Sickness insurance and spousal labour supply

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HIGHLIGHTS

• Is spousal labour supply related to the partner’s sickness insurance?
• We analyse a Swedish reform of the sickness insurance.
• A higher replacement rate for a spouse increases the partner’s sick days.

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ABSTRACT

Analysing a reform in the Swedish public sickness insurance, we find that an increased replacement rate for one spouse has a negative cross effect on the other spouse’s labour supply. The cross effects are present in the labour supply margins that workers can easily adjust. For wives of treated husbands, the total number of sick days increases on average 9.1% per month, whereas labour earnings are unchanged. The cross effect on total sick days for husbands to treated wives is 6.1% on average, with no effect on annual labour earnings. The total number of sick days and annual labour earnings for treated spouses are estimated to be unaffected by the reform, which indicates that the cross effects stem specifically from higher insurance coverage for the couples.

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1. Introduction

There is a long tradition of analysing interdependencies in the spousal labour supply (Ashenfelter and Heckman, 1974). Interdependencies in the spousal labour supply are of particular importance when designing social insurance systems that involve work disincentive effects because the impact of a policy can extend well beyond the targeted population through spousal cross effects (Alesina et al., 2006; Maurin and Moschion, 2009; Dahl et al., 2014). Because the labour supply is more responsive to social insurance benefits than to labour earnings, spousal cross effects within social insurance systems can be significant (Heckman, 1993; Krueger and Meyer, 2002).

In this paper, we analyse whether the spousal labour supply responds to changes in public sickness insurance. We use independent

1 The public sickness insurance compensates workers’ foregone earnings in case of short-term sickness absence and is known to create significant work disincentive effects. OECD countries spend an average of nearly 1% of GDP on sickness benefit payments (OECD, 2010). In countries such as the Netherlands, Norway and Sweden, expenditures exceed 3.5% of GDP. In California, the total sum of net benefits in 2005 amounted to $4.2 billion, which can be compared to the corresponding amount of $4.6 billion for unemployment insurance (Social Security Administration, 2008). Several studies have documented the labour supply effects of individuals’ sickness insurance benefits; see Henreksson and Persson (2004), Johansson and Palme (2002, 2005), Pettersson-Lidbom and Skogman Thoursie (2013), and Ziebarth and Karlsson (2010, 2013).
variation in the replacement rate for one spouse to analyse whether it has an effect on the other spouse’s labour supply. The independent variation is the result of Swedish sickness insurance reform in 1987. The reform is unique because it increased the replacement rate for workers in the non-governmental sector but left the replacement rate unchanged for workers in the governmental sector. The fact that government workers were excluded by the reform provides an opportunity to estimate causal cross effects using a difference-in-differences strategy. The idea is to analyse workers in the governmental sector married to workers in the non-governmental sector whose replacement rate was changed by the reform (the treatment group). The control group consists of married workers in the governmental sector whose spouses also work in the governmental sector; i.e., couples for whom the replacement rate for both spouses was unaffected by the reform.

From a theoretical perspective, the effect of the partner’s replacement rate in the sickness insurance on the labour supply behaviour of the other spouse is ambiguous. The labour supply and income can change for the partner who receives a higher replacement rate. We refer to these effects as direct effects. The direct effects can in turn cause several types of cross effects that have an impact on the other spouse’s labour supply.

A cross-hour effect arises if the spousal labour supply is interdependent. In the case of complementarity in non-market time between husbands and wives, for instance, if they enjoy spending time together, the direct effect and cross-hour effect from a higher replacement rate are of similar signs. However, if the non-market time of husbands and wives is substitutable, the effects have opposite signs. For example, a spouse who reports sick can shift time endowments to more home production and thereby free up time for market work for the other spouse. Updated norms are an additional cross effect that arises if a spouse perceives it as more acceptable to not work if the partner works less.

If the spousal labour supply depends on the couple’s income, a direct income effect creates a cross-income effect, as both partners react to the higher replacement rate for one of them. In regard to social insurance programmes, a specific cross-income effect occurs if the spousal labour supply reacts to the insurance coverage of the partner. With a higher sickness insurance benefit level, expected household income increases because the costs of future illnesses decrease. Thus, the spousal labour supply becomes less important as an insurance for future income shocks, a feature recognised in the added worker literature (see for instance Mincer (1962), Lundberg (1985) and Cullen and Gruber (2000)). Thus, when one spouse receives a higher benefit level, the other spouse can reduce his/her labour supply even if there is no direct income effect.

We use information on start and end dates for all sick spells in Sweden registered by the Swedish National Insurance Board matched with Longitudinal Individual Data (LINDA), which constitutes a 3.3% representative sample of the Swedish population and includes register information on annual earnings and demographic characteristics. The sickness data are unique, as they contain the universe of all sick spells in Sweden during our analysed period from 1986 to 1991. We measure labour supply in two ways. The first measure is sick reporting, for which we analyse the probability of starting sick leave and the total number of sick days. The second measure of labour supply is annual labour earnings. The advantage of using sickness absence as a labour supply measure is that it is a margin that workers can easily adjust. In comparison to contracted hours, which take a long time to adjust, a worker can immediately adjust his or her labour supply by calling in sick from work. For directly affected partners, our results indicate that a higher replacement in the sickness insurance induced by the reform in 1987 increased the number of sick spells but decreased the average duration of a spell. These two effects cancel each other, leaving no direct effect on total sick days or annual earnings. When looking at spousal responses, we estimate significant cross effects. We estimate that wives, in couples for whom only the husband receives a higher replacement rate, increase the total number of sick days by more than 9% on average per month. This corresponds to a cross effect of more than 1.5 sick days per year. In couples for whom only the wives receive a higher replacement rate, husbands increase their total sick days by more than 6% on average, corresponding to 0.5 more sick days per year. Simultaneously, we find no cross effects on annual labour earnings, which indicates that indirectly affected spouses mainly respond by changing labour supply margins, which can be easily adjusted. As we find no direct effects on total sick days or annual earnings, our results provide weak support for cross-income or cross-hour effects. Furthermore, we do not find that a joint sickness effect and, hence, complementarity in spousal leisure time are important for the estimated overall cross effect. Instead, an interpretation of our results is that the cross effect stems from increased insurance coverage of the partner per se.

