Temporary Disability Insurance and Labor Supply: Evidence from a Natural Experiment*

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Abstract

Most developed countries have compulsory insurance programs for temporary disability, i.e., cash benefits for non-work-related sickness. Despite the economic significance of these programs, little is known about their effects on work absenteeism or labor supply. We exploit a policy reform which consisted of the abolishment of a waiting day together with an increase of cash benefits for short sick leaves. We find that the total number of days of sickness absence was reduced by the reform which is likely due to that the abolishment of the waiting period made it less costly for workers to be absent for short periods.

Keywords: Paid sick leave, labor supply, difference-in-differences

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I. Introduction

Disability policies have become a key policy area in many industrialized countries.[1] This paper deals with one such policy, namely temporary disability insurance (henceforth TDI) programs, also referred to as cash sickness benefits. TDI is the most common method used to provide workers with compensation for loss of wages caused by *temporary non-occupational* sickness or injury.[2] All OECD countries but South Korea have some form of TDI program. Perhaps less well known, there are also five US States that have TDI programs.[3] Typically, the vast majority of employed workers are covered by TDI programs but there are exceptions.[4] The total amount of TDI benefits paid is often substantial. For example, Ireland, Spain, Denmark, Poland, Norway, New Zealand, Slovak Republic, and Sweden typically spend more than 1 percent of GDP on cash sickness benefits (OECD, Social Expenditure Data Base).

Despite the economic significance of TDI programs, however, there is limited knowledge about the effect of sickness insurance benefit levels on labor supply or sickness absenteeism. As a case in point, the recent survey of labor supply responses to social insurance programs by Kreuger and Meyer (2002) in the *Handbook of Public Economics* does not even cover TDI programs even though these programs can be as large as unemployment insurance programs.[5] Nonetheless, there are some previous studies of the effect of TDI benefits on labor supply (e.g., Barmby *et al*. 1991, 1995, Henrekson and Persson 2004 and Johansson and Palme 2005).

However, it is questionable whether previous studies have identified a causal effect since there are a number of important limitations in their identification strategies. The key problem of studying the effect of benefits on sickness absence is that benefits differ across

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[1] See, for example, the recent book *Transforming Disability into Ability* published by the OECD. Moreover, there is also a recent debate in the U.S. on whether employers should be forced to provide short-term disability benefits i.e., the Healthy Families Act (S. 910 and H.R. 1542, 110th Congress), since the current law – Family and Medical Leave Act – does not require employers to offer sick leave. The Healthy Family Act would instead guarantee a minimum of seven paid sick days annually for full-time employees and a pro-rata amount for part-time employees.

[2] TDI programs are different from public programs that provide income support to individuals unable to continue work due to disability, i.e., disability insurance (DI) programs.

[3] TDI provides workers with partial protection against the loss of wages due to non-occupational disability. This protection is offered to workers in California, Hawaii, New Jersey, New York, Rhode Island, Puerto Rico, and the railroad industry. Most of the U.S. State programs were established during the 1940s as an outgrowth of the unemployment insurance (UI) program. For more information about TDI, see the information provided by the Social Security Administration, i.e., Social Security Programs in the United States (http://www.ssa.gov/policy/docs/progdesc/sspus/tempdib.pdf) and Kerns (1997).

[4] In the United States, for example, only 24 million or about 22 percent of the national private sector workforce are covered by TDI programs.

workers primarily through their past earnings histories. However, an individual's earnings history will most likely be highly correlated with his/her tastes for work, and it is difficult to disentangle the behavioral effects of TDI from these differences in taste (e.g., Bound 1989, 1991).

To convincingly estimate the causal impact of benefits on labor supply, a variation in benefits that is independent of a worker's taste for work is therefore required. Henrekson and Persson (2004) and Johansson and Palme (2005) use variation in the sickness benefit level due to changes in the Swedish sickness insurance system. Although this is an arguably better identification strategy than those previously used (e.g., Barmby *et al*. 1991, 1995), there is still a number of serious threats to this type of strategy. First, changes in the sickness insurance system typically affect *all* workers at the same time. Therefore, this implies that the empirical evaluation can at best be based only on a before and after comparison. A before and after evaluation strategy might be useful if the variation in the outcome is stable over time, but sickness absence rates are notoriously volatile (at least in Sweden), which makes it doubtful whether a before and after design is useful in practice. Second, since all workers are affected by the change in the sickness insurance system at the same time, this raises important issues about how to compute valid standard errors if there are common group and time effects as recently discussed by Bertrand *et al*. (2004) and Donald and Lang (2007). Accounting for the clustering in the data typically leads to dramatic changes in the inference. Finally, many of the changes in the Swedish sickness insurance system reforms were caused by concerns about a high sickness absence. For example, the cut of cash benefits in the 1991 reform, as explicitly analyzed by Johansson and Palme (2004), was the result of the central government's concern about the very large increase in sickness benefits costs. Thus, this makes the policy change potentially endogenous (e.g., Besley and Case 2000) which again raises some doubts about the causal interpretation of previous work.

In this paper, we use a change in the Swedish sickness insurance system on December 1 1987, which has a number of attractive features. Most importantly, while there was a general increase in cash benefits for most of the Swedish workforce, there were some workers that had the same benefits levels before and after the reform. Thus, there is a well-defined control group of workers not affected by the policy change, which is crucial since the problems discussed above can then be solved. In other words, we make use of a difference-in-

[5] In California, for example, the benefits paid in 2001from TDI were $2.7 billion while the UI amounted to $3.4 billion. (Social Security Administration, Annual Statistical Supplement, 2004, Table 9A and 9C, respectively).

differences (DD) approach to estimate the effect of a change in the benefit level on sickness absence. Importantly, thanks to the data – a representative longitudinal sample of 3.3 percent of the Swedish population – we can address most of the concerns about the DD method such as whether time effects are common across treatment and control groups (the parallel trend assumption), whether the composition of both treatment and control groups is stable before and after the policy change (compositional bias), and clustering in the data due to the fact that the policy only varies at the group level.

It is noteworthy that the policy change included a combination of an increase in the replacement level for spells up to fourteen days and the abolishment of a waiting period. Effects of waiting days on sickness absence behavior have not previously been studied.[6] A waiting period is an important policy parameter since the majority of the OECD countries have such a period (see table A1 in the appendix). The abolishment of a waiting period can have additional implications for sickness absence behavior as compared to a pure change in the replacement rate. For example, if a worker faces some uncertainty about whether he or she will be sick again after a period of sickness absenteeism, the abolishment of a waiting period will make it less costly to have multiple short sickness spells rather than having one long sickness spell only. As a result, the abolishment of a waiting period might increase the number of sick spells but decrease the average length of ongoing spells.

