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The sick pay trap

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ABSTRACT

The Sick Pay Trap^{*}

In most countries, employers are financially responsible for sick pay during an initial period of a worker's absence spell, after which the public insurance system covers the bill. Based on a quasi-natural experiment in Norway, where pay liability was removed for pregnancy-related absences, we show that firms' absence costs significantly affect employees' absence behavior. However, by restricting pay liability to the initial period of the absence spell, firms are discouraged from letting long-term sick workers back into work, since they then face the financial risk associated with subsequent relapses. We show that this disincentive effect is statistically and economically significant.

JEL Classification: C14, C41, H55, I18, J23

Keywords: absenteeism, social insurance, experience rating, multivariate hazard rate models

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1 Introduction

Based on extensive reviews of disability prevention experiences in 13 countries, OECD (2010, p. 125) argues that “employers are key players in preventing health problems at work and facilitating a swift return to work for people absent from work due to sickness.” But, while there is ample empirical evidence regarding the responsiveness of absenteeism with respect to worker incentives (Henreksson and Persson, 2004; Johansson and Palme, 2005; Ziebarth and Karlsson, 2009), there is little evidence regarding the impact of firm incentives. OECD (2010, p. 133) notes that countries where employers are responsible for a large share of their employees’ sick pay costs tend to have much lower absence rates than countries where employers can pass the costs on to the public purse, and also that absenteeism has dropped significantly in the Netherlands and UK after a shift of financial responsibility towards employers. Yet, to our knowledge, scientific evidence establishing a causal relationship between firm incentives and worker absenteeism is non-existent. The design of firm incentives with respect to sick-leave prevention also involves a potential tradeoff between sick-leave and labor market exclusion: While more extensive pay liability (or experience rating) improves incentives for absence prevention, it may at the same time undermine incentives for employing persons perceived to have a high risk of absence in the first place.

Although the extent of employer co-payment differs sharply across different countries, the incentive structure faced by employers in most industrialized economies typically implies that the firm is responsible for sick pay expenditures during an initial stage of a workers’ sick leave, but that the national insurance scheme (or another insurer) covers the costs accruing after some duration threshold (OECD, 2010, Table 5.1). This means that firms do have financial incentives to prevent short-term absences. However, in cases where absence spells stretch beyond the co-payment period, employers may (rightly) think that it is not in their interest to facilitate a quick return to work, since the return to work also entails the risk of new short-term absences for which the employers are again financially responsible. Hence, current incentives structures may have the unintended side-effect of discouraging employers from exerting appropriate effort to curb long term absenteeism, effectively trapping long-term absentees in inactivity.

In the present paper, we examine empirically the impacts of employers’ pay liability by exploiting a reform in the Norwegian sick leave insurance scheme, whereby pay liability for workers’ short-term sick-leaves was removed for pregnancy-related absences. The motivation for this reform was that it was feared that the elevated risk of sickness absence associated

with pregnancies made employers reluctant towards hiring young women. Markussen *et al.* (2011) show that the increased risk of absenteeism associated with pregnancies is indeed substantial; the hazard rate of entering into a sick-leave spell with a diagnosis predicting full exploitation of the employer's pay liability is raised by a factor of five at the onset of a pregnancy and further to a factor of 15 during the last 2-3 months before delivery. The reform thus clearly removed a potentially important disincentive with respect to hiring young female labor; but at the same time it also changed employers' incentives to prevent sick leave among pregnant workers. On the one hand, the reform made short-term absence less costly for the firms. On the other hand it also made it less risky to let long-term absent pregnant workers return to work, since the firms no longer were responsible for the costs associated with subsequent relapses. Hence, the reform offers a neat setting for identifying the impacts of firm incentives: If firms actually influence their employees' sick leave behavior and respond to financial incentives, we would expect short-term sick leave to rise and long-term sick leave to fall in response to the reform.

Based on a combination of regression discontinuity and difference-in-difference methodologies, we find that the reform had significant impacts on the affected employees' absence behavior. The hazard rate into sick-leave spells for which pay liability was removed rose by around 5 percent, while the corresponding recovery hazard dropped by around 4 percent, *ceteris paribus*. Furthermore, the recovery hazard from (longer term) spells that never were subject to pay liability rose by 9 percent, as the expected cost from new sick leave episodes was removed. All these results are exactly as one would expect on the basis of simple economic theory, provided that firms do have some influence on their employees' sick leave behavior. Our findings thus indicate that policy makers indeed may have good reasons to focus on improving employer incentives in their efforts to curb absenteeism. Restricting firms' pay liability to short-term absenteeism only may be a bad idea, since it has the unintended side effect of discouraging firms from helping long-term absent workers back into work. Interestingly, our findings suggest that the Norwegian system, with 16 days pay liability for firms, does not reduce overall absenteeism at all, compared to a system with no pay liability – despite that it allocates as much as 34 % of total insurance costs to the firms and that absenteeism is highly responsive with respect to firm incentives.

We also find some evidence indicating that the removal of pay liability for pregnant workers made young women more employable. According to our estimates, the school-to-work transition for young females was speeded up by around 6-8 percent as a direct result of

the reform. This suggests that policy makers may have to trade off incentives for sick-leave prevention against incentives for employing workers with high expected absenteeism in the first place.