Our paper is related to a growing body of empirical research on how labour market reforms and social reforms targeted at a specific segment of workers have labour supply effects well beyond the initially targeted segment (see, e.g., Dahl et al. (2014), Maurin and Moschion (2009) and Alesina et al. (2006)). Previous studies have shown that retirement decisions by husbands and wives are positively correlated, thereby suggesting complementarities in spousal leisure (Blau, 1998; Gustman and Steinmeier, 2000), and that wives’ labour supply decreases if their husbands receive more generous unemployment benefits, suggesting substitutability in spousal leisure (Lundberg, 1985; Cullen and Gruber, 2000). Furthermore, Gelber (2014) exploits a Swedish tax reform that took place in 1990–1991 and finds that a lower marginal tax rate on earned income for one spouse has a positive effect on earnings for both spouses. Interpreting earnings as labour supply, the result suggests complementarity in spousal leisure. Our paper is perhaps most related to Goux et al. (2014), who study cross effects from a French workweek reduction reform. Because the reform had no effect on earnings, their results can be interpreted as pure cross-hour effects. Goux et al. (2014) show that husbands reduce their working hours when their wives experience a reduction in working hours but that wives do not change their working hours when their husbands experience reduced working hours. The results in Goux et al. (2014) are consistent with complementarity in spousal leisure with respect to husbands’ behaviour.

Our contribution to the literature is to provide a new assessment of the insurance role played by spousal labour supply, with a specific emphasis on the public sickness insurance. To our knowledge, this is the first study to reveal significant cross effects of the public sickness insurance on spousal labour supply. An important lesson is that sickness insurance reforms can have significant effects that go well beyond the targeted population even if there is no direct income effect and no direct labour supply effect on total sick days.

Moreover, our results highlight the importance of considering labour supply margins that workers can easily adjust, such as sick reporting, when evaluating labour market and social insurance reforms. That labour supply responses are found on such margins is consistent with the results in Goux et al. (2014), in which the cross effects from a reduction in working hours are mainly driven by “non-usual” hours, such as overtime and absenteeism.

We also emphasise the relevance of studying the cross effects of the sickness insurance system. For example, in contrast to unemployment or retirement decisions, sickness absence is a situation most couples face many times during a year and over their entire working life. Thus, spousal labour supply responses from the sickness insurance represent a general behaviour compared to behavioural responses from the unemployment insurance.
The remainder of this paper is organised as follows. Section 2 discusses the reform. Section 3 describes the empirical strategy and the data we use. Section 4 presents a graphical analysis, and Section 5 presents regression results. Section 6 discusses how to interpret the results, and Section 7 provides concluding remarks.

2. Institutional context and the reform

As described in Pettersson-Lidbom and Skogman Thoursie (2013), Sweden has a compulsory publicly administered sickness insurance primarily funded through a payroll tax levied on employers. Income compensation constitutes the major part of the programme, but the replacement rate has varied over time. The insurance replaces income losses up to a given income cap. A doctor’s certificate is required from the eighth day. For a majority of workers, collective agreements supplement the compensation from the public system.

Swedish sickness insurance was reformed the first of December 1987. In 1987, Swedish workers were employed in three sectors: the private sector (45%), the local government (39%), and the central government (16%). The reform had differential effects on workers depending on their sector of employment.

Before the reform, workers in the private sector received sickness benefits from the second day of sickness, and the replacement rate was dependent on a number of factors, such as working part time or full time, working irregular hours, and working as a shift worker. Thus, the replacement rate could vary from zero to one (see the government report Ds 5 1986:8). Many workers also received additional benefits from their employers as a result of collective agreements between unions and employers. Thus, we are unable to compute the exact replacement rate before the reform due to the complexity of collective agreements. In the private sector, the replacement rate was increased to 90% from the second day of sick leave.

Local government workers received before the reform sickness benefits from the first day of sickness after the reform. The replacement rate for central government workers was unaffected by the reform in 1987. The central government had made use of the same time. Thus, the reform induced an 11% increase in the replacement rate.

Local government workers received before the reform sickness benefits from the first day of sickness, and the replacement rate was 90% from the second day of sick leave. After the reform, local government workers received full wage replacement (100%) from the first day because the collective agreements were renegotiated at the same time. Thus, the reform induced an 11% increase in the replacement rate for local government workers.

The replacement rate for central government workers was unaffected by the reform in 1987. The central government had made use of the Social Security Act (1962:381), which allowed employers to administer their employees’ sick leave payments while collecting the employees’ sickness benefits (called arbetsk Garrisonsträde in Swedish). Thus, the replacement rate was 92% of earnings both before and after the reform, and cash benefits were paid from the first sick day.

All workers in Sweden received a letter with detailed information about the reform from the Swedish Social Insurance Agency (previously known as the National Insurance Board) several months before December 1, 1987. The letter stated that all workers were required to provide information about their number of working days per year for them to receive the benefits. The reform was also extensively covered in the media, by both public television and all large newspapers.

3. Empirical strategy and data

3.1. Empirical strategy

In this paper, we attempt to understand spousal interdependencies in the labour supply and the importance of cross effects in the public sickness insurance. The variation is the result of the 1987 reform that increased the replacement rate for workers in the private sector and in the local governmental sector but left the rate unchanged for central government workers. A central feature of our empirical strategy is that central government workers were not targeted by the reform. We analyse married central government workers and compare the outcome for those with spouses employed in the local governmental or the private sector (the treatment group) before and after the reform with the outcome for those with spouses employed in the central government sector (the control group).

In other words, we apply a difference-in-differences strategy whereby the treatment status for an individual is defined based on whether the spouse’s replacement rate was affected by the 1987 reform.

We analyse labour supply decisions for directly affected spouses (direct effects) and indirectly affected individuals (cross effects), and separate analyses are performed for husbands and wives. Directly affected spouses are denoted by the superscript s, and indirectly affected individuals are denoted without any superscript. The difference-in-differences regression model for directly affected spouses is formulated as follows:

\[ Y_{igt} = \alpha_g + \lambda_t + \delta Post_{igt} + \beta X_{igt} + u_{igt} \]  

(1)

where \( Y_{igt} \) is the outcome (incidence of a sick spell, total sick days or annual labour earnings) for spouse i in group g (\( g = 0, 1 \) for control and treated, respectively) at time t. \( X_{igt} \) contains the individual age, whether a person lives in an urban area and the size of the family. \( \alpha_g \) is a group effect, time effects are represented by \( \lambda_t \), and \( Post_{igt} \) is a dummy variable taking the value of one for the treatment group after the 1987 reform and zero otherwise. The difference-in-differences estimate of the reform effect for directly affected spouses is represented by \( \delta \).

