly data with or without controlling for covariates. We choose to take a conservative route and use monthly observations for the rest of the analysis which also leaves us with most degrees of freedom.

Table 2. Reform effect on log durations, with and without controlling for age, wives and household size

	Year	*Half Year*	*Quarterly*	*Monthly*
Effect	0.042**	0.042*	0.044*	0.042*
	(0.010)	(0.011)	(0.012)	(0.013)
Effect with controls	0.041	0.041*	0.043*	0.040*
	(0.019)	(0.010)	(0.011)	(0.013)
Observations	*5*	*10*	*20*	*60*

*/**/*** indicates a 1/5/10 percent significance level. OLS standard errors in parentheses are used since they are larger than White's heteroscedasticity robust standards errors.

To investigate the dynamics of the reform effects and in order to evaluate the parallel trend assumption more formally than by inspection of *Figure 2*, we estimate half year treatment effects based on the following model:

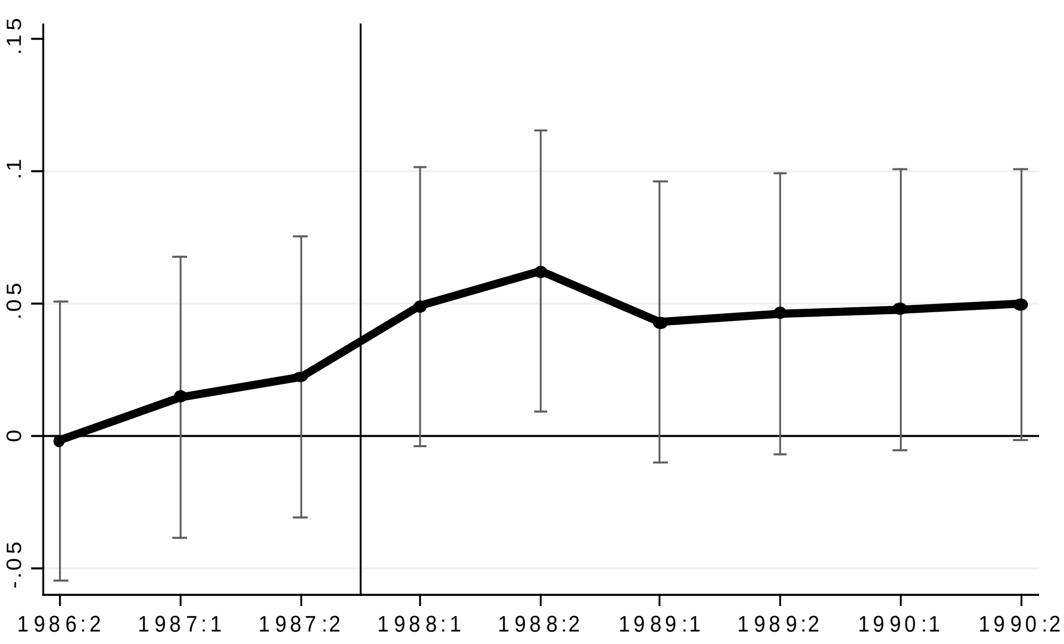
[14] To use a higher aggregation level is also a remedy for serial correlation, so by comparing results derived from data on different time intervals we can get an indication of whether it is a problem or not. Another way to correct for serial correlation is to use Newey-West standard errors. These are robust to arbitrary heteroscedasticity and serial correlation. When applying this strategy all standard errors are found to be even smaller.

$$Y_{igt} = \alpha + \lambda_t + \beta X_{it} + \pi D_{gt} + \sum_{h=2}^{H} \delta_h (D_{gt} \times Half_h) + \varepsilon_{igt} \qquad (6)$$

where $Half_h$ are half year dummy variables and δ_h are half year treatment effects. To account for correlated errors within group-time we apply the two-step approach as describe above. In order to have enough degrees of freedom when estimating half year effects on data at group-time levels we use month as the underlying time dimension.

Figure 3 reveals what was earlier seen in *Figure 2*; the treatment and the control group have similar trends in average duration up to the reform date. After the reform, duration increases for the treatment group relative the control group. Interestingly, the reform is found to have had a gradually increasing effect on spousal sickness duration during the first year after it was implemented and it then stabilizes on a somewhat lower level the following two years.