2 Related literature

When an insurance scheme is troubled by moral hazard problems, efficiency considerations suggest that coverage should decrease with duration. This is studied extensively in relation to the design of optimal unemployment insurance, starting from the seminal paper by Shavell and Weiss (1979). The argument is simple: In the presence of moral hazard there is an inevitable trade-off between insurance and incentives. By reshuffling the benefit schedule to provide lower payments tomorrow and higher payments today, agents are given stronger incentives to search for jobs while their expected utility remains constant.

Many countries have adopted declining benefit schemes for the unemployed, most often in the form a single fall after some time in addition to an overall duration limitation (Cahuc and Zyllerberg 2004, p. 143). Maximum duration limitations are also typically in place for sickness insurance payments. However, there are also examples of sickness benefits that *increase* with duration. Johansson and Palme (2005) study an example from Sweden, where a time-constant sickness insurance payment of 90 percent of former earnings was replaced by a time-increasing payment schedule, with 65 percent replacement rate the first three days, 80 percent the next 77 days, and then 90 percent from day 80. Johansson and Palme (2005) found that the reform changed workers' behavior exactly as the altered incentives would imply. The cost of starting a sickness spell increased, and the incidence of new spells fell. The cost of continuing a newly started spell also increased, and the return-to-work hazard rose accordingly. However, for a worker having spent close to 80 days on sick-leave, the reform reduced the return-to-work incentives insofar as a positive probability was attached to the possibility of a relapse. And the return-to-work hazard fell after 80 days on sick leave. This example illustrates a potential benefit trap: Having reached the highest level of replacement, it is not particularly tempting to risk a return to the bottom of the replacement ladder.

In the present paper, we focus on the employer's incentives, rather than those of the employees. But, provided that employers influence their employees' sick leave behavior, the story is basically the same. If the employer is financially responsible for short-term absenteeism only, the firm obviously has incentives to prefer a single *long* absence spell over many

short ones. And when employees have been on sick leave long enough to have exhausted the firms' pay liability, the financial risk associated with possible relapses may convince the employer not to accommodate a quick return to work.

3 Institutions and data

All Norwegian workers are fully insured against sickness absence for up to one year, with a 100% replacement ratio. The general rule is that the first 16 days of each absence spell is paid for by the employer, whereas the social security administration pays for the remaining days, and also for subsequent disability benefits. If a new absence spell starts within 16 days after a previous spell was completed it is counted as a continuation of the previous spell. This implies that a new pay liability period for the firm is not triggered until the worker has been present for at least 16 days. The social security costs are covered through uniform payroll taxes; hence there is no experience rating. In April 2002, a reform was implemented implying that firms were entitled to exemption from the 16 days pay liability for pregnancy-related absences.

During periods of sickness absence, Norwegian workers enjoy a special protection against dismissals, implying that they cannot be dismissed on grounds that are related to their sickness.¹ After the one year absence period, however, the firm is allowed to lay off the absent worker with direct reference to the sickness. Hence, if an employer for some reason wishes to lay off a worker – but is prevented from doing so due to the general employment protection regulations – the incentives for facilitating that worker's return to work from a long-term absence spell is particularly weak.

Norway also has a high level of absenteeism. On a typical working day, around 7 percent of all workers are absent due to sickness. This places Norway among the countries with the world's highest sickness absence rates; see, e.g., Bonato and Lusinyan (2007).

The data we use in the present paper comprise complete longitudinal administrative records on employment and absence 2001-2005, merged with information on firms and workers on the basis of encrypted identification numbers. Absence spells are recorded insofar as they are certified by a physician. Such certification is normally required if a spell exceeds

¹ The burden of proof lies with the firm. In practice, this implies that absent workers can only be laid off as part of a mass displacement.

three days (eight days in some firms). This implies that the occurrence of very short absence spells is underreported in our data.

4 Empirical analysis

Our empirical analysis consists of two parts. We first examine the extent to which the removal of firms' pay liability for pregnant workers' sick leaves affected these workers absence behavior. We then investigate whether the reform affected young women's employment opportunities.

4.1 Absence behavior during pregnancies

To examine the impact employer incentives on absenteeism we construct the following dataset: We start out with all pregnant workers in Norway from May 2001 to December 2005, and follow them through work presence and work absence during their pregnancies. These workers constitute our potential treatment group. For each pregnant worker, we pick a female non-pregnant worker from exactly the same workplace, at exactly the same point in time, of approximately the same age, and with a similar earnings level.² These workers constitute our control group.

Table 1 and Figures 1 and 2 offer some descriptive statistics. There are around 94,000 pregnant workers in our dataset and a slightly lower number of controls (since some workers are controls for more than one treatment). Figure 1 illustrates that the rate of sickness absence has been fairly stable throughout our observation window for both pregnant and non-pregnant workers, except for seasonal fluctuations. The timing of the reform is marked in the figure as a vertical line. Based on a visual inspection of the figure, it is not easy to spot any indication of a reform effect whatsoever. A closer comparison of absence rates by pregnancy duration before and after the reform is provided in Figure 2. Panel (a) clearly illustrates the sharp rise in absence rates that typically occur as the pregnancy progresses, with absence rates as high as 60 % the last few months before delivery. Panel (b) reports the changes in absence rates from before to after the reform at different stages of the pregnancy for the members of the treatment group minus the corresponding changes for the members of the control group. According to these descriptive difference-in-difference estimators, the reform apparently raised absenteeism slightly at early stages of the pregnancy, while it lowered it at later stages (though

² We select the co-worker with the closest income level, provided that the age difference is less than three years. If we cannot find a female coworker with less than 3 years age difference, the pair is not included in our analysis.