The difference-in-differences regression model for indirectly affected individuals is

\[ Y_{igt} = \alpha_g + \lambda_t + \delta Post_{igt}^s + \beta X_{igt} + u_{igt} \]

(2)

where the variables and coefficients are defined in an analogous way as in Eq. (1). The treatment variable, \( Post_{igt}^s \), has the superscript s because we are interested in how individuals react to the reform that affected their spouses. The difference-in-differences estimate \( \delta \) captures the average cross effect of the reform in 1987.

If the error term in Eq. (2) contains a group-time error \( v_{igt} \) so that \( u_{igt} = v_{igt} + \rho_{igt} \), OLS standard errors are flawed (Moulton, 1986; Donald and Lang, 2007; Angrist and Pischke, 2009). To account for the group-time error term, we adapt the strategy suggested by Donald and Lang (2007). First, we construct covariate adjusted group-time effects by estimating:

\[ Y_{igt} = \mu_{igt} + \beta X_{igt} + \tau_{igt} \]

(3)

where \( \mu_{igt} = \alpha_g + \lambda_t + \delta Post_{igt} + v_{igt} \). The estimated group-time effects, \( \hat{\mu}_{igt} \), from Eq. (3) are group-time means of the outcome adjusted for individual characteristics. In the next step, we regress these estimated group-time effects on the variables that only vary at the group and time levels:

\[ \hat{\mu}_{igt} = \alpha_g + \lambda_t + \delta Post_{igt} + \nu_{igt} \]

(4)

where \( \nu_{igt} = v_{igt} + (\mu_{igt} - \hat{\mu}_{igt}) \). As noted by Donald and Lang (2007), the homoskedasticity of \( \nu_{igt} \) is a natural assumption when the number of observations in each group-time cell is large, which is true in our case.

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4 In 2013, the income cap is set at a yearly income of SEK 333,750. For employed individuals, the replacement rate is based on the individual’s expected annual labour earnings. The expected annual labour earnings are predicted by the Swedish Social Insurance Agency.

5 The government decision was taken on December 18, 1986, as some workers received a relatively small fraction of their income if they were sick for a short period (SOU 1981:22 and SOU 1983:48).

6 A doctor’s certificate was required from the eighth day both before and after the reform for all workers.

7 To account for intra-class correlation and serial correlation, we adapt the exact same strategy for Eqs. (1) and (2). However, for simplicity, we only show how we adjust Eq. (2).
Although we account for intra-class correlation using Eq. (4), $\varepsilon_{gt}$ may still be serially correlated. To account for serial correlation, we take the yearly group difference of Eq. (4), thus leaving us with the following single time series:

$$\hat{\mu}_t - \mu_0 = \alpha_1 - \alpha_0 + \delta (\text{Posts}_t - \text{Posts}_0) + \varepsilon_1 - \varepsilon_0.$$  

In Eq. (5), the estimate of $\delta$ is identical to an estimate from a fixed-effects model with $N = 2$ and $t$ time periods. We use month-group data for the 1986–1991 period, implying 72 monthly observations. We apply the Newey-West estimator to Eq. (5) to make the standard errors robust to any type of heteroskedasticity and serial correlation. Because we estimate Eq. (5) with 72 observations, the standard errors have

### Table 1
Summary statistics for indirectly treated spouses aged 20–64, yearly data.

<table>
<thead>
<tr>
<th></th>
<th>Treated</th>
<th></th>
<th>Control</th>
<th></th>
<th>Difference-in-differences</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>After</td>
<td>Before</td>
<td>After</td>
<td>Before</td>
<td>[1)-(2)][(3)-(4)]</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td></td>
</tr>
<tr>
<td>Panel A: wives</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Incidence of sickness absence</td>
<td>0.65</td>
<td>0.68</td>
<td>0.68</td>
<td>0.72</td>
<td>0.008</td>
</tr>
<tr>
<td>Total days of sickness</td>
<td>13.56</td>
<td>11.68</td>
<td>11.78</td>
<td>13.00</td>
<td>1.509**</td>
</tr>
<tr>
<td>Annual labour earnings (SEK in hundreds)</td>
<td>1261.67</td>
<td>935.23</td>
<td>1348.76</td>
<td>1001.91</td>
<td>$-20,404^*$</td>
</tr>
<tr>
<td>Family size</td>
<td>3.43</td>
<td>3.40</td>
<td>3.40</td>
<td>3.36</td>
<td>$-0.009$</td>
</tr>
<tr>
<td>Age</td>
<td>42.34</td>
<td>41.68</td>
<td>42.58</td>
<td>42.20</td>
<td>0.275</td>
</tr>
<tr>
<td>Absolute age difference in couple</td>
<td>3.12</td>
<td>3.12</td>
<td>3.39</td>
<td>3.36</td>
<td>$-0.029$</td>
</tr>
<tr>
<td>Urban</td>
<td>0.42</td>
<td>0.42</td>
<td>0.38</td>
<td>0.37</td>
<td>$-0.010$</td>
</tr>
<tr>
<td>Observations</td>
<td>19,280</td>
<td>9471</td>
<td>8295</td>
<td>41,639</td>
<td></td>
</tr>
<tr>
<td>Individuals</td>
<td>5565</td>
<td>3559</td>
<td>2201</td>
<td>5565</td>
<td></td>
</tr>
<tr>
<td>Panel B: husbands</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Incidence of sickness absence</td>
<td>0.56</td>
<td>0.60</td>
<td>0.59</td>
<td>0.61</td>
<td>$-0.005$</td>
</tr>
<tr>
<td>Total days of sickness</td>
<td>8.62</td>
<td>8.88</td>
<td>8.69</td>
<td>9.51</td>
<td>0.566</td>
</tr>
<tr>
<td>Annual labour earnings (SEK in hundreds)</td>
<td>2007.88</td>
<td>1498.46</td>
<td>2076.17</td>
<td>1547.46</td>
<td>$-19,296$</td>
</tr>
<tr>
<td>Family size</td>
<td>3.54</td>
<td>3.47</td>
<td>3.40</td>
<td>3.36</td>
<td>0.034</td>
</tr>
<tr>
<td>Age</td>
<td>44.39</td>
<td>43.94</td>
<td>45.00</td>
<td>44.65</td>
<td>0.108</td>
</tr>
<tr>
<td>Absolute age difference in couple</td>
<td>3.15</td>
<td>3.15</td>
<td>3.39</td>
<td>3.36</td>
<td>0.004</td>
</tr>
<tr>
<td>Urban</td>
<td>0.32</td>
<td>0.32</td>
<td>0.38</td>
<td>0.37</td>
<td>$-0.008$</td>
</tr>
<tr>
<td>Observations</td>
<td>37,254</td>
<td>18,830</td>
<td>8295</td>
<td>4323</td>
<td></td>
</tr>
<tr>
<td>Individuals</td>
<td>10,276</td>
<td>6836</td>
<td>2146</td>
<td>10,276</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Columns (1) to (4) report mean values and standard deviations in parentheses. Column (5) reports difference-in-differences estimates with standard errors clustered on individuals in parentheses. "*" denotes statistical significance at the 5% level. "Before" refers to the 1986–1987 period, and "After" refers to the 1988–1991 period. Treated refers to spouses in the central government sector married to a partner in the local governmental or the private sector. Control refers to spouses employed in the central government sector married to a central government worker. Urban is a dummy variable taking the value one if the individual lives in one of the three largest cities in Sweden (Gothenburg, Malmö or Stockholm) and zero otherwise.