Figure 3. Half year treatment effects with 90-percent confidence interval.



[15] The concordance of the results using different time intervals show that serial correlation is not a problem and is not making us draw too strong inference.

5.1 Quantifying the effect

One way to quantify the estimated reform effect is to calculate an elasticity of spousal sick reporting with respect to the partner's benefit. As mentioned above, we are not able to calculate the exact change in benefit for all workers in our sample, but for local government workers we know that the replacement rate increased from 90 to 100 percent from the first day of sickness.[16] In 1987, 39 percent of the workforce was employed in this sector. The estimated reform effect for local government workers is 4.6 percent, implying an elasticity of 0.4.[17] Since no effect were found for the incidence this is to be interpreted as the total elasticity of spousal sick reporting with respect to the partner's benefit. In comparison to estimated labor supply elasticities for other social insurance programs this is non-negligible.

Another way to quantify the indirect effect is to relate it to the estimated effect from Pettersson-Lidbom and Skogman Thoursie (2008), who found that the total reform effect for the directly affected non-governmental workers added 924,000 new sick cases per year (77,000 per month). With an average length of a sick spell for non-governmental workers of 6.8 days, this corresponds to almost 6.3 million new sick days. To be able to calculate how the spill-over effect among governmental workers adds to the previously estimated total effect we perform a back-on-the-envelope calculation. In 1987, the workforce was around 4 million who approximately had 8.1 million sick cases. Married central government workers constitute 7.2 percent of the work force, i.e., 288,000 workers. Assuming an equal incidence rate across sectors, central government workers therefore had 583,000 sick cases. The indirect increase in spousal sick duration due to the reform estimated in this paper was 4.2 percent which corresponds to 0.25 more days per sick case (5.82´ (1.042-1)). With no effect on the incidence, the 583,000 sick cases reported in 1987 should add almost 146,000 new sick days as an indirect effect to the reform per year. The estimated indirect spill-over effect therefore adds more than 2 percent to the total effect of 6.3 million new sick days estimated by Pettersson-Lidbom and Skogman Thoursie (2008).

[16] For this sub-sample we have full compensation after the reform which implies that the income effect is either zero or positive.

Another question is how large fraction of the previously estimated total effect is due to a spill-over effect among couples? If we are willing to assume that indirect effects are equally large and common among couples where both partners were directly affected as among couples where only one partner where directly affected, spill-over effects between couples will amount to as much as 18 percent of total reform effect. This figure is based on the following calculation; 2 million of the work force were married and directly affected by the reform (84 percent work in the non-governmental sector and 60 percent were married). If those 288,000 indirectly affected workers generated 146,000 more sick days as a reaction to the reform, then those 2 million workers directly affected should have generated more than 1 million indirect days ((2,000,000/288,000)´ 146,000). Thus, indirect effects are around 1,000,000 plus 146,000 days of a total of 6,300,000 plus 146,000 days. In this perspective, indirect effects are non-negligible.

5.2 Possible mechanisms

As discussed earlier, the indirect effect can be an outcome of three different sources; an insurance effect, an income effect and a joint leisure effect. In this section we will provide additional analyses and explanations to be able to learn more about the relative importance of these effects. The focus will be on the treatment group consisting of spouses to local government workers as we know the exact change in replacement rate for this group and for which the income effect is weakly positive as the replacement rate changed from 90 to 100 percent. In addition, this group experienced no change regarding the day of notice, thereby lowering any income effect.

In general, disentangling work disincentives effects, for example the existence of an insurance effect is very difficult. For instance, it is not possible to eliminate a potential income effect by controlling for the partner's income in the regression model. The problem being that the income for an absent partner is affected by the reform, so including it in the regression model will create a selection bias for which the sign is ambiguous and most likely non-monotonic over the income distribution. Nevertheless, assuming

[17] For workers above the income cap the change in replacement rate is smaller. This group is less than 5 percent of the sample and the estimated reform effect is basically unchanged when they are excluded.

the bias to be negligible, the effect (applying the two step approach with covariates) is found to be 2.7 percent and highly significant when we exclude the income effect. This should be compared with an effect of 4.2 percent when not including the partner's income. The decrease in magnitude is consistent with a non-negative income effect which we know is the case for local government workers. The assumption of a negligible selection bias can be relaxed if our main concern is to analyze if an insurance effect is of importance or not. For that purpose, the assumption we need to make is that the selection bias is weakly negative, so the estimate of 2.7 percent is too small. Moreover, the joint leisure effect is likely to be of less importance as more than 90 percent of all spells did not overlap with the partner's spell, and by ignoring spells that did, the reform effect is unchanged and highly significant (this holds with and without controlling for covariates).