Table 1. Descriptive statistics of data used to analyze absence behavior

	Treatment group (pregnant)		Control group (non-pregnant)	
	Before reform	After reform	Before reform	After reform
Number of persons	25 849	68 377	24 759	66 117
Characteristics (means)				
Age	29.9	30.0	30.1	30.2
Education				
<i>Compulsory or lower secondary</i>	14.9	12.8	16.1	14.2
<i>Upper secondary</i>	29.6	28.4	29.5	29.0
<i>College, low level</i>	42.7	44.2	41.6	42.8
<i>College, high level</i>	9.5	11.0	9.5	10.3
Earnings (deflated)	252 705	247 009	238 197	235 128
Non-European background (%)	5.0	5.3	4.3	4.9
Outcome (mean)				
Absence rate (Nov.-March)	27.5	27.8	7.5	7.4

the latter effect is not statistically significant). This pattern is consistent with the hypothesis that the reform raised the incidence of absence spells, but reduced their duration (since incidence is likely to dominate at early stages of pregnancies, while duration effects become more important as the time of delivery comes closer). The overall absence rate for pregnant women rose from 27.5% before the reform to 27.8% after the reform (adjusted for seasonal variation), while it dropped from 7.5% to 7.4% for the members of the control group; see Table 1. A simple descriptive difference-in-difference (DiD) estimate for the overall reform effect measured by this outcome is thus a modest increase of 1.4 percentage points. However, this small change could of course easily have been explained by factors other than the reform.

The fact that a visible reform effect does not show up in descriptive statistics does not necessarily imply that firms and workers did not respond to the reform. As discussed above, the reform not only reduced the incentives to prevent short-term absence, it also removed a *disincentive* with respect to preventing long-term absence. Hence, the overall effect on the absence rate is ambiguous. Given the rather subtle and potentially conflicting ways in which employer incentives were affected by removal of pay liability, with likely effects on the incidence as well as on the duration composition of absence spells – we set up a multivariate hazard rate model for transitions between the states of presence and absence, and identify the behavioral effects of interest on the basis of a difference-in-difference design. But before we turn to that model, we take a closer look at what happened at the exact time of the reform implementation by means of a regression discontinuity analysis. More specifically, we examine the aggregate rate of entry into absence among pregnant workers during a short time window just before and just after the reform, under alternative assumptions regarding the underlying

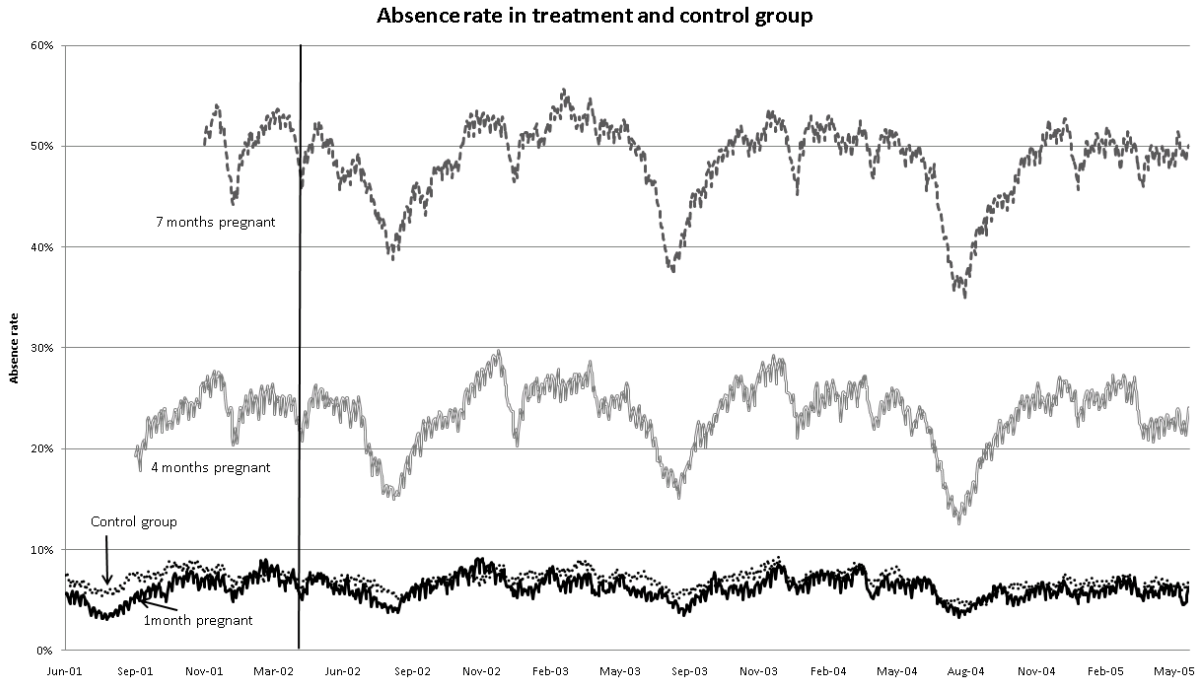


Figure 1. Daily absence rates 2001-2005 in treatment group (at different stages of the pregnancy) and in control group. Vertical line indicates timing of reform.