The results show that the December 1 1987 reform caused an 11 percent increase in the share of workers with new sick spells. There was also a large shift in the distribution of spell lengths which resulted in an increase in the number of short spells and a decrease in the number of long spells. The estimated net effect of the reform on the total number of days of sickness absence was a three-percent reduction. This negative impact of the reform on the total number of days is perhaps not surprising since the policy reform made it less costly for a worker to be absent for short periods as noted above. Our finding thus suggests that the length of the waiting period and how the income replacement rates vary with spell lengths are likely to have important implications for the design of social insurance programs more generally. For example, the insurance literature stresses the importance of deductibles in order to reduce moral hazard behavior. A waiting period may also be an effective way of reducing such behavior in the sickness insurance context. However, a waiting period can be argued to be

[6] Studies on how benefit time profiles affect duration behavior are more common in the unemployment insurance literature (see e.g., Carling *et al.* 1996). One exception is Johansson and Palme (2005) who find that sick spells became longer when the profile of the sickness benefit increased as the result of a reform in 1991.

unfair for workers with work capacity but who are not allowed to go to work even in the case of the slightest risk of being infectious, such as when catching a minor cold. This is, for example, the case for many workers in the care and food sectors where the costs of contagion can be really high. This paper contributes to this discussion by showing that the abolishment of a waiting period does not necessarily increase the total number of sick days.

This paper contributes to the literature studying labor supply responses of social insurance programs. As discussed by Kreuger and Meyer (2002), this literature is faced by challenging identification issues. They suggest that data from federal countries (e.g., US States or Canadian Provinces) may provide a useful exogenous source of variation in social insurance programs since it is possible to exploit variation across federal units. A case in point is Gruber's (2000) study of disability insurance (DI) which exploits the fact that Quebec had a different DI system than the rest of the Canadian provinces. Using a DD approach, he finds strong behavioral effects of DI. However, also using Canadian data, Campolieti (2004) finds little evidence that disability benefits are associated with an increase in the probability of non-participation or non-employment. One possible explanation for the conflicting results is that Gruber's (2000) standard errors may have been too small since he does not adjust for the clustering in the data as discussed by Campolieti. Thus, this suggests that a difference-in-differences approach might not be particularly useful in practice for estimating the behavioral responses to social insurance programs. Nonetheless, this study shows that it is sometimes possible to convincingly use a DD approach using data from a unitary country – Sweden in this case. A particularly attractive feature of using data from a unitary country is that the institutional environment is the same, which greatly facilitates treatment-control comparisons. In contrast, studies using data from Federal countries must also take into account that there may be important differences in the institutional setting across States or Provinces.

The paper is organized in the following way. In the next section, we describe the TDI system in Sweden and the particular reform that will be used to estimate labor supply responses of sickness benefits. Section 3 discusses the empirical framework and the data while Section 4 presents the results. Section 5 summarizes and gives some concluding remarks.

II. Sweden's TDI Program

As discussed in the introduction, all OECD countries except one have TDI programs. As a service to the reader, we therefore provide an overview of TDI programs and their different characteristics for most of the OECD countries in the Appendix. We think that this overview is useful since TDI programs are under-researched relative to their economic importance.

In this section, we focus on the Swedish Sickness Insurance System. We first briefly describe the general features of the Swedish TDI program. Then, we turn to a description of the specific TDI reform in December 1, 1987 that will be used to estimate labor supply responses from changes in the benefit level.

Sweden has a compulsory publicly administered TDI program. During the period of study, it was publicly financed.[7] For the majority of workers, collective agreements often 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 employers. A physician's certificate is only required from the eighth day of temporary disability which, in practice, gives the worker full discretion of claiming benefits the first seven days. There was no time limit for how long benefits could be paid.

The Swedish TDI program has changed quite frequently over the past 30 years.[8] We will use a change in the TDI program that took place on December 1, 1987, to estimate labor supply or sickness absence responses. The aim of the 1987 reform was to increase the benefit replacement rate to 90 percent for short-term disabilities, i.e., those that lasted less than two weeks (see, e.g., Proposition 1986/87:69 and Ds S 1986:8). 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 very short period. This fact was considered to be unfair by policymakers and different methods for solving this problem had been discussed since the mid 1970s, which resulted in two government reports (i.e., SOU 1981:22 and SOU 1983:48). Nevertheless, it was not until December 18, 1986 that the government decided to increase the replacement rate for short-term disability. This was accomplished by abolishing the one-day-waiting period, and changing the way of calculating temporary disability benefits. The new TDI law came into force on December 1, 1987.

[7] From 1993, the TDI program has primarily been funded through a payroll tax levied on employers.

[8] See Henrekson and Person (2004) for a description of the major reforms of the TDI that have taken place in Sweden during the last 30 years.

All types of workers except *central* government workers were affected by the reform. The reason why central government workers were not affected by the reform was that the central government took advantage of the Social Security Act (1962:381). This Act made it possible for an employer (the central government in this case) to provide paid sick leave to its workers while the TDI benefits to which the workers were entitled were instead paid out to the employer (i.e., *arbetsgivarinträde*). As a result, the cash sickness benefits for central government workers were 92 percent of the current earnings both before and after the reform. In addition, 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. Thus, for all other types of workers, there was an increase in the sickness benefits. However, we are unable to compute an exact increase in the replacement rate for many of these workers due to the lack of information about their job characteristics and their collective agreements.[9]

An important aspect of the reform was that 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 for them to get the benefits. The reform was also extensively covered in the media: both by public television and by all newspapers. Consequently, the reform was very well-known and therefore, anticipating the results, it should not come as surprise that the labor supply effect is almost immediately noticeable. Another important fact about the Swedish TDI system is that all workers are required by law (Social Security Act 1962:381, chapter 3, §10) to report to the Social Insurance Agency that they are sick in order to receive TDI benefits.

Figure 1 shows the total amount of sickness cash (TDI) benefits (in fixed-prices) paid out each year during the period 1974-2002. During the period 1974 to 1987, on average about 30 billion SEK were paid out on an annual basis. However, in 1988 to 1990, there was a sharp

[9] Due to the pre-reform rules of TDI, the replacement rate for workers 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 or not etc. As a consequence of these job characteristics, the replacement rate could vary a great deal since the worker could be compensated even for non-working days (e.g., see the government report Ds S 1986:8). Many workers also received additional benefits from their employers as a result of collective agreements between unions and employers. Unfortunately, we are unable to compute an exact replacement rate due to the complexity of collective agreements.

increase in the amount paid out to slightly more than 30 billion.[10] This large increase was due to the reform on December 1 1987 which provided more generous sickness cash benefits. In 1991, there was a large drop in the TDI expenditures which was due to the reduced benefit levels induced by the TDI reform that came into force on March 1 1991. This nicely illustrates the problem with endogenous policy changes as discussed by Besley and Case (2000), since the reform in 1991 was the result of the sharp increase in spending on TDI. Consequently, it is doubtful whether Johansson and Palme (2005) and Henrekson and Persson (2004) have estimated a causal effect of cash sickness benefits on labor supply since both make use of this specific reform in their work.

Figure 1 about here

On the other hand, there is little evidence in Figure 1 that the reform on December 11987 was related to the previous level of expenditures on sickness cash benefits. In addition, as noted above, this reform was discussed for a long period of time and it was decided upon one year before it came into force (December 18, 1986) which, taken together, makes it less likely that the reform will be endogenous. Nonetheless, convincingly addressing problems with endogenous policy reforms requires that one has a comparison group which had the same trend in the outcome as the treatment group before the treatment. Fortunately, as will be clear below, central government workers (the control group) and the group of other workers (the treatment group) have strikingly similar trends in sickness absence. In fact, even the levels are similar which arguably makes central governments a compelling control group.