Another question is if there are differential effects depending on the partner's income level. Since spousal labor supply is one way for families to self-insure against adverse economic shocks, the need for this insurance might not be the same for high and low income families. For low income families, the change in the partner's TDI benefit level might play a more important role for the total family income than what is the case for high income families. As such, effects of changes in the partner's TDI benefit level would be more important among low income families. To investigate this we divide the sample into two sub-samples: central government workers having partners with yearly incomes above and below the median, respectively. This procedure is equivalent and relies on the same assumptions as when the partner's income is included in the regression model. The results support the idea that spouses of low income partners reacted more to the reform than those to high income partners (6.7 percent versus 3.9 percent). This pattern is sustained when applying the two-step approach.

So, what have we learnt from these exercises? The results should be interpreted with caution as they are likely to be biased, but we believe that they still contain some valuable information. First, results indicate that sickness insurance for a partner is to some extent influencing the spouse's sickness behaviour through an insurance effect. Second, the income change for partners induced by the reform appear to have affected spousal behaviour and we find support for that low income families reacted more on the reform relative high income families. Third, the estimated effect is not driven by couples' using the TDI system to spend more time together.

6 Conclusion

In this paper we investigate whether the partner's social insurance coverage affects spousal labor supply. Exploiting a reform in sickness insurance system in 1987 that increased the benefits for workers in the non-government sector, we compare the change in sick-reporting for married central government workers based on whether their partner received an increased coverage or not. The results show that a higher replacement rate for the partner did not affect the spouse's probability to start a new sick spell, but prolonged ongoing spells with on average 4 percent. Using a sub-sample where the size of the change in the partners' replacement rate is known, the spousal elasticity of sick days with respect to the partner's benefit is estimated to 0.4. Back-on-the-envelope calculations suggest that the estimated spill-over effect in this paper adds another 2 percent to the previously estimated reform effect. Moreover, part of the previously estimated reform effect might also include indirect effects. Our calculations suggest that 18 percent of the total reform effect is such spill-over effects.

Various theoretical explanations as to why the partner's insurance coverage affects spousal labor supply are discussed. An increase in the partner's benefit level reduces the costs of future unexpected events such as increased sickness absence by the partner. This is a so called *insurance effect* and might exist regardless of whether the partner reacts to the reform or not. The partner's benefit level might also affect household income and change spousal sick reporting through an *income effect*. Other explanations are related to couples having a demand for joint leisure. To empirically disentangle these types of work disincentives effects is difficult. Based on a heroic assumption we take account of the income effect by controlling for the partner's income and find that the insurance effect is present and influences spousal labor supply. Moreover, for the sample of couples who have no overlapping sick cases – which is the vast majority – the estimated reform effect is of the same size as the total reform effect, suggesting that the joint leisure hypothesis is of less importance. From these analyses we tentatively conclude that at least a part of the estimated spill-over effect consists of an insurance effect.

If one is not willing to rely on the assumptions made to separate out an insurance effect, we still conclude that spouses' pool their labour supply since they react to each others' insurance coverage. A more general conclusion is that if policy evaluations fail to take indirect effects into account they will notoriously underestimate the total effect of the reforms.

References

Angrist, Joshua D., and Jörn-Steffen Pischke. 2009. Mostly Harmless Econometrics: An Empiricist's Companion. Princeton, Princeton University Press.

Ashenfelter, Orley E. 1980. "Unemployment of Disequilibrium in a Model of Aggregate Labor Supply." *Econometrica*, Vol. 48, No. 4, pp. 547-64.

Athey, Susan, and Guido W. Imbens. 2006. "Identification and Inference in Nonlinear Difference-In-Differences Models." *Econometrica*, Vol. 74, No. 2, pp. 431-97.