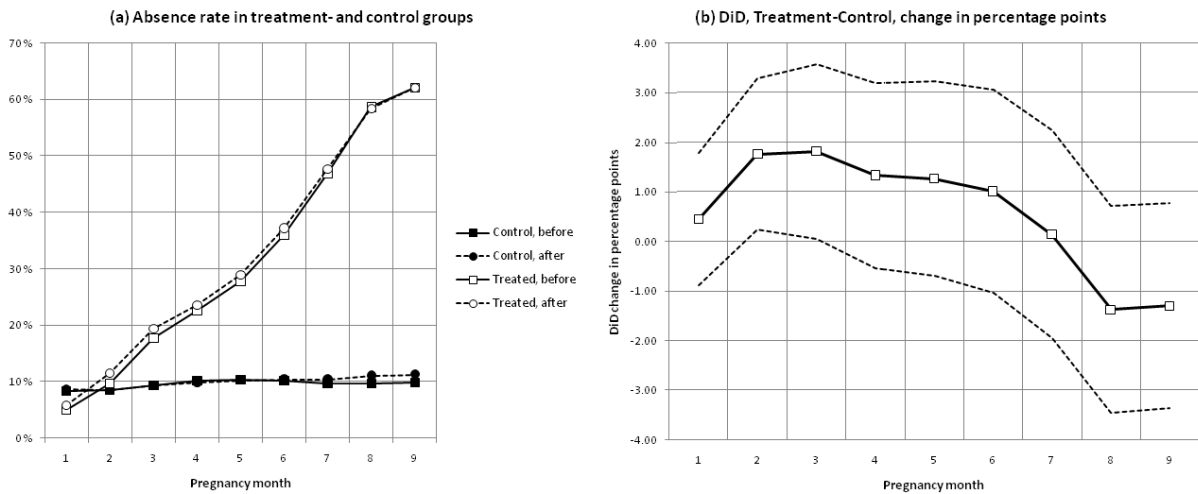


Figure 2. Absence rates by number of months into the pregnancy. Treatment and control group before and after the reform (with 95 percent confidence intervals).

Note: For control group members, pregnancy month refers to the number of months since they were picked as controls to a newly pregnant colleague. To prevent seasonal variation from disturbing the before/after reform comparison, the graphs compare workers that became pregnant in June or July 2001 (and their control group members) with those that became pregnant in June or July 2002.

time-pattern.³ Figure 3 illustrates the basic idea. Panel a) shows how the daily sick-leave entry rates among pregnant workers evolved from 90 days before to 90 days after the reform. Due to the seasonal pattern in absence behavior (recall that the reform was implemented on April 1), there is a declining time trend in this period. Imposing a linear calendar time function to control for this development yields a reform effect corresponding to the small upwards shift illustrated in the graph. Panel c) shows how the size of this estimated reform effect varies as we change the bandwidth (the number of days before/after the reform included in the analysis). The shorter the time window, the larger are the estimated effects, suggesting (perhaps) that there was a slight scope for firms/employees to “postpone” the starting date by a day or two just around the implementation of the reform. The effect is statistically significant, however, regardless of bandwidth. As a sort of “placebo test”, we examine in panel e) how the effect estimate changes if we assume alternative false implementation dates (with bandwidth -90,90). The graph shows that the effect estimates become smaller the further we move away from the true implementation date. Additional placebo tests can be obtained by repeating the whole estimation procedure on the control group. The results are illustrated in panels b), d), and f). The message coming out of these graphs is clear: There is no reform effect whatsoever in the control group.

A natural objection to the regression discontinuity results presented so far is that they may depend heavily on the linearity assumption for calendar time effects. Table 2 shows, however, that the finding of a positive reform effect is highly robust with respect to the specification of the underlying time-function. The table presents results for models with from one to four degree polynomials in a time function assumed to be the same before and after the reform, and also with from one to four degree polynomials in time functions allowed to shift from before to after the reform (in the latter case the reported reform-effect is the shift that occurred at the time of reform implementation). All the results are presented for three alternative bandwidths and for both treatments and controls. We interpret the results as convincing evidence that the reform indeed raised the frequency of absence spells among pregnant women. *All* the 24 alternative specifications produce significant positive reform-effects for the treatment group. *None* of them produce significant results for the control group of non-pregnant colleagues.

³ Daily entry rates are adjusted for weekdays, public holidays, and for first day after holidays.