[10] The estimated expected increase in benefits was SEK 2 676 billion (Government bill 1987/87:69: *Om förbättrad kompensation vid korttidsjukdom och vid tillfälligt vård av barn*) but the actual increase was SEK 7 974 billion. Thus, the actual increase was about three times larger than expected.

III. Empirical Framework and Data

In this section, we describe our empirical identification strategy and the data to which this is applied. As discussed above, we will use a Difference-in-Differences (DD) approach where central government workers constitute the control group and all other workers make up the treatment group. Using individual data, a DD approach amounts to running a regression of the form:

$$Y_{igt} = \mu_g + \lambda_t + \pi Post_{gt} + u_{igt}, \qquad (1)$$

where i denotes individuals, g indicates groups and t time. μ_g is a group effect, λ_t is a time effect, and *Post* is a dummy variable taking the value of one for the treatment group after the reform, and zero otherwise. An estimate of π will be the difference-in-differences estimate of the reform effect.

For π to measure the causal effect of the policy change, it must be the case that: (i) time effects are common across treatment and control groups (parallel trend assumption) and that: (ii) the composition of both the treatment and control groups must be stable before and after the policy change (see, e.g., Blundell and McCurdy 1999). Recently, there have also been other important issues raised about the DD approach such as correcting the standard errors because the treatment indicator $Post_{gt}$ only varies at the group level (e.g., Bertrand *et al*. 2004, and Donald and Lang 2007) and functional form issues (Athey and Imbens 2006).

With our data, we can address most of the concerns about the DD approach. The data is a register-based longitudinal data set (Longitudinal Individual Data, LINDA) consisting of a large number of individuals that are representative for the Swedish population (the sample is about 3.3 percent of the population).[11] Our data includes all start and end dates of all individuals' spells of temporary disability during the period 1986 to 1991.[12] Thus, we have data from two years before and four years after the policy change, which makes it possible to allow for common group and time effects when computing the standard errors. The panel feature of the data also makes it possible to circumvent the problem with compositional bias since we know the individual treatment status before and after the policy change. In fact, there is almost no change in the treatment status.[13] Thus, we can simply ignore issues about

[11] See Edin and Fredriksson (2000) for a general description of LINDA.
[12] Due to the fact that the National Insurance System changed in 1992, it is not possible to go beyond 1991.
[13] Only around 1 percent of the workers changes treatment status from one year to another.

compositional bias.[14] Moreover, there is no problem with censored outcomes since we have all start and end dates of all spells. Below we describe our DD approach in more detail.

To begin with, as the outcome of interest, we will use the incidence of sickness absence i.e., Y_i=1 if individual *i* starts a *new* sick spell during a period of time, and zero otherwise. We will also estimate distributional effects, i.e., $1[Y_i > c]$ where *c* is the duration of sickness spells. By focusing on the distributional effects rather than the duration effect, we avoid the problem of selection bias as discussed by Angrist and Pischke (2009). In other words, the duration effect, i.e., what Angrist and Pischke label a conditional-on-positive effect, cannot be estimated without bias since the policy reform is likely to change the composition of the group with positive spells of sickness absence.

We will not use equation (1) since there is no way of correcting the standard errors with only two groups and two time periods. Specifically, Donald and Lang (2007) note that if the error term in regression equation (1) consists of a group-time error term δ_{gt}, i.e., $u_{igt} = \delta_{gt} + r_{igt}$, the OLS standard errors of (1) will be grossly understated.

To be able make the inference robust to common group and time effects, we will instead aggregate data on a monthly basis, i.e., the share of people that starts a new sick spell within a particular month.[15] Thus, if the spell started in a previous month and is still ongoing, this observation will not be part of this measure. Donald and Lang (2007) show that one can use a GLS approach, which is equivalent to OLS on aggregated data at the group-time level, as a solution to the clustering problem (Moulton 1986).[16] Thus, this is the reason why we use group-month data and estimate the following equation:

$$Y_{gt} = \mu_g + \lambda_t + \pi Post_{gt} + u_{gt}, \quad (2)$$

where $Y_{gt} = \Sigma Y_{igt}/N_g$ and $u_{gt} = \delta_{gt} + r_{gt}$. Since the error term includes the component δ_{gt}, group-month effects are considered in estimations and inference can be based directly on standard errors from this second-step estimation. As pointed out by Donald and Lang (2007), homoskedasticity 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,

[14] If compositional changes were important, this problem could be addressed with an IV method where pre-reform treatment status is used to construct instruments for post-reform treatment status.

[15] There will be almost no multiple observations on an individual's sickness absence spells within a month. This is due to the administrative rule which says that if an individual becomes sick again within a three-week period from the last sickness spell, it does not count as a new sickness spell.

the most efficient estimator is the unweighted OLS estimator. Nonetheless, even though we have taken the Moulton problem into account, u_{gt} may still be serially correlated. We will therefore difference the data across the two groups (where *g=1* represents the control group and *g=2* the treatment group) which results in the following *single* time series:

$$Y_{2t} - Y_{1t} = \mu_{2-}\mu_1 + \pi(Post_{2t} - Post_{1t}) + u_{2t} - u_{1t}, \tag{3}$$

which can be written in the following way:

$$\Delta Y = \mu + \pi Post_t + \Delta u_t, \tag{4}$$

where $\Delta Y= Y_{2t} - Y_{1t}$, $\mu = \mu_{2-}\mu_1$ and $\Delta u = u_{2t} - u_{1t}$. Note that the difference in the treatment indicator between the groups becomes an indicator taking the value of one after the reform (zero otherwise) since $Post_{1t}$ is always zero. Using this transformation, the estimate of π will be identical to an estimate from a fixed-effect model (where N=2 and T=72). When estimating equation (4), we will make the standard errors robust to any type of heteroskedasticity and serial correlation by applying the Newey-West estimator. Since we estimate (4) with 72 observations, these standard errors will have good properties.[17]

We estimate equation (4) using the LINDA data set for the years 1986-1991 matched with register data from the Swedish National Social Insurance Board which includes start and end dates for all sick spells.[18] The sample is restricted to the population of employed workers aged 20-64 in each year and with an annual labor income of at least SEK 6,000 in each year since this is the threshold to qualify for sickness benefit.[19] The final sample consists of around

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[16] Bertrand *et al*. (2004) also suggest that one should collapse the data to avoid the group error problem.

[17] Since the analysis is based on panel data and most individuals are observed over the whole period, compositional changes are a minor issue in this study. However, it is possible to apply a covariate-adjusted version of equation (4) by first estimating the probability of reporting sick based on individual data controlling for individual characteristics such age, gender and education and including group-month specific intercepts. To control for individual characteristics, equation (4) is applied on data consisting of these estimated group-month intercepts. Using this approach does not alter the results obtained in that study.