Notes: Upper/Lower CI refers to the upper and lower bound of a 95% confidence interval, respectively. The figure displays one hundred difference-in-differences estimates based on Eq. (6).
good properties. When estimating the effect on annual labour earnings, we use yearly data and thus have six observations.

3.2. Data

We match information on start and end dates for all sick spells in Sweden registered by the Swedish National Insurance Board with yearly individual information from the data register Longitudinal Individual Data (LINDA). LINDA constitutes a 3.3% representative sample of the Swedish population and includes register information on annual labour earnings and demographic characteristics. \(^8\) We measure labour supply in two ways. The first labour supply measure is sickness absence, for which we analyse the probability of starting a sick spell as well as the total number of sick days. The advantage of using sickness absence is that it is a labour supply margin that workers can easily adjust. In comparison to contracted hours, which take a long time to adjust, a worker can immediately adjust his or her labour supply by calling in sick from work, especially because a worker does not need a doctor’s certificate until the eighth day of sickness. Moreover, the high compensation rate from the first day of reported sickness makes sickness absence a well-suited measure of labour supply that workers de facto can adjust. We use annual labour earnings as a second and more traditional measure of labour supply. Annual labour earnings capture all types of margins, such as working hours, vacation and parental leave. A drawback with labour earnings as a measure of labour supply in our setting is that we cannot rule out that the reform affected wages. For instance, Markussen (2012) estimates that an increase in sick leave reduces the earnings two years later for workers still employed. \(^9\) Thus, any reform effect on earnings should be interpreted with care, as both a labour supply effect and a wage effect can be present.

We analyse married workers aged 20–64 with annual labour earnings above SEK 6000 and employed in the central government sector during the 1986–1991 period. \(^10\) LINDA contains no direct linkage between married partners. Thus, we define two individuals as a married couple if both are reported as married, registered in the same household, are of different sexes and have an age difference of at most fifteen years. The reform was implemented in December 1987. Thus, we exclude all spells that start in one year and end in another year to keep treatment constant within all spells. Our final sample of indirectly affected spouses is a repeated cross section consisting of 110,341 observations (41,639 observations for wives and 68,702 observations for husbands).

Table 1 displays the average values before and after the reform for the individual sample of indirectly treated wives in panel A and indirectly treated husbands in panel B. Before the reform, a majority of all individuals have at least one spell of sickness per year and the average total sick days are approximately 12 days for wives and approximately 9 days for husbands. A comparison between the treated and control groups before and after the reform suggests that the sickness incidence is unaffected by the reform for both husbands and wives. However, wives’ total sick days are extended by 1.509 days per year on average (see Column 5 of Table 1). An increase by 1.509 days corresponds to a 12.9% increase relative to the pre-reform average. Our second measure of labour supply, annual labour earnings, also reveals a cross effect for wives; annual labour earnings drop by 2.6% compared to the pre-average. For husbands, a raw mean comparison indicates a negative cross effect on labour supply; the total days of sickness increase and annual earnings decrease, but the difference-in-difference estimates are imprecise.

A concern with the difference-in-differences strategy arises if the composition of the treatment group changes relative to that of the control group over time. The results in Column 5 in Table 1 indicate no sign of compositional changes due to the reform; difference-in-differences estimates for family size, age, absolute age difference between partners in couples and living in an urban area are all statistically insignificant. \(^11\)

4. Graphical evidence

Before we perform the refined statistical analysis, we present graphical evidence of the direct effects and the cross effects of the 1987 reform. Separate analyses are performed for husbands and wives. Thus, when we analyse the cross effect on wives’ behaviour, the direct effect involves an analysis of how directly affected husbands reacted to the reform and vice versa.

4.1. Direct effects for husbands

We start to estimate reform effects over the entire distribution of sickness lengths. In other words, we estimate the reform effect on the probability that a sick spell exceeds a certain length. Estimations are conducted separately for all sick spells between zero and one hundred days. By doing these estimations separately, we analyse whether

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\(^8\) See Edin and Fredriksson (2000) for a general description of LINDA.

\(^9\) Specifically, Markussen (2012) finds that a 1% increase in the sick leave rate reduces the earnings two years later with 1.2%, and less than half of the effect is explained by the probability of being employed.

\(^10\) To be eligible for sickness insurance, an individual’s earnings have to be at least SEK 6000 per year.