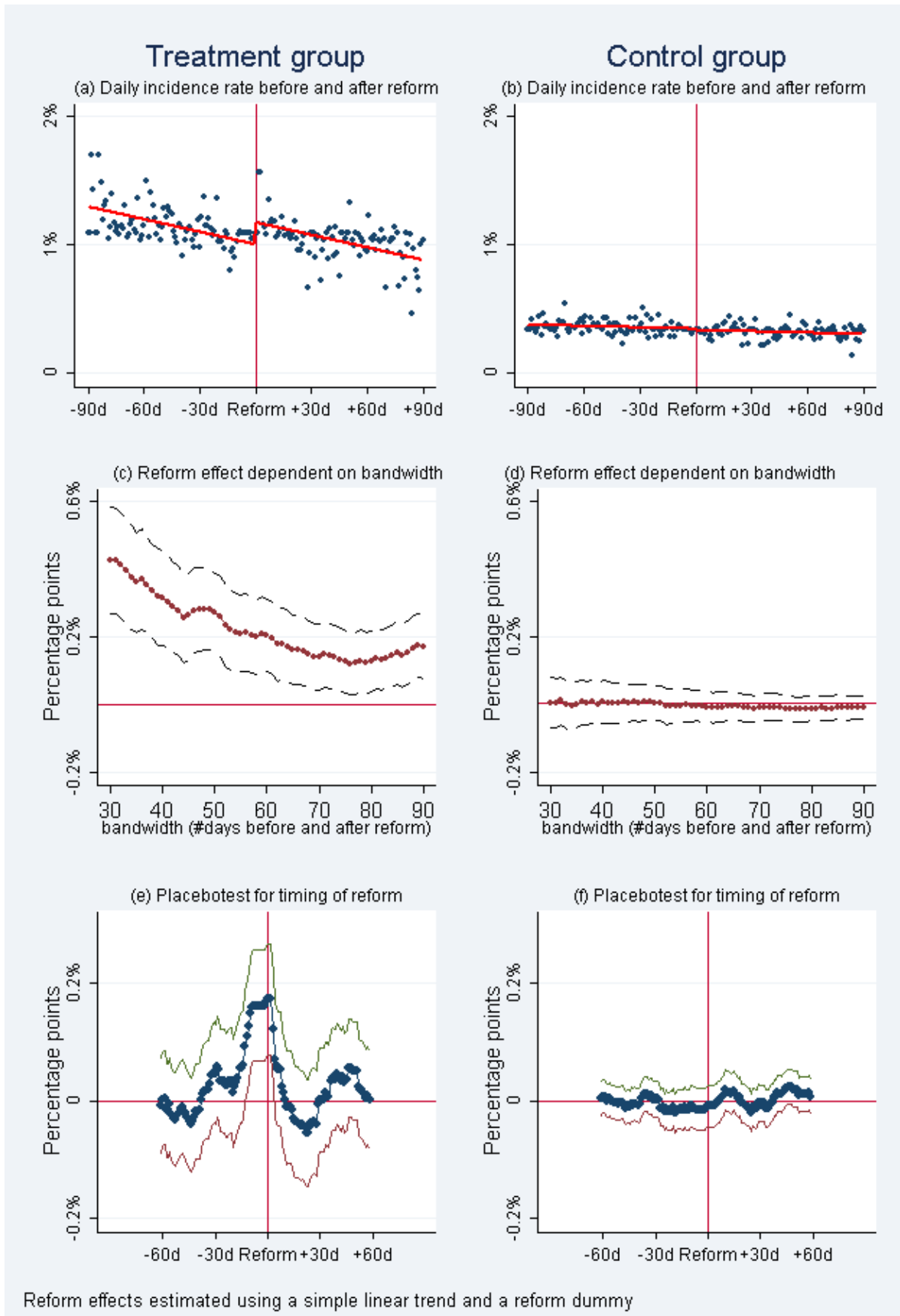


Figure 3. Regression discontinuity analysis. Estimated impact on absence incidence of removing firms' pay liability (with 95 percent confidence intervals).

Table 2. Regression discontinuity analysis: Estimated impact on absence incidence of removing firms' pay liability. By band width and time function specification (t-values in parentheses). Percentage points change.

Polynomial deg.	Common time function				Separate time functions before/after			
	1 st	2 nd	3 rd	4 th	1 st	2 nd	3 rd	4 th
Treatment group								
Bandwidth (days)								
[-30,30]	0.43 (5.26)	0.41 (5.18)	0.38 (3.21)	0.37 (3.10)	0.40 (5.14)	0.36 (2.78)	0.57 (3.34)	0.93 (3.87)
[-60,60]	0.20 (3.71)	0.20 (3.72)	0.41 (2.61)	0.33 (4.43)	0.20 (3.67)	0.37 (4.38)	0.34 (4.37)	0.41 (2.61)
[-90,90]	0.17 (3.57)	0.17 (3.57)	0.16 (2.33)	0.16 (2.29)	0.17 (3.55)	0.19 (2.37)	0.53 (5.02)	0.44 (3.22)
Control group								
Bandwidth (days)								
[-30,30]	0.01 (.16)	0.01 (.26)	-0.06 (-1.08)	-0.05 (-.91)	0.01 (.33)	-0.06 (-.97)	0.04 (.46)	0.15 (1.17)
[-60,60]	-0.01 (-.29)	-0.01 (-.25)	0.00 (.12)	0.00 (.07)	-0.01 (-.22)	0.00 (.06)	-0.02 (-.43)	-0.04 (-.59)
[-90,90]	-0.01 (-.53)	-0.01 (-.53)	-0.01 (-.25)	-0.00 (-.18)	-0.01 (-.51)	-0.00 (-.05)	0.00 (.10)	-0.02 (-.31)

We now turn to a more detailed analysis of the reform effects based on a hazard rate model. In this exercise, we exploit a much larger time window (2001.5-2005.12), and base our inference on differences in the differences between the treatment and the control groups before and after the reform. Let P be an indicator variable denoting potential treatment (pregnancy), let R be an indicator denoting that the reform has been implemented (after April 1, 2002), let D be an indicator denoting that the current state (presence or absence) has lasted less than 16 days, and let I be an indicator denoting that the last absence spell (if any) also lasted less than 16 days.⁴ Furthermore, let P_j , $j = 9, 8, \dots, 2$, be indicator variables denoting the number of months until expected delivery for those who are pregnant. We write the hazard rate of moving from presence to absence as

$$\begin{aligned} \theta_{it} = & \exp(x_{it}\beta_{1k} + v_{it} + \sigma_{1t} + \delta_{1j}P_j \\ & + \varphi_{11}D + \varphi_{12}D \times I + \varphi_{13}D \times R + \varphi_{14}D \times I \times R + \varphi_{15}D \times P + \varphi_{16}D \times P \times I \\ & + \alpha_{11}P(1 - D + D \times I)R + \alpha_{12}P(D - D \times I)R), \end{aligned} \quad (1)$$

and the hazard rate of moving back to presence (recovery) as

$$\begin{aligned} \theta_{2it} = & \exp(x_{it}\beta_{2k} + v_{2it} + \sigma_{2t} + \delta_{2j}P_j \\ & + \varphi_{21}D + \varphi_{22}D \times R + \varphi_{23}D \times P \\ & + \alpha_{21}P \times D \times R + \alpha_{22}P(1 - D)R), \end{aligned} \quad (2)$$