[18] All sick leaves starting before December 1987 are included, even those that continue their spell after the reform. Whether these spells are included or not do not quantitatively alter the results.

[19] Some of the central government workers, the control group, did not entirely belong to the employer insurance scheme. These workers (25 percent) were excluded since it is not clear whether they were affected by the reform. For the same reason, local government workers who were observed to be under the employer insurance scheme were excluded from the sample (around 7 percent). Local government-, white-collar- and blue-collar workers constitute the treatment group.

124,000-132,000 individuals, depending on the year, where 87-88 percent belong to the treatment group.[20]

Table 1 reports sample statistics (average monthly sick rate, age, annual labor income and sex) by treatment status for the 1987 data, the last pre-treatment year (December is excluded since this month belongs to the post-treatment period). The third column reports the normalized difference in average characteristics values by treatment status, normalized by the standard deviation of these characteristics. In general, a difference in means larger than 0.25 standard deviations is substantial (Imbens and Wooldridge 2008). Thus, according to this metric, there are small differences between the treated and the control group with the exception of labor earnings which just marginally exceed the 0.25 threshold.[21] Nonetheless, differences in average characteristics between treated and control may be problematic for a DD approach if such differences asymmetrically affect the outcome across the two groups, i.e., the parallel trend assumption would then be violated.

Table 1 about here

As a way of visually checking whether the two groups have parallel trends, Figure 2 plots the outcome variable – the monthly fraction of individuals who report absent due to illness – for the years 1986 to 1991, for these two groups. Although the two data series are very volatile, the control and treatment groups have strikingly similar trends in their outcomes during the two-year pre-treatment period (January 1, 1986 to November 1, 1987). Figure 2 also reveals that the outcome for the treatment group increases relative to the control group after the reform in December 1987 and constantly lies above the corresponding development for the control group. It also noteworthy that issues about the correct functional form in a DD set up as raised by Athey and Imbens (2006) will not be a problem here since the control and treatment groups do not only have similar trends in the outcomes but also have the same levels in the pre-reform period. The issue about functional form will only be a problem when the pre-treatment outcome levels of treatment and controls differ significantly.

[20] The number of individuals varies somewhat from year to year. In 1986 there are 123,507 individuals whereof 12 percent are treated. The corresponding figures for the remaining years are the following: 1987: 126,059 and 12; 1988: 127,708 and 11; 1989: 129,431 and 11; 1990: 130,303 and 11; 1991: 132,152 and 11.

Figure 2 about here

Figure 2 suggests that there is an effect of the increased benefit level on individual sick reporting behavior since the treatment group has a higher absence rate than the control group after the reform. To more clearly illustrate whether there is a treatment effect, we also plot the difference in the outcomes between two groups, i.e., ΔY, in Figure 3. Figure 3 once more shows that the two groups move in parallel, since the difference ΔY fluctuates around zero prior to the reform. Most important, shortly after the policy change (December 1, 1987), ΔY sharply increases to 0.2 and stays at this level during most of the post treatment years (Dec 1987 to Dec 1991). This means that the effect of the reform is about 2 percentage points. Since the average share who reported sick among the treated before the reform is 16.4 percent, this amounts to a 12 percent increase in the incidence of sick spells.

It is noteworthy by only looking at the single time series for the treatment group in Figure 2 that there does not seem to be any effect at all from this reform. This clearly illustrates the problem of using a before and after comparison. Since our data also includes the reform explicitly studied by Johansson and Palme (2004), we can graphically analyze whether there is a visible reform effect. This is illustrated in Figure 2 which shows that at the time of the reform on March 31, 1991 (the second vertical line), there is a clear drop in both series. Nonetheless, there are other equally large breaks in the series at other points in time which again casts some doubt on whether a before and after analysis is useful in practice.

Next, we estimate the quantitative reform effect and also establish to what extent the effects are statistically significant from zero.

Figure 3 about here

[21] We reported this normalised difference in averages instead of a t-statistics from difference in means test. Essentially, the t-statistic is equal to the normalized difference multiplied by the square root of the sample size. As such, the t-statistic partly reflects the sample size.

IV. Results

The estimated effect of the reform based on equation (4) using the incidence of sickness absence i.e., Y_i=1 if individual i starts a *new* sick spell during a particular month, and zero otherwise, is 0.018 and it is highly statistically significant (s.e.= 0.0020). Thus, the reform increased the share who reported sick by almost two percentage points which is an 11 percent increase from the average monthly share of the treated who reported sick prior to the reform (see Table 1). As outlined in the previous section, we are holding our results to a high statistical standard since we account for random group-time effects and any form of heteroskedasticity as well as serial-correlation.

Next, we turn to the estimation of distributional effects by, for each spell length, estimating the effect of the reform on the likelihood that a sick spell exceeds such a spell length. Figure 4 shows all estimates for lengths from 1 to 100 days combined. The figure reveals two important insights. The first insight is that the reform significantly increased the share of started spells that is between one and seven days. The second insight is that the reform decreased spells between eight and up to around fifty days. This supports the hypothesis that individuals tend to shorten their spells when it becomes relatively less costly to start a new spell, as previously noted.

Figure 4 about here

Since there are distributional effects, the net effect on the total number of days is ambiguous. The total number of days on an annual basis for the governmental- and non-governmental sector, respectively, is shown in Figure 5 (the underlying dependent variable is the total days per individual of spells started in a given year). Figure 5 shows that both groups have parallel trends in the total number of days before 1988 and that total days for the two groups seems to converge slightly after the reform. We have chosen to show the development of total days on an annual basis since the development of total days on a monthly basis is very jumpy and hard to interpret by visual inspection.

Figure 5 about here

To estimate the reform effect and account for correlated errors within groups and also allow for serial correlation, we again apply equation (4) to group-month averages on total days. Thus, the underlying dependent variable is the total days per individual of spells started in a month. The estimate is –0.06 and it is significant at the 3 percent significance level. On a yearly basis, this implies a reduction of around 0.7 days which is a decrease by just over 3 percent since the average total days for treated before the reform was 19.3. If we instead use the 12 observations in Figure 5 in the estimation, we obtain the same quantitative effect but the effect is now only significant at the 15 percent significance level. We would like to emphasize that the major point in this paper is that the reform made individuals start new spells to a larger extent but that ongoing spells became shorter which is clearly illustrated in Figure 4. Thus, the effect on total days is fairly small.

Gender-specific reform effects

It is interesting to estimate separate effects for men and women. Sickness absence behavior and labor supply elasticities are often found to differ across gender. For example, Johansson and Palme (2005) find that the elasticity of sickness absence with respect to the benefit level is higher for men. Figure 6 shows the monthly fractions of individuals who reported sick, separately for men (panel A) and women (panel B). The figure suggests that there is an effect of the reform on individual sick reporting behavior for both men and women. When estimating the reform effect based on equation (4), the effect is higher for men than for women. The estimated increase in the share of males who reported sick due to the reform is 2.2 percentage points which amounts to a 14 percent increase in sick reporting (the baseline is 15 percent). For women, the corresponding increase is 1.6 percentage points which amounts to an increase of 8 percent (the baseline is 20 percent).[22]

Figure 6 about here

As regards distributional effects, it seems that women redistributed sick spells to a larger extent than men (see Figure 7). This is also confirmed when estimating reform effects on total sick days. For men, the total days decreased by 1.5 percent due to the reform. For women, the

[22] We have also elaborated using different treatment groups in the estimations. For example, it might be argued that municipality workers are more similar to government workers. The results remain quantitatively unchanged when different treated groups are used separately.

corresponding decrease was 5.6 percent. Thus, it seems that men reacted more to the reform in terms of newly started sick spells but that they did not decrease the length of on-going spells to the same extent as women. One reason for this gender difference might be that it is more costly for women to have multiple spells in a waiting period since women, for example, are over-represented in the care sector where, in many cases, you are not allowed to go to work even in the case of a slightest cold.