\(^11\) The difference-in-differences results for total days are robust to the inclusion of individual fixed-effects. For wives, the estimate is 1.401 (with a p-value of 0.057), and for husbands, the estimate is 0.455 (with a p-value of 0.374).
husbands who receive a higher replacement rate change their incidence or duration of sick spells without making any sample selections (as discussed by Angrist and Pischke (2009); we avoid looking at positive spells, which are potentially endogenously determined). The distributional direct effects on sick spells are estimated using the following version of Eq. (1):

\[ Y_{g}^{d+}(t) = \alpha_{g}^{s} + \lambda_{t}^{s} + \delta_{g}^{s} \text{Post}_{g}^{s} + u_{g}^{s} \]

(6)

where \( Y_{g}^{d+}(d) \) takes the value of one if a directly affected spouse in group \( g \) has a sick spell of at least \( d \) days \( (d = 1, 2, \ldots, 100) \) that starts in month \( t \) (the notations are otherwise equivalent to the notations from Section 3.1). \( \delta_{g}^{s} \) measures the average reform effect on the probability that a directly affected spouse has a sick spell that lasts at least \( d \) days. If the reform increases the probability of starting a new spell, we have a positive estimate at the first threshold, i.e., \( \delta_{1}^{s} > 0 \). Signs of higher-order coefficients capture effects on duration. A negative sign of, for example, \( \delta_{2}^{s} \), indicates that spells of seven days are less frequent after the reform. Eq. (6) is estimated with monthly-group level data using OLS, as explained in Section 3.1.

Fig. 1 displays one hundred difference-in-differences estimates on the probability of having a sick spell exceeding a given length for directly affected husbands (with a corresponding 95% confidence interval). The direct effect at threshold one is significant and positive, implying that the reform increases the incidence of sick reporting for directly treated husbands.\(^{12}\) According to Fig. 1, spells from one to seven days are significantly more frequent after the reform, whereas spells between nine and fifteen days are less common. Thus, the sign of the net effect on total sick days is ambiguous. As discussed by Pettersson-Lidbom and Skogman Thoursie (2013), who analyse the direct effect, the opposing effects on the incidence and duration support the hypothesis that the reform made it less costly for workers to be absent for short periods once the waiting day was abolished.\(^{13}\) Instead of having a sick spell of eight days or more, after the reform, individuals can break the spell into two separate spells without having a doctor’s certificate. It is possible that with a one-day waiting period, workers want to be sure that they are fully recovered from their sickness before going back to their jobs. If they go back too early and become sick again, they have to “face” the one-day waiting period again. Without the one-day waiting period, workers have less to lose economically if they go back to their jobs too early and become sick again.

Fig. 2 displays the differences in average total sick days, on a half-year basis, between directly affected husbands and their control group for the period 1986 to 1991.\(^{14}\) The differences in average total sick days between treated and controls are normalised with respect to the average difference in the first half-year of 1986. The relative differences in average total sick days are around zero in the period up to the reform, while a small relative increase seems to occur in the years after the reform. Whether the increase in significant is analysed in Section 5.1.

Fig. A2 in Appendix A indicates that annual earnings for directly treated husbands do not change relative to the control group until 1990, when earnings drop relatively for treated husbands. Thus, our interpretation of Fig. A2 is that there is no immediate effect of the reform on annual labour earnings for directly affected husbands. The fact that labour earnings for directly treated husbands do not drop until 1990 is consistent with Markussen (2012), who finds that sickness absence affects earnings negatively with a two-year lag.

Taken together, the graphical evidence suggests that directly affected husbands increased their number of sick spells after the 1987 reform, whereas the duration of spells becomes shorter implying a small effect on the total number of sick days.

4.2. Cross effects for wives

To analyse reform cross effects over the entire distribution of sickness lengths, we estimate an analogous version of Eq. (6) but for indirectly affected wives. The results are displayed in Fig. 3. The probability of starting a new spell during a given month is unaffected.\(^{15}\) However, Fig. 3 reveals that spells longer than four days are significantly more common after the reform among indirectly treated wives. Thus, Fig. 3 suggests that the duration of spells as well as total sick days increase for indirectly affected wives when their husbands’ sickness insurance becomes more generous.

\(^{12}\) Fig. A1 in Appendix A displays the development of the differences in the incidence of sickness absence for directly treated husbands and their controls relative to the difference in the first half-year of 1986. The most important message from Fig. A1 is that the directly treated and controls have parallel trends in the incidence of sick reporting before the reform.

\(^{13}\) We find that directly affected individuals increase their number of sick spells but that the duration of spells becomes shorter. The net effect on total sick days is positive but insignificant for both wives and husbands. The pattern with more but shorter sick spells is in line with Pettersson-Lidbom and Skogman Thoursie (2013). They use the same reform as in the current paper to estimate the direct effect on all workers, regardless of whether workers are married. In contrast to our study, Pettersson-Lidbom and Skogman Thoursie (2013) estimate a negative direct effect of an increased replacement rate on total sick days. When we use our sample of married workers and pooling wives and husbands together, our effect on total days is positive and marginally significant. We explain why our estimate on total days differs from the corresponding estimate in Pettersson-Lidbom and Skogman Thoursie (2013) in the online Appendix.

\(^{14}\) Note that our identification strategy does not allow us to estimate reliable standard errors when looking at half-year differences. For this reason Figs. 2, 4, 6 and 8 do not contain confidence bands.

\(^{15}\) This result is confirmed by Fig. A3 in Appendix A, which displays the half-year development of the relative incidence of sick reporting for indirectly treated wives and their controls.
Notes: The differences in average total sick days between treated and controls are normalised with respect to the average difference in the first half-year of 1986. Treated refers to female workers in the local government sector married to a partner in the central government sector. Control refers to female workers in the central government sector married to a central government worker.

**Fig. 4.** Normalised differences in average total sick days on a half-year basis for indirectly treated wives and their controls during the 1986–1991 period. Notes: The differences in average total sick days between treated and controls are normalised with respect to the average difference in the first half-year of 1986. Treated refers to wives in the central government sector married to a partner in the local governmental or the private sector. Control refers to wives employed in the central government sector married to a central government worker.

Notes: Upper/Lower CI refers to the upper and lower bounds of a 95% confidence interval, respectively. The figure displays one hundred difference-in-differences estimates based on Eq. (6).

**Fig. 5.** Direct reform effects on the likelihood that a sick spell exceeds a certain spell length with 95% confidence bands. Wives. Notes: Upper/Lower CI refers to the upper and lower bounds of a 95% confidence interval, respectively. The figure displays one hundred difference-in-differences estimates based on Eq. (6).

Notes: The differences in average total sick days between treated and controls are normalised with respect to the average difference in the first half-year of 1986. Treated refers to female workers in the local government sector married to a partner in the central government sector. Control refers to female workers in the central government sector married to workers married in the central government sector. Control refers to female workers in the central government sector married to central government workers.