⁴ The duration of absence spells is measured inclusive of days belonging to previous absence spells that were terminated less than 16 days prior to the start of a current spell.

where x_{it} is a vector of observed covariates and (v_{1i}, v_{2i}) are unobserved covariates.⁵ Note that calendar time effects $(\sigma_{1t}, \sigma_{2t})$ are estimated separately for each month, implying that any general differences between the pre and post reform periods are absorbed by these effects. We also allow the effects of spell duration (less/more than 16 days) and the effects of recent absenteeism (more/less than 16 days) to vary between the treatment and the control groups and between the pre and posts reform periods. The reform effects $(\alpha_{11}, \alpha_{12}, \alpha_{21}, \alpha_{22})$ are then identified by the shift in pregnant workers' absence behavior from before to after the reform, relative to the control group members. An important assumption underlying this identification strategy is that the calendar time effects are the same for the treatment and the control groups. If pregnant workers have been subject to different time trends than non-pregnant workers for reasons that are not related to the reform, the estimated reform effects may be biased. We return to this issue below.

The model allows for four different reform effects. The first (α_{11}) captures the effect of removing the firms' pay liability on sick leave incidence. Since the reform clearly made sick leave less costly for the firms, we expect that $\alpha_{11} > 0$. The second effect (α_{12}) captures the effect of the reform on incidences for which there was no pay liability either before or after the reform. Since the reform did not change the incentives for these events, we expect that $\alpha_{12} \approx 0$ (the inclusion of this term may thus be thought of as a sort of placebo test). The third effect (α_{21}) captures the direct effect of removing the firms' pay liability on the speed of recovery during the first 16 days of absence. Since the reform made it less costly to let the workers stay away from work in this period, we expect that $\alpha_{21} < 0$. The final effect (α_{22}) captures the indirect effect of removing the firms' pay liability on the speed of recovery after 16 days of absence. Given that there was no pay liability after 16 days of absence either before or after the reform, the removal of pay liability did not affect firm incentives directly. However, since the removal of pay liability for shorter spells made it less risky for the firms to accept employees back into work (realizing that there is always a risk of relapse), we expect that $\alpha_{22} > 0$.

To avoid setting up a model for the initial state, we condition on workers being present to start with; i.e., workers enter into the dataset at the first occasion of presence. To derive the

⁵ The vector of observed covariates includes age (34 dummy variables, corresponding to age 16-49), county (19 dummy variables), income (15 dummy variables), and education/industry (15 dummy variables).

likelihood function for observed data, we split each individual's event history into parts characterized by constant explanatory variables and unchanged state. Let $S_{ki}, k = 1, 2$ be the set of observed spell parts under risk of event k (sickness, recovery) for individual i . Let l_{kis} denote the observed length of each of the spell parts, and let the indicator variables y_{kis} denote whether a spell part at risk of transition k actually ended in such a transition or was right-censored. Conditional on unobserved heterogeneity, the likelihood function for individual i can then be written as

$$L_i(v_{1i}, v_{2i}) = \prod_{s \in S_{1i}} [\theta_{1it}(v_{1ki})]^{y_{1is}} \exp(-l_{1is}(\theta_{1it}(v_{1ki}))) \\ \times \prod_{s \in S_{2i}} [\theta_{2it}(v_{2ki})]^{y_{2is}} \exp(-l_{2is}(\theta_{2it}(v_{2ki}))). \quad (3)$$

Since the likelihood contribution in (3) contains unobserved variables, it cannot be used directly for estimation purposes. This problem may be solved by formulating a model for the distribution of unobserved variables and then replace Equation (3) with its expectation. In the present context, however, it may be important to avoid unjustified restrictions on the joint distribution of the unobserved heterogeneity. It is probable that the unobserved characteristic affecting absence incidence is correlated to that affecting recovery, implying that manipulation of the inflow is also likely to change the distribution of unobserved recovery propensities in the stock of absentees. We thus model unobserved heterogeneity by means of a nonparametric approach. Under mild technical assumptions, results in Lindsay (1983, Theorem 3.1) and Heckman and Singer (1984, Theorem 3.5) ensure that for the purpose of maximizing the likelihood, the heterogeneity may be modeled by a discrete distribution without loss of generality. Let Q be the (a priori unknown) number of support points in this distribution and let $\{(v_{1l}, v_{2l}), p_l\}$, $l = 1, 2, \dots, Q$, be the associated location vectors and probabilities. In terms of observed variables, we write the likelihood function as

$$L = \prod_{i=1}^N E_{v_i}[L_i(v_{1i}, v_{2i})] = \prod_{i=1}^N \sum_{l=1}^Q p_l L_i(v_{1i}, v_{2i}), \quad \sum_{l=1}^Q p_l = 1, \quad (4)$$

where $L_i(v_{1i}, v_{2i})$ is given in Equation (3). Our estimation procedure is to maximize the likelihood function (4) with respect to all model and heterogeneity parameters repeatedly for alternative values of Q . The non-parametric maximum likelihood estimators (NPMLE) are obtained by starting out with $Q=1$, and then expanding the model with new support points until the model is "saturated" in the sense that it is no longer possible to increase the likelihood

function by adding more points. The preferred model is then selected on the basis of the Akaike Information Criterion (AIC). Monte Carlo evidence presented in Gaure, Røed and Zhang (2007) indicates that parameter estimates obtained this way are consistent and approximately normally distributed. They also indicate that the standard errors conditional on the optimal number of support points are valid for the unconditional model as well, and hence can be used for standard inference purposes.