Figure 7 about here

V. Conclusions

An important consideration for the design of insurance systems that provide workers with compensation for temporary non-occupational sickness or injuries, i.e., temporary disability insurance (TDI) programs, is the responsiveness of work absenteeism or labor supply to the generosity of benefits and waiting periods. The challenge when constructing the insurance is to balance the incentives to work and economic security. Despite the economic significance of TDI programs, there is limited knowledge about how workers respond to economic incentives within such a system. Moreover, estimating the behavioral effect has proved difficult. There are only a few previous studies of the effect of TDI benefits on labor supply but whether these studies have identified a causal effect can be questioned since there is a number of important limitations in their identification strategies.

In this paper, we provide credible evidence on the behavioral response from a policy change in Sweden which consisted of the abolishment of a waiting period of one day and an increase in the benefit levels for sick leaves shorter than 14 days. By exploiting this particular policy reform, we can overcome several of the problems with previous studies. Most importantly, since we have a control group of workers not affected by the policy change, we avoid the obstacle associated with a before and after analysis, which is basically the approach that has been used in previous TDI studies. Moreover, thanks to the data, we can also address most of the concerns about the Difference-in-Differences approach such as the parallel trend assumption, whether the composition of both the treatment and the control groups is stable before and after the policy change, and clustering in the data due to the fact that the policy only varies at the group level (an issue that has recently received increased attention). An additional advantage of the Swedish setting is that the institutional environment is the same which greatly facilitates treatment-control comparisons.

We find strong behavioral effects of the policy reform. The results show that the increase in sickness benefits caused a sharp increase in the share of new sick spells. There was also a large shift in the distribution of spell lengths which resulted in an increase in the number of short spells and a decrease in the number of long spells. The estimated net effect of the reform on the total number of days of sickness absence was a three-percent reduction. The negative impact of the reform on sickness absenteeism is perhaps not surprising given the fact that the change in the sickness insurance system made it less costly for a worker to be absent for short periods, since the policy reform consisted of the abolishment of a one-day waiting period and an increase in cash benefits for periods up to 14 days. In other words, if a worker faces some uncertainty about whether he or she will be sick again after a period of sickness absenteeism, the abolishment of the waiting period implies that after the reform, it was less costly to have multiple short sickness spells rather than having one long sickness spell only. This result is also confirmed when estimating gender-specific reform effects where the effect on a newly started sick spell was larger for men but that women tended to re-distribute sick spells to a larger extent. This result seems intuitive if it is more costly for women to have multiple spells in the presence of a waiting period which is the case if women are to a larger extent allocated in sectors where you are not allowed to go to work if the cost of contagion is high, such as the care sector. Thus, our finding suggests that the length of the waiting period and how the income replacement rates vary with spell lengths are likely to have important implications for the design of social insurance programs more generally.

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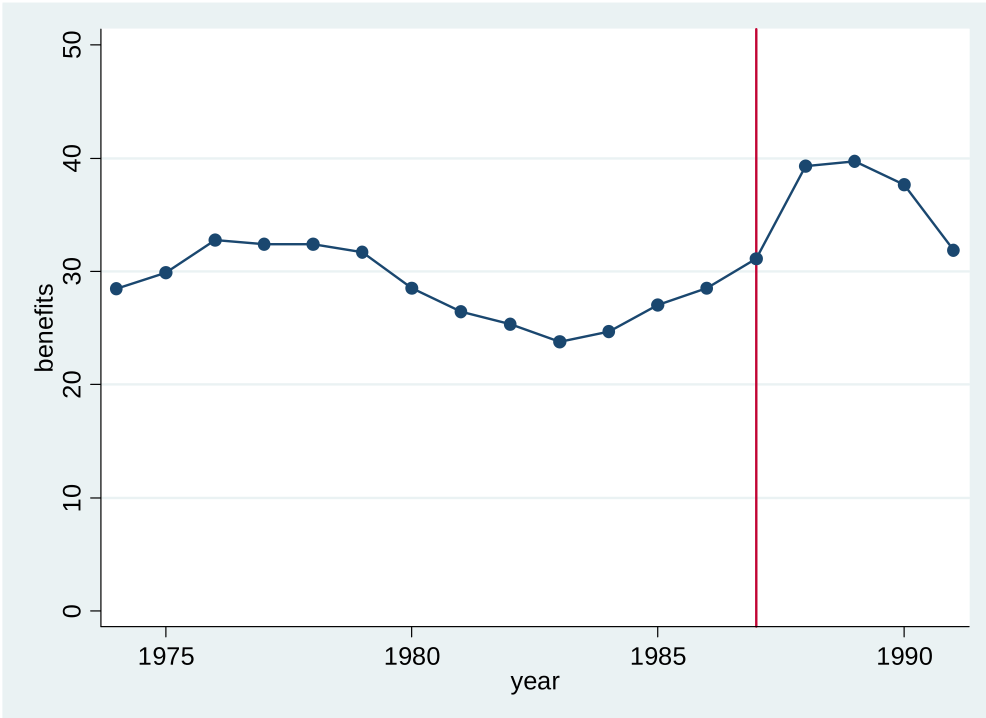
Table 1. Mean Characteristics by Treatment Status from LINDA 1987

	Treated group	Control group	Normalized difference
Average monthly share reporting sick (%)	16.4	16.6	-0.04
	(0.04)	(0.04)	
Age	38.9	41.5	-0.16
	(12.1)	(11.7)	
Annual Labor Income	100,003	121,159	-0.26
	(60,727)	(55,959)	
Female (%)	49.5	40.1	0.13
	(50.0)	(49.0)	
Number of individuals	111,486	14,573	
Percent of total	88	12	

Note. Treated are workers belonging to the local government sector and the private sector. The controls are workers belonging to the central government sector. The month of December is excluded. Standard deviations within parentheses.

Figure 1. Total Sickness Cash Benefits 1974-1991



Note. Benefits are measured in billion SEK at fixed prices (1991). Source: Social Security Administration.