**Fig. 6.** Normalised differences in average total sick days on a half-year basis for directly treated wives and their controls during the 1986–1991 period. Notes: The differences in average total sick days between treated and controls are normalised with respect to the average difference in the first half-year of 1986. Treated refers to female workers in the local government sector married to a partner in the central government sector. Control refers to female workers in the central government sector married to workers married in the central government sector.
Fig. 4 plots the differences in average total sick days between indirectly affected wives and their control group (all differences are relative to the difference in the first half-year of 1986). Prior to the reform, the relative differences in average total sick days are around zero or less for treated wives compared to untreated wives on average. After the reform in 1987, average total sick days increases for indirectly treated wives relative to the control group of wives. Given the shift in the relative differences in average total sick days before and after the reform, Fig. 4 lends support to a cross effect, i.e., wives’ total sick days increase when their husbands receive a higher replacement rate within the sickness insurance system.

We also analyse cross effects on wives’ labour earnings (see Fig. A4 in Appendix A). No clear trend break is observed after 1987, an issue that we further explore in Section 5.

4.3. Direct effects for wives

Before analysing cross effects on husbands, we first analyse direct effects on their wives. Fig. 5 displays one hundred difference-in-differences estimates on the probability of starting a sick spell exceeding a certain length for directly affected wives using the model represented by Eq. (6). The pattern for directly affected wives is similar to that of directly treated husbands (see Fig. 1). The probability of starting a new sick spell and having spells up to five days is greater after the reform, whereas spells between eight and 19 days become less common for directly affected wives after the reform. No effect is found for longer spells.

In addition, a sharp positive jump in the incidence of sickness absence for directly treated wives following the reform is shown in Fig. A5 in Appendix A.

Fig. 6 displays the development of the differences in average total sick days for directly affected wives and their control group (all differences are relative to the difference in the first half-year of 1986). No clear relative change is observed in the years after the reform. When analysing annual labour earnings for directly affected wives a relative decrease is observed after the reform (see Fig. A6 in Appendix A).

Analogous to the analysis on directly affected husbands, directly affected wives appear to increase the number of sick spells due to the 1987 reform, whereas the duration of spells becomes shorter. Next, we analyse the cross effects on husbands.

4.4. Cross effects for husbands

The estimated reform effects on husbands’ distribution of sick spells based on Eq. (6) are displayed in Fig. 7. The incidence is
unaltered by the reform, but sick spells from 3 to as many as 20 days are significantly more common after the reform, suggesting that only the duration of spells increases for indirectly affected husbands. Fig. A7 in Appendix A confirms a lack of cross effects on the husbands’ incidence.

Fig. 8 displays the development of the differences in average total sick days for indirectly treated husbands and their controls (all differences are relative to the difference in the first half-year of 1986). Fig. 8 illustrates that there is a relative increase in total sick days for indirectly treated husbands in the period after the reform. Whether the relative increase is significant or not is analysed in Section 5.

Fig. A8 in Appendix A displays the evolvement of husbands’ earnings for indirectly treated and controls. In the pre-period, the groups have parallel trends, and no clear spillover effect on husbands’ earnings is observed in the post-period.

To summarise, the graphical analysis presented above preliminary suggests that directly affected individuals increase their number of sick spells when their sickness insurance benefit increases, whereas the duration of spells becomes shorter. In contrast, indirectly affected spouses appear to prolong their existing sick spell rather than starting more spells as a reaction to the spouse’s more generous sickness insurance. This results in an increase in total sick days. The next step is to use regression analysis to quantify the effect and test its robustness.

5. Regression results

In this section, we estimate the effect of the 1987 reform using the difference-in-differences model presented in Section 3.1. We start by estimating the direct effects and then continue with the cross effects.

5.1. Direct effects

We estimate the direct effects on the sickness incidence, total sickness days and annual labour earnings based on Eq. (1) using month-group data. Estimates of direct effects on the monthly incidence and total sick days are displayed in Table 2. Columns 1 and 2 report results for directly affected wives and Columns 3 and 4 report results for their husbands. The sickness incidence for directly affected wives is estimated to increase by 1.6 percentage points on average in response to the reform. In the pre-period, the fraction of wives starting a sick spell during a month is approximately 14%, implying that the incidence of sick reporting for directly affected wives increased by 11.4%. The corresponding average effect for husbands is 10.9% and statistically significant.

We do not estimate any significant direct effects on total sick days. The lack of direct effects is consistent with the patterns observed in Figs. 1 and 5, in which the increase in the number of short spells was offset by a smaller number of longer spells after the reform for directly treated spouses.

Next, we turn to the direct effects on annual labour earnings reported in Table 3. Column 1 reports the direct estimates for wives using data at a group-year level, and the results in Column 2 are based on
individual data for which standard errors are clustered at the individual level. The corresponding estimates for husbands are reported in Columns 3 and 4. The direct effect on wives' annual labour earnings is statistically significant when using individual data and estimated to be $-4.6\%$. For husbands, we estimate no statistically significant effect. The size of the estimated reform effects for wives and husbands is similar regardless of whether the model uses group-year data or individual-level data.

5.2. Cross effects

We estimate reform cross effects on husbands and wives using the difference-in-differences model given by Eq. (2) (once again, we rely on month-group level data). Table 4 displays estimated cross effects on monthly sickness incidence and total sick days per month. Columns 1 and 2 report results for indirectly affected wives, and Columns 3 and 4 report results for indirectly affected husbands. Consistent with the above graphical analysis, we estimate no effect on the incidence. However, we estimate a significant cross effect on total sick days. Among wives, total sick days increase significantly by $9.1\%$ on average per month if their husbands' sickness insurance benefits become more generous; see Column 2 of Table 4. The effect on a yearly basis is approximately 1.5 more sick days per year (0.125 $\times$ 12). The corresponding cross effect for husbands is $6.1\%$ (see Column 4 of Table 4), which amounts to 0.5 more sick days per year.

Cross effects on annual labour earnings are displayed in Table 5. Column 1 reports estimates for wives using data at a group-year level, and Column 2 reports estimates for wives using individual-level data. The corresponding estimates for husbands are reported in Columns 3 and 4. Regardless of the aggregation level of the data, our estimates are imprecisely estimated.