Autor, David H., and Mark G. Duggan. 2007. "Distinguishing Income from Substitution Effects in Disability Insurance." *American Economic Review*, Vol. 97, No 2, pp. 119-24.

Baker, Michael. 2002. "The Retirement Behavior of Married Couples: Evidence from the Spouse's Allowance." *Journal of Human Resources*, Vol. 37, No. 1, pp. 1–34.

Bandiera, Oriana, Barankay, Iwan, and Imran Rasul. 2005. "Social Preferences and the Response to Incentives: Evidence from Personnel Data." *Quarterly Journal of Economics*, Vol. 120, No. 3, pp. 917–62.

Bandiera, Oriana, Barankay, Iwan, and Imran Rasul. 2007. "Social Incentives in the Workplace." unpublished.

Cullen, Julie B., and Jonathan Gruber. 2000. "Does Unemployment Insurance Crowd out Spousal Labor Supply?" *Journal of Labor Economics*, Vol.18, No. 3, pp. 546-72.

Donald, Stephen G., and Kevin Lang. 2007. "Inference with Difference in Differences and Other Panel Data." *Review of Economics and Statistics*, Vol 89, No. 2, pp. 221-33.

Ds S 1986:8. "Förbättrad kompensation vid korttidsjukdom och vid tillfälligt vård av barn".

Edin, Per-Anders, and Peter Fredriksson. 2000. "LINDA - Longitudinal INdividual DAta for Sweden," Working Paper, Uppsala University.

Gustman, Alan B., and Thomas L. Steinmeier. 2004. "Personal Accounts and Family Retirement." Working Paper. NBER. No. 10305.

Heckman. James J., and Thomas E. MaCurdy. 1980. “A Life Cycle Model of Female Labor Supply.” *Review of Economic Studies*, Vol. 47, No. 1, pp. 47-74.

Henreksson, Magnus, and Mats Persson. 2004. “The Effects on Sick Leave of Changes in the Sickness Insurance System,” *Journal of Labor Economics*, Vol. 22, No. 1, pp. 87-113.

Hesselius, Patrik. 2004, *Sickness Absence and Labour Market Outcomes,* Ph. D Dissertation, Economic Studies 82, Department of Economics, Uppsala University.

Hesselius, Patrik, Johansson, Per, and Peter Nilsson. 2009. “Sick of Your Colleagues’ Absence.” *Journal of the European Economic Association*, Vol. 7, No 2-3, pp. 583-94.

Johansson, Per, and Mårten Palme. 1996. “Do Economic Incentives Affect Work Absence? Empirical Evidence Using Swedish Micro Data.” *Journal of Public Economics*, Vol. 59, No. 2 195-218.

Johansson, Per, and Mårten Palme. 2002, “Assessing the Effects of a Compulsory Sickness Insurance on Worker Absenteeism.” *Journal of Human Resources*, Vol 37, No 2, pp. 381-409.

Johansson, Per, and Mårten Palme. 2005. “Moral hazard and sickness insurance.” *Journal of Public Economics,* Vol. 89, No. 8-9, pp. 1879-90.

Kapur, Kanika, and Jeannette Rogowski, 2007. ”The Role of Health Insurance in Joint Retirement among Married Couples” *Industrial and Labor Relations Review*, Vol. 60, No. 3, pp. 397-407.

Kerns, Wilmer L. 1997. “Cash Benefits for Short-Term Sickness, 1970-94.” Social Security Bulletin, Vol. 60, No. 1, pp. 49-53.

Krueger, Alan, B., and Bruce D. Meyer 2002. “Labor Supply Effects of Social Insurance.” In Auerbach, Alan, J., and Martin S Feldstein, eds., *Handbook of Public Economics*, Vol 3, pp 2327-2392. North-Holland.

Lindbeck, Assar. 2006. *The Welfare State–Background, Achievements, Problems*. New Palgrave Dictionary, 2nd edition.

Lundberg, Shelley J. 1988. “Labor Supply of Husbands and Wifes: A Simultaneous Equations Approach.” *Review of Economics and Statistics*, Vol. 90,No. 2, pp. 224-35.