Figure 2. Monthly Share of Workers Reporting Sick 1986-1991 by Treated and Control Group

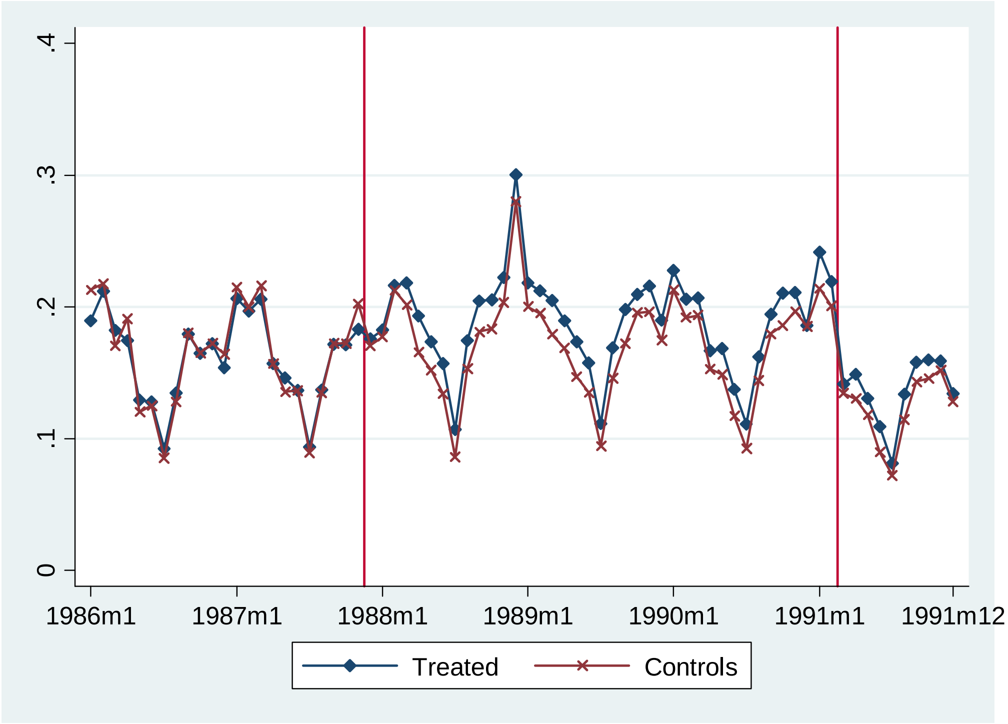


Figure 3. Differences between Treated and Control Group in the Monthly Share of Workers Reporting Sick

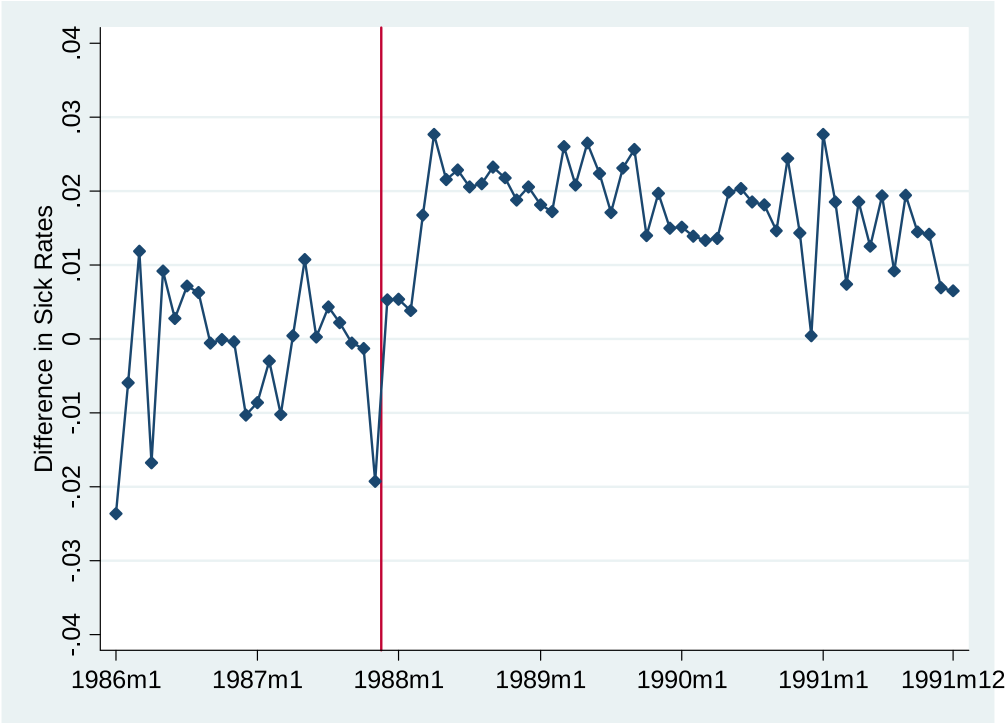


Figure 4. Estimated distribution of reform effects on the likelihood that a sick spell exceeds a certain spell length with 95 percent confidence bands