5.3. Cross effects for local government workers

We know that the reform increased the replacement rate by $11\%$ for workers in the local government, but the exact increase in the replacement rate is unknown for workers in the private sector. Because more than $80\%$ of the directly affected husbands in our sample are employed in the private sector, our main results use spouses of both local government workers and private sector workers. However, to adequately compare the size of the cross effect between husbands and wives, the change in the replacement rate for the directly affected spouses must be the same. Table 6 displays direct effects on the sickness incidence and cross effects on total sick days excluding spouses in the private sector and their partners. The results are similar to the previous results when all spouses were included in the sample. Because the exact change in the replacement rate for local government workers is known, the elasticity of total sick days for indirectly affected wives with respect to husbands' replacement rate is approximately 0.83 (0.091/0.11). For indirectly affected husbands, the elasticity is 0.59 (0.065/0.11). This suggests that the cross effects are relatively larger for wives compared to husbands.

6. Interpretation of the results

Our analysis suggests that the direct effect and the cross effect are different. The direct effect increased the sickness incidence and decreased the average spell length, whereas the cross effect had no effect on the incidence but prolonged the average spell length. The opposing direct effects on the incidence and duration support the hypothesis that the reform increased the replacement rate.

Table 7

<table>
<thead>
<tr>
<th></th>
<th>Wives</th>
<th>Husbands</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>Total sick days</td>
<td></td>
</tr>
<tr>
<td></td>
<td>when the husband had</td>
<td></td>
</tr>
<tr>
<td></td>
<td>a low sick propensity</td>
<td></td>
</tr>
<tr>
<td></td>
<td>in 1986</td>
<td></td>
</tr>
<tr>
<td>(1)</td>
<td>(2)</td>
<td></td>
</tr>
<tr>
<td>Cross effect</td>
<td>0.029***</td>
<td>0.060**</td>
</tr>
<tr>
<td>(DD)</td>
<td>(0.049)</td>
<td>(0.026)</td>
</tr>
<tr>
<td>Pre-average</td>
<td>0.974</td>
<td>1.516</td>
</tr>
<tr>
<td>Reform effect</td>
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<td>11.1</td>
</tr>
<tr>
<td>in %</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>72</td>
<td>72</td>
</tr>
</tbody>
</table>

Notes: All difference-in-differences estimates (DD) are based on Eq. (5). All models control for family size (linear and quadratic), workers' age (linear and quadratic) and living in an urban area. Newey-West standard errors are displayed in parentheses using the formula $0.75T^{-1/3}$, where $T$ equals the number of time periods. *** denotes statistical significance at the 1% level, ** denotes statistical significance at the 5% level and * denotes statistical significance at the 10% level. Pre-average refers to average value in the period from 1986 to 1987.

Table 8

<table>
<thead>
<tr>
<th></th>
<th>Couples for whom the husband receives a higher replacement rate</th>
<th>Couples for whom the wife receives a higher replacement rate</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Total joint sick days</td>
<td>Total joint sick days</td>
</tr>
<tr>
<td>(1)</td>
<td>(2)</td>
<td></td>
</tr>
<tr>
<td>Joint sickness effect (DD)</td>
<td>0.009**</td>
<td>0.005</td>
</tr>
<tr>
<td>(0.004)</td>
<td>(0.004)</td>
<td></td>
</tr>
<tr>
<td>Pre-average</td>
<td>0.036</td>
<td>0.031</td>
</tr>
<tr>
<td>Reform effect</td>
<td>25.1</td>
<td>16.1</td>
</tr>
<tr>
<td>in %</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>72</td>
<td>72</td>
</tr>
</tbody>
</table>

Notes: All difference-in-differences estimates (DD) are based on Eq. (5). All models control for family size (linear and quadratic), workers' age (linear and quadratic) and living in an urban area. Newey-West standard errors are displayed in parentheses using the formula $0.75T^{-1/3}$, where $T$ equals the number of time periods. ** denotes statistical significance at the 5% level. Pre-average refers to average value in the period from 1986 to 1987.
that the reform made it less costly for a worker to be absent for short periods once the waiting day was abolished by the reform in 1987. However, for the indirectly treated partner, the reform did not explicitly make it less costly to be absent for short periods since they had no waiting day either before or after the reform. Although we are not able to pin down the exact mechanisms, some of them appear to be more important for the cross effect than others.

To start, our results lend weak support for a pure cross-hour effect. The reason is that the total number of sick days per month remained intact for the directly affected partner (only the distribution of sick days changed).

We also find weak support for a cross-income effect. First, the direct effect on labour earnings for wives is negative, whereas the cross effect on husbands’ total sick days is positive. Such an effect is inconsistent with leisure being a normal good. Second, we find no direct effect on husband’s labour earnings, but we still find a positive cross effect on wives’ total sick days. However, the higher insurance coverage for one spouse can create another type of income effect if the increased future expected household income affects the labour supply of the other spouse today. Previous research indicates that wives of unemployed husbands would work more hours if their husbands had no unemployment insurance (Mincer, 1962; Lundberg, 1985; Cullen and Gruber, 2000). The magnitude of our estimated cross effects is in line with Cullen and Gruber (2000). They find that for each dollar increase in the husband’s unemployment insurance benefit level, wives earn 73 cents less. The elasticity of 0.73 is comparable with our estimated elasticity of total sick days for wives of 0.83 with respect to the husband replacement rate in the sickness insurance (see Section 5.3).

If an insurance effect is important in our setting, we expect a stronger cross effect from the reform if the directly affected spouse tends to be sick. The results in Table 7 indicate that the cross effect is larger when the directly affected spouse had a higher propensity of being sick in 1986. For wives, we estimate no cross effect if their husbands had a low sickness propensity in 1986. However, the cross effect on wives’ total sick days is more than 11% if the directly affected husband had a high sickness propensity in 1986. For husbands, the cross effect is approximately 10% regardless of the wives’ sickness history.