Mas, Alexandre, and Enrico Moretti. 2009. “Peers at Work.” *American Economic Review*, Vol. 99, No. 1, pp. 112-45.

Moulton, Brent R. 1986. “Random Group Effects and the Precision of Regressions Estimates.” *Journal of Econometrics*, Vol. 32, No. 3, pp. 385-97.

Pettersson-Lidbom, Per, and Peter Skogman Thoursie. 2008. “Temporary Disability Insurance and Labor Supply: Evidence from a Natural Experiment.” Working Paper, Stockholm University.

Regeringens proposition 1987/87:69. ”Om förbättrad kompensation vid korttidsjukdom och vid tillfälligt vård av barn.”

Statens offentliga utredningar (SOU) 1983:48.

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

Rapporter/Reports

2009:20 Böhlmark Anders, Oskar Nordström Skans and Olof Åslund ”Invandringsålderns betydelse för social och ekonomisk integration”

2009:21 Sibbmark Kristina ”Arbetsmarknadspolitisk översikt 2008”

2009:22 Eliason Marcus ”Inkomster efter en jobbförlust: betydelsen av familjen och trygghets–systemet”

2009:23 Bennmarker Helge, Erik Grönqvist and Björn Öckert ”Betalt efter resultat: utvärdering av försöksverksamhet med privata arbetsförmedlingar”

2009:24 Hensvik Lena, Oskar Nordström Skans and Olof Åslund ”Sådan chef, sådan anställd? – Rekryteringsmönster hos invandrade och infödda chefer”

2010:1 Hägglund Pathric ”Rehabiliteringskedjans effekter på sjukskrivningstiderna”

2010:2 Liljeberg Linus and Martin Lundin ”Jobbnätet ger jobb: effekter av intensifierade arbetsförmedlingsinsatser för att bryta långtidsarbetslöshet”

2010:3 Martinson Sara ”Vad var det som gick snett? En analys av lärlingsplatser för ungdomar”

2010:4 Nordström Skans Oskar och Olof Åslund ”Etnisk segregation i storstäderna – bostadsområden, arbetsplatser, skolor och familjebildning 1985–2006”

Working papers

2009:21 Åslund Olof, Anders Böhlmark and Oskar Nordström Skans “Age at migration and social integration”

2009:22 Arni Patrick, Rafael Lalive and Jan C. van Ours ”How effective are unemployment benefit sanctions? Looking beyond unemployment exit”

2009:23 Bennmarker Helge, Erik Grönqvist and Björn Öckert “Effects of outsourcing employ–ment services: evidence from a randomized experiment”

2009:24 Åslund Olof, Lena Hensvik and Oskar Nordström Skans “Seeking similarity: how immigrants and natives manage at the labor market”

2009:25 Karlsson Maria, Eva Cantoni and Xavier de Luna “Local polynomial regression with truncated or censored response”

2009:26 Caliendo Marco “Income support systems, labor market policies and labor supply: the German experience”

2009:27 Brewer Mike “How do income-support systems in the UK affect labour force participation?”

2009:28 Gautier Pieter A. and Bas van der Klaauw “Institutions and labor market outcomes in the Netherlands”

2009:29 Brugiavini Agar “Welfare reforms and labour supply in Italy”

2009:30 Forslund Anders "Labour supply incentives, income support systems and taxes in Sweden"

2009:31 Võrk Andres "Labour supply incentives and income support systems in Estonia"

2009:32 Forslund Anders and Peter Fredriksson "Income support systems, labour supply incentives and employment – some cross-country evidence"

2010:1 Ferraci Marc, Grégory Jolivet and Gerard J. van den Berg "Treatment evaluation in the case of interactions within markets"

2010:2 de Luna Xavier, Anders Stenberg and Olle Westerlund "Can adult education delay retirement from the labour market?"

2010:3 Olsson Martin and Peter Skogman Thoursie "Insured by the partner?"

Dissertation series

2009:1 Lindahl Erica "Empirical studies of public policies within the primary school and the sickness insurance"

2009:2 Grönqvist Hans "Essays in labor and demographic economics"

2009:3 Vikström Johan "Incentives and norms in social insurance: applications, indentifications and inference"

2009:4 Nilsson Peter "Essays on social interactions and the long-term effects of early-life conditions"