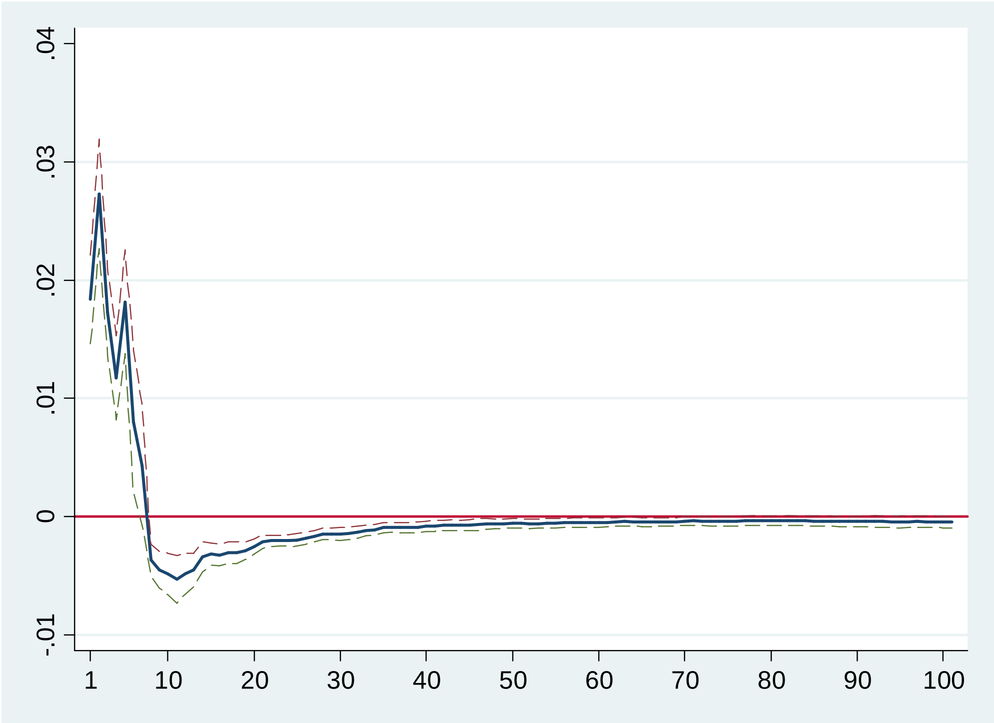


Figure 5. Annual total days per person 1986-1991 by Treated and Control Group

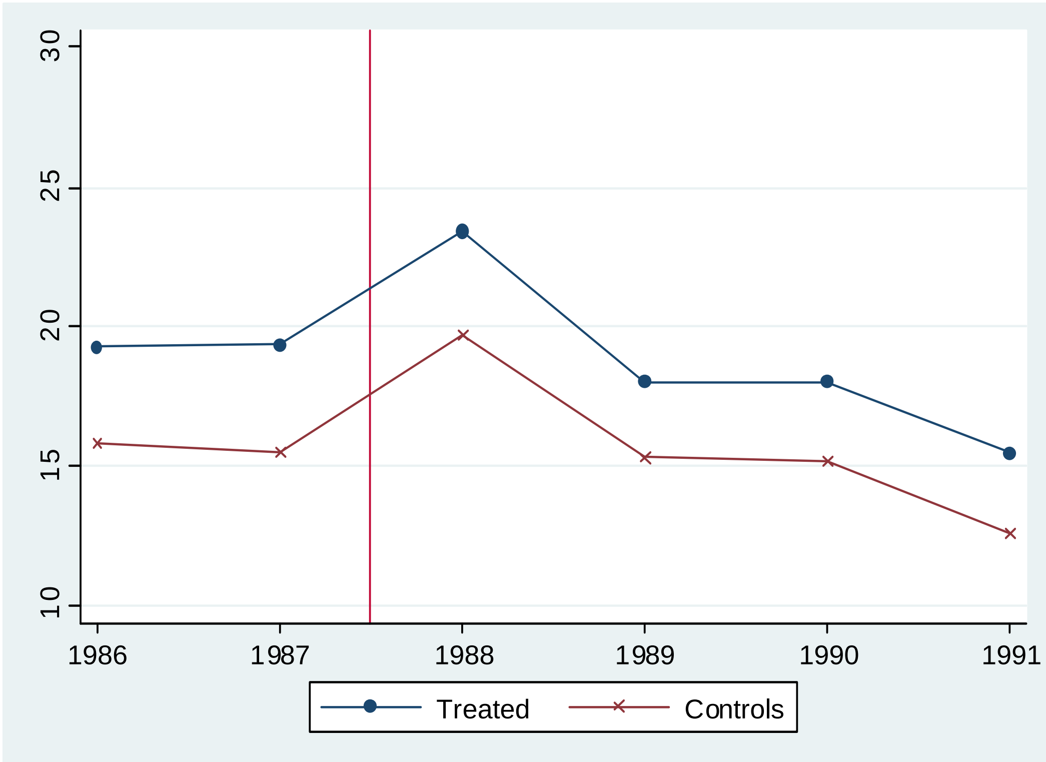


Figure 6. Monthly Share of Workers Reporting Sick 1986-1991 by Treated and Control Group, and separately by gender

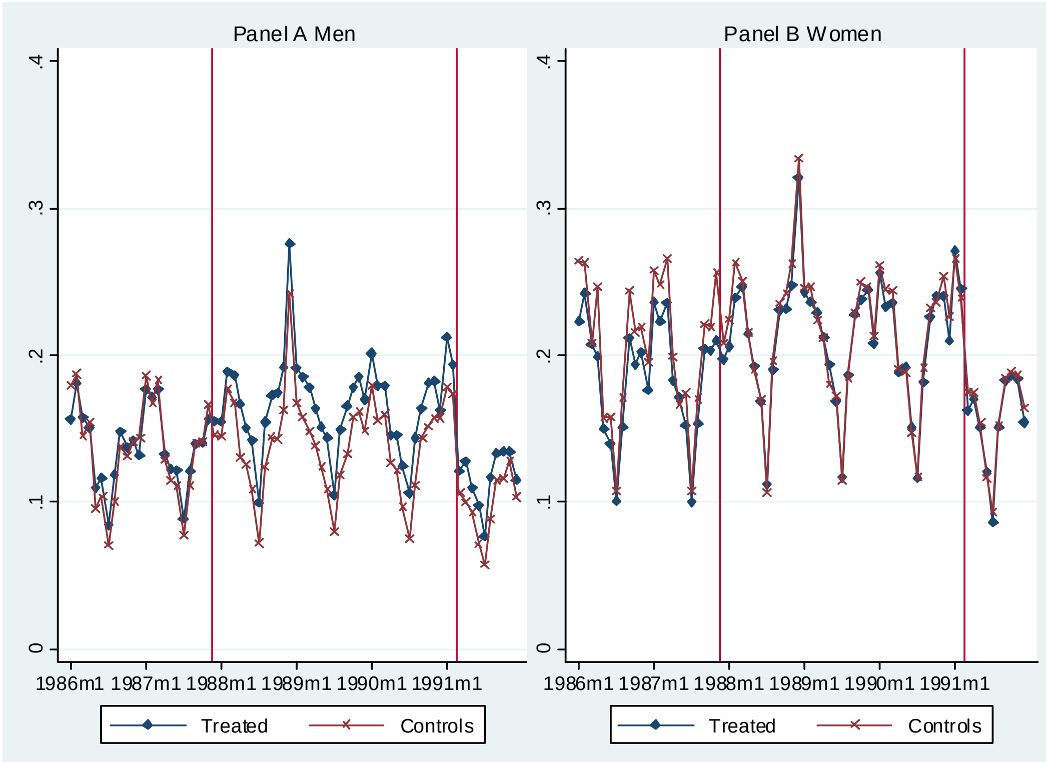
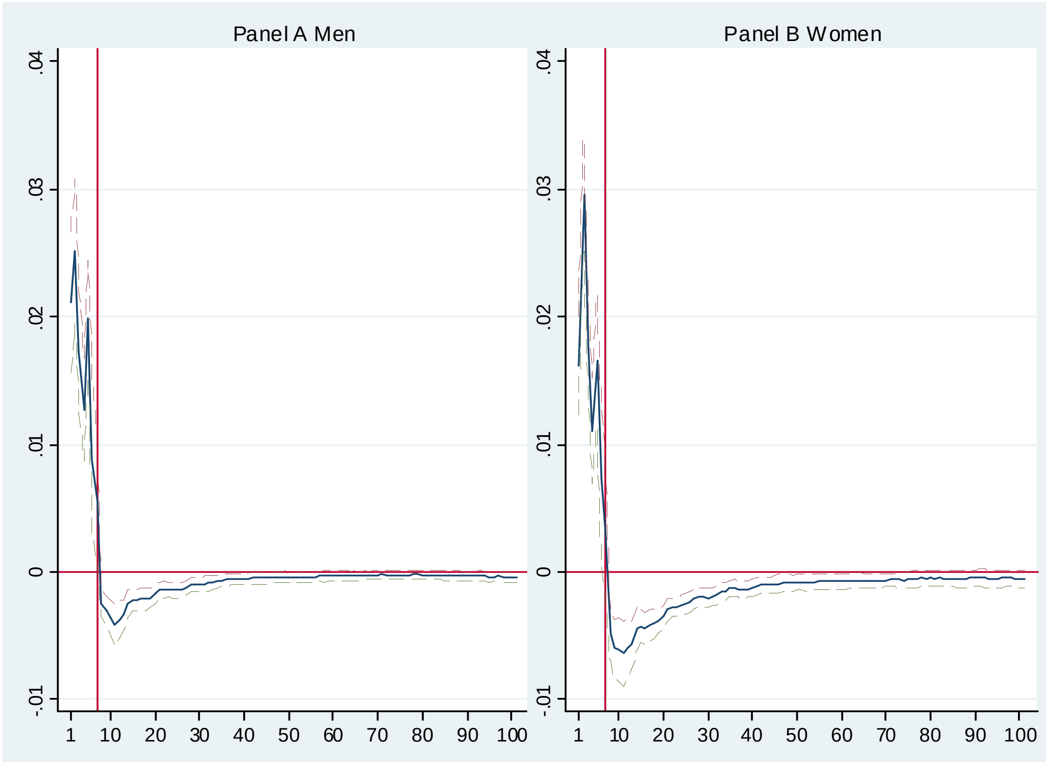