Can the cross effect on total sick days be explained by a joint sickness effect? For example, Goux et al. (2014) find that husbands reduce their working hours when their wives experience a reduction in working hours suggesting complementarity in spousal leisure. A joint sickness effect occurs in our setting if couples like to spend leisure time together (a joint leisure effect).\(^\text{17}\)

To understand the importance of a joint leisure effect, we estimate whether the number of joint sick days in a couple changes as a reaction to the reform.\(^\text{18}\) The results in Table 8 indicate that in couples for whom the husband receives a higher replacement rate, the partners spend on average 0.009 more days per month being sick at the same time. However, part of the increase can be a mechanical effect. Even if the partners’ sickness absence are independent from each other, the cross effect on total sick days makes joint sickness absence more likely in statistical terms. If we assume that sick cases have a uniform distribution over time and that sick cases within a couple are independent, the average number of joint sick days for group \(g = 0, 1\) for control and treated, respectively in month \(m\) is given by

\[
\text{Joint sick days}_{gm} = \sum_{d=1}^{30} y_{gdm} \times y_{gdm}
\]

where \(y_{gdm}\) is the fraction of directly affected spouses that are sick at day \(d\) in month \(m\) and \(y_{gdm}\) is the corresponding fraction for indirectly affected spouses (we assume that all months have 30 days). In couples for whom the husband receives a higher replacement rate in December 1987, Eq. (7) tells us that if spousal sick days are independent of each other, we expect 0.026 joint sick days before the reform in the treatment group.\(^\text{19}\) After the reform, the number of joint sick days increases by an average of 0.004 days in the treated group relative to the control group.\(^\text{20}\)

Thus, only 0.005 days (0.009−0.004) of the estimated increase in joint sick days can be attributed to a joint leisure effect explaining 4% of the total cross effect on total sick days for wives (recall that the estimated cross effect is 0.125 sick days). The same exercise for couples for whom the wife received a higher replacement rate suggests that a joint leisure effect explains 4.3% of the average cross effect on husbands’ total sick days. Altogether, a joint sickness effect does not appear to be an important mechanism for the estimated overall cross effect.

7. Conclusion

In this paper, we have analysed interdependencies in spousal labour supply. We exploit independent variation in the replacement rate for one spouse to analyse whether it has an effect on the labour supply of the other spouse. Indirectly affected spouses mainly respond by adjusting the labour supply margins that can easily be adjusted, namely, sick reporting. We estimate that in couples for whom only the husband receives a higher replacement rate, wives increase their total sick days by more than 1.5 sick days per year. The corresponding cross effect for husbands is smaller and amounts to one-third of the cross effect found for wives. Our estimated cross effects are observed even though we do not find any effect on total sick days or annual earnings for directly affected spouses; we do find that directly affected individuals increase their number of sick spells, but the duration of spells becomes shorter, and thus, the net effect on total sick days is zero.

One interpretation of our estimated cross effect is that it stems from an increased insurance coverage of the partner per se; with a higher sickness insurance benefit level, the expected household income increases because the costs of future illnesses decrease. Spousal labour supply thereby becomes less important as insurance for future income shocks to the family, which increases sick reporting by the indirectly affected spouse.

The traditional spousal labour supply literature estimates cross effects of changes in the other spouse’s tax rate, earnings or labour supply. In contrast to this literature, an important lesson from this study is that sickness insurance reforms can have significant effects that go well beyond the targeted population, even if there is no significant direct effect on labour supply. In general, we have demonstrated that spousal cross effects from sickness insurance programmes can be significant and must be taken into account when evaluating the overall impacts of reforms of sickness insurance programmes.

\(^{16}\) To ensure that the partners’ sickness absence behaviour is unaffected by the reform, we use the distribution in total sick days in 1986 for directly affected spouses and run separate regression models for the bottom quartile and the top quartile.

\(^{17}\) Another joint sickness effect occurs if the home environment becomes more infectious (a contagion effect). However, a contagion effect seems unlikely in our case because the total number of sick days of the directly affected spouse remained intact after the reform.

\(^{18}\) The reform effect on joint sick days is estimated using a difference-in-differences model with month-group data as described in Section 3.1.

\(^{19}\) The values from Tables 2 and 4 illustrate that before the reform, average joint sick days are \((0.680/30) \times (1.139/30) \times 30 = 0.026\).

\(^{20}\) The average joint sick days after the reform are \([(0.680 + 0.028)/30] \times [(1.139 + 0.125)/30] \times 30 = 0.030\). Thus, the difference is 0.004 (0.030 – 0.026) joint sick days.
Appendix A

Fig. A1. Normalised differences in the incidence of sickness absence on half-year basis for directly affected husbands and their controls during the 1986–1991 period. Notes: The differences in incidence of sickness absence between treated and controls are normalised with respect to the average difference in the first half-year of 1986. Treated refers to male workers in the local government or the private sector married to a partner in the central government sector. Control refers to male workers in the government sector married to workers in the government sector.

Fig. A2. Normalised differences in average annual earnings for directly affected husbands and their controls during the 1986–1991 period. Notes: The differences in average annual earnings between treated and controls are normalised with respect to the average difference in 1986. Treated refers to male workers in the local government or the private sector married to a partner in the central government sector. Control refers to male workers in the government sector married to workers in the government sector.

Fig. A3. Normalised differences in the incidence of sickness absence on half-year basis for indirectly affected wives and their controls during the 1986–1991 period. Notes: The differences in incidence of sickness absence between treated and controls are normalised with respect to the average difference in the first half-year of 1986. Treated refers to wives in the central government sector married to a partner in the local governmental or the private sector. Control refers to wives employed in the central government sector married to a central government worker.
Fig. A4. Normalised differences in average annual earnings for indirectly affected wives and their controls during the 1986–1991 period. Notes: The differences in average annual earnings between treated and controls are normalised with respect to the average difference in 1986. Treated refers to wives in the central government sector married to a partner in the local governmental or the private sector. Control refers to wives employed in the central government sector married to a central government worker.

Fig. A5. Normalised differences in the incidence of sickness absence on half-year basis for directly affected wives and their controls during the 1986–1991 period. Notes: The differences in incidence of sickness absence between treated and controls are normalised with respect to the average difference in the first half-year of 1986. Treated refers to female workers in the local government or the private sector married to a partner in the central government sector. Control refers to female workers in the government sector married to workers in the government sector.

Fig. A6. Normalised differences in average annual earnings for directly affected wives and their controls during the 1986–1991 period. Notes: The differences in average annual earnings between treated and controls are normalised with respect to the average difference in 1986. Treated refers to female workers in the local government or the private sector married to a partner in the central government sector. Control refers to female workers in the government sector married to workers in the government sector.
Appendix B. Supplementary material

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.labeco.2015.02.005.

References