Table 3. The estimated reform effects on absenteeism. Percent change in hazard rate (t-values in parentheses)

Effect on incidences for which pay liability was removed (α_{11})	5.13*** (3.54)
Effect on incidences for which pay liability did not apply either before or after the reform (α_{12})	2.74 (0.70)
Effect on recovery during period for which pay liability was removed (α_{21})	-3.92** (-1.98)
Effect on recovery during period for which pay liability did not apply either before or after the reform (α_{22})	9.31*** (3.38)
Number of estimated parameters	341
Number of mass-points in heterogeneity distribution	11

** (***) significant at 5(1) percent level.

The estimated reform effects based on the hazard rate model are presented in Table 3. The rate of entry into sick-leave spells that used to be subject to pay liability rose by around 5 percent (α_{11}), while the recovery rate from the same spells dropped by around 4 percent (α_{21}). Moreover, the rate of recovery from spells that were not subject to pay liability either before or after the reform rose by as much as 9 percent (α_{22}). Finally, the rate of entry into spells that were exempted from pay liability prior to the reform was unaffected by it. All these results are exactly as one would expect on the basis of economic theory. Firms responded to the new incentives by reducing efforts to prevent short term absence, for which they no longer faced any direct costs. At the same time, they apparently became less skeptical towards allowing long-term absent employees back into work, knowing that they no longer were responsible for the costs associated with relapses. It is of interest to note that the latter effect, according to our point estimates, is sufficiently large to imply that the removal of firms' pay liability actually caused the expected duration of a pregnant worker's absence spell to drop by 4-5 percent. Since it also raised the entry rate by around 5 percent, this implies that the effect on overall absenteeism of this particular reform was close to zero, consistent with the descriptive pattern conveyed by Figure 1 above. Hence, even though we have found that absenteeism is highly responsive to firm incentives, we also conclude that an initial pay liability period of 16 days – which on average implies that firms' cover around 34 percent of the absence costs for

pregnant workers (Bjerkedal and Thune, 2003) – does not reduce overall absenteeism to any noticeable extent. This suggests that making firms financially responsible for short-term absenteeism only is a highly inefficient way of structuring public sickness insurance programs.

As discussed above, the results in Table 3 are based on the assumption that young non-pregnant female colleagues constitute an appropriate control group for pregnant workers, in the sense that they were subject to the same time-variation in absence behavior *apart from the reform*. If pregnant workers changed their absence behavior relative to non-pregnant workers for reasons other than the reform, our estimates may be biased. Yet, while it is conceivable that idiosyncratic absence trends could explain the relative rise in entry rates and the decline in recovery rates among pregnant workers, it is hard to see why this should yield the particular twist in the recovery profiles captured by our estimates. We interpret the finding that the recovery rate declined during the first 16 days of absence (for which there used to be a pay liability period), while at the same time it rose at longer durations, as convincing evidence of a reform effect. To assess the robustness of our findings, we have also estimated a version of the hazard rate model with a separate linear time-trend for pregnant workers included in the model. What happens then is that the estimated reform-effects on entry and short term recovery (α_{11}, α_{21}) becomes somewhat smaller and statistically insignificant, whereas the reform-generated twist in the recovery profile remains equally large and statistically significant. Taken together, we view the totality of our results, based on both the regression discontinuity design and difference-in-difference design, as convincing evidence that employer's incentives do have a significant impact on employees' absence behavior.

4.2 Employment opportunities for young females

The firms' pay liability for pregnant workers' absences was removed for the purpose of making individuals conceived to have a high risk of becoming pregnant more employable. In this subsection, we evaluate whether the reform had this intended effect or not. Employers' scope for discriminating against pregnant or pregnancy-prone workers is limited insofar as workers already are employed. Hence, to the extent that the higher expected absence costs associated with pregnancies affect employment opportunities at all, they are likely to do so primarily through the hiring decision. We therefore start out with the population of not-yet-employed graduates in this analysis; i.e., we include in the analysis persons who ended their educational career in the period from 2000 to 2004 without already having got a job three months prior to graduation.