Figure 7. Estimated distribution of reform effects on the likelihood that a sick spell exceeds a certain spell length with 95 percent confidence bands, separately by gender



Appendix A

In this appendix, we provide an overview of the short-term sickness benefits system in the various OECD countries as a way of increasing the knowledge about this topic.[23] Almost all OECD countries have some official and universal form of Temporary Disability Insurance (TDI) or cash benefits to compensate workers in the event of temporary illness or injury that prevents them from working. To qualify for TDI benefits, workers must generally be unable to perform their regular work because of a physical or mental condition. Claimants must usually also have a specified amount of past employment or earnings to qualify for benefits. The system for compensation usually comes in one of two flavors: through a public system (i.e., TDI) or via a combination of an employer-financed initial phase, followed by a second phase that is paid by the national system. There is a large variation in program characteristics as can be seen from Table 1. The replacement rate, that is, TDI benefits as a ratio of foregone earnings, ranges from 50 (France, Italy and Turkey) to 100 percent (Norway and Luxembourg). However, the effective income replacement rate from short-term disability is often larger than the TDI replacement rates in Table 1. One reason for this is that benefits from the national TDI program are often topped up through collective agreements. For example, in the Netherlands, nearly all employees receive a 100 percent income replacement rate due to collective agreements instead of the statutory 70 percent rate. A second reason is that several countries have full wage replacement during an employer-paid period of several weeks or even months. A third reason is that the TDI benefits are not taxed in some countries. The bottom line is that it is quite difficult to calculate an average income replacement for short-term sickness that is comparable across countries.

The TDI programs also differ according to the waiting period. As a result, benefits may not be payable if an illness or injury lasts only a few days. Nevertheless, in many cases, workers will instead receive sick pay from their employers as discussed above. A waiting period of 2 to 7 days is typically imposed under most TDI programs as can be seen from column 2 in Table 1. Under some programs, however, benefits are paid retroactively for the

[23] This section is based on information from three sources, namely from the information provided by the Social Security Administration's publication Social Security Programs Throughout the World (http://www.ssa.gov/policy/docs/progdesc/ssptw/), the information provided by the Mutual Information System on Social Protection in the EU Member States and the EEA (http://europa.eu.int/comm/employment_social/missoc2001/index_chapitre3_en.htm), and the book *Transforming Disability into Ability* published by OECD.

waiting period when the disability continues beyond a specified time period, normally 2 to 3 weeks.

The period during which a worker may receive benefits for a single illness or injury also varies a great deal across countries as can be seen from column 3 in Table 1. The duration of benefits is typically limited to 26 weeks. In some instances, however, benefits may be drawn for considerably longer and even for an unlimited duration. A number of countries permit the agency to extend the maximum entitlement period to 39 or 52 weeks in specific cases. In most countries, when cash sickness benefits are exhausted, the recipient is paid a disability benefit if the incapacity continues.

Even if there is no federal program that provides income replacement for short-term disability in the United States, income maintenance is available through mandatory public programs in several States and also through a variety of private employment plans. [24] More specifically, three programs protect workers from this kind of income loss: (i) temporary disability insurance (TDI) programs in certain States, (ii) paid sick leave and (iii) employment-related group insurance. TDI provides workers with partial protection against the loss of wages due to nonoccupational disability. This protection is offered to workers in California, Hawaii, New Jersey, New York, Rhode Island, Puerto Rico, and the railroad industry (see Table A2).

[24] This section is based on the information provided the Social Security Administration, i.e., Social Security Programs in the United States (http://www.ssa.gov/policy/docs/progdesc/sspus/tempdib.pdf) and Kerns (1997).

Table A1. Program characteristics of TDI for OECD countries

Country	Income replacement rate	Waiting period	Duration
Australia	Flat rate (means tested)	7 days	n.a.
Austria	60	3 days	78 weeks
Belgium	60	1 days	52 weeks
Canada	55	14 days	45 weeks
Czech Republic	69	No	1-2 years
Denmark	n.a	No	52 weeks
Finland	At least 70	9 days	300 days
France	50	3 days	52 weeks
Germany	70	No	78 weeks
Greece	At least 50	3 days	182-720 days
Hungary	60-70	No	1 year
Ireland	n.a	3 days	No limited, or 52 weeks
Italy	50	3 days	26 weeks
Japan	60	3 days	18 months
Luxembourg	100	No	n.a
Mexico	60	3 days	52 or 78 weeks
Netherlands	70	No	52 weeks
New Zealand	n.a.	n.a.	n.a.
Norway	100	No	52 weeks
Poland	80	No	26 weeks
Portugal	65	3 days	1,095 days
Slovak Republic	55	No	n.a.
Spain	60	3 days	52 weeks
Switzerland	n.a.	3 days	720 days
Sweden	80	1 day	No limit
Turkey	50 or 67	2 days	52 weeks
United Kingdom	n.a.	3 days	52 weeks

Notes. The figures in the table are based on information from three sources, namely from the information provided by the Social Security Administration's publication Social Security Programs Throughout the World (http://www.ssa.gov/policy/docs/progdesc/ssptw/), the information provided by the Mutual Information System on Social Protection in the EU Member States and the EEA (http://europa.eu.int/comm/employment_social/missoc2001/index_chapitre3_en.htm), and Table A3.3 in the book *Transforming Disability into Ability* published by OECD.

Table A2. Program characteristics of TDI for US States

States	Replacement rate	Minimum weekly benefits	Maximum weekly benefits	Waiting period	Duration
California	55-60	50	$840	7 days	52 weeks
Hawaii	58	1	$418	7 days	26 weeks
New York	50	n.a.	$170	7 days	26 weeks
New Jersey	66.67	n.a.	$488	7 days	26 weeks
Rhode Island	75	63	$607-819	7 days	30 weeks

Notes. The figures in the table are based on information from the following web pages: Rhode Island: http://www.dlt.ri.gov/tdi, New Jersey: http://www.state.nj.us/labor/tdi/tdiindex.html, California: http://www.edd.ca.gov/direp/diind.htm, Hawaii: http://hawaii.gov/labor/dcd/abouttdi.shtml, New York: http://www.wcb.state.ny.us/content/main/workers/wc06003.htm.