Table 4. Descriptive statistics of data used to analyze transitions to employment

	Treatment A		Control A		Treatment B		Control B	
	All young women		All young men		Newly married young women		All other young women	
	Before	After	Before	After	Before	After	Before	After
No. graduates	48 461	50 623	48 850	53 050	6 278	5 883	42 183	44 740
...included in sample after matching	27 609	30 283	27 609	30 283	6 017	5 661	6 017	5 661
...not employed 3 months prior to graduation	22 377	25 193	22 183	24 273	4 750	4 434	4 801	4 514
Age	24.1	23.7	23.4	23.2	26.7	26.8	26.7	26.8
Education								
<i>Compulsory or lower secondary</i>	23.9	27.9	24	28.8	21.9	25.8	21.7	25.1
<i>Upper secondary</i>	37.8	38.7	39.4	39.0	16.0	15.4	16.4	15.5
<i>College, low level</i>	23.9	19.6	22.3	18.7	46.9	42.4	46.6	42.7
<i>College, high level</i>	14.4	13.8	14.3	13.5	15.2	16.4	15.3	16.7
Non-European background (%)	6.9	7.5	7.9	8.0	11.9	13.6	5.1	6.0
Outcome								
Fraction employed								
...at graduation	4.0	3.6	4.1	3.5	4.4	5.5	4.5	4.9
...6 months after	34.3	28.2	31.0	24.7	45.4	41.6	47.8	41.2
...12 months after	42.1	35.5	37.2	31.2	52.0	48.3	55.9	51.0

We set up a duration model accounting for the transition to the first job. In this case, it is not obvious how the treatment group should be defined; and even less obvious how an appropriate control group can be established. Loosely speaking, the treatment group consists of women at risk of becoming pregnant, which from the employer point of view basically includes all young women. As one alternative, we thus define all young women (aged 19-34) to be in the treatment group, whereas we use men in the same age group as controls. For each female graduate, we pick as control a male student who completed the same education at the same point in time (based on approximately 650 education-graduation-time groups). Since men and women tend to choose rather different educations, this procedure has the unfortunate consequence that we lose approximately 42 percent of the observations due to the lack of an appropriate control person (we return to this issue below). The approach also suffers from the potential problem that men and women have been subject to different trends in the school-to-work transitions for reasons not related to the reform (although we do not see any particular reason why this should be the case, given that we have matched on education and graduation time). Hence, we also define a much more narrow treatment group, namely newly married women (less than four years ago) with less than 3 children, whereas we use a matched comparison group of other young women (aged 19-34) as controls. In this analysis, only a very small number of observations are lost due to lack of appropriate controls. But, while this latter

approach ensures more similar treatment and controls, it also suffers from contamination bias generated by the fact that even the controls are really treated to some extent, in the sense that employers may consider their probability of becoming pregnant as strictly positive. Descriptive statistics for the two treatment and control groups are provided in Table 4. The statistical model we use to examine the transition to and the duration of employment is similar to the one we used in the previous subsection to evaluate the transition to and the duration of sick leave. The school-to-work hazard is specified as

$$\theta_{3it}(x_{it}, v_{3i}) = \exp(x_{it}\beta_{3k} + \sigma_{3t} + \varphi_{3d} + \delta_3 T + \alpha_3 T \times R + v_{3i}), \quad (5)$$

where T is the treatment group dummy and φ_{3d} are monthly spell duration parameters, measuring the time since graduation.⁶ The setup of the likelihood function is completely parallel to Equations (3) and (4); hence, we do not repeat the derivation here.

Table 5. The estimated reform effect on the school-to-work transition. Percent change in hazard rate (t-values in parentheses)

	Treatment: Young women Control: Young men		Treatment: Newly married young women Control: Other young women	
	I 2000-2004	II 2001-2003	III 2000-2004	IV 2001-2003
Graduation period	2000-2004	2001-2003	2000-2004	2001-2003
Effect on transition to employment (α_3)	8.72*** (4.02)	6.47*** (2.24)	3.96 (1.00)	5.23 (0.94)
Number of estimated parameters	109	98	107	98
Number of mass-points in heterogeneity distribution	2	2	2	3

*** Significant at 1 % level.

Table 5 reports the estimated effects. The main results, reported in Columns I and III, seem to indicate that the reform did make it easier for “pregnancy-prone” graduates to obtain a first job. Using young men as controls, we find that the reform raised the transition rate into a first job by as much as 8.7 percent. The effect is significant, both from economic and statistical viewpoints. When we only include newly married women in the treatment group and use other young women as controls, the estimated effect drops to 4.0 percent and also becomes statistically insignificant. Given the contamination bias caused by the obvious pregnancy-risk among the controls, however, we should indeed expect a smaller effect estimate from this model. To evaluate the robustness of our findings, particularly with respect to the risk of diverging trends in hazard rates for reasons not related to the reform, we also estimate the two

⁶ The vector of control variables x_{it} for the male-female comparison includes separate age dummy variables for men and women (34 dummy variables, 17 for each gender corresponding to age 18-34), educational attainment (14 dummy variables), nationality (3 dummy variables), and indicators for pregnancy at three different stages before/after the reform (6 dummy variables).

models with a more narrow time window, i.e., 2001-2003. The results, reported in Columns II and IV, are very similar to the main results, although the statistical uncertainty obviously increases. Finally, given the large number of observations that were lost in the male-female comparison due to the matching requirements, we also estimate a model using the complete set of observations for young men and young women, where we, instead of matching on education and graduation-time, control for education/graduation-time by means of 632 additional indicator variables (and thus use all observations). Again, the estimates are similar to the main results (not shown in the table); for the complete sample, the estimated reform effect is 6.8 percent (t-value 3.8). For the reduced sample (2001-2003), the estimated effect is 6.6 percent (t-value 2.8).

In order to assess whether it became easier for *actually* pregnant workers to find work after the reform, we also included in the model indicators for pregnancy (at three different stages) and interacted them with the reform dummy variable. If there was an effect of the reform on employment opportunities for pregnant workers, we should clearly expect it to be largest in the beginning of the pregnancy, since there are then still many potential sickness months left to pay for that could be affected by the reform. The point estimates that came out of this exercise indeed indicated a small favorable reform-effect early in the pregnancy, whereas the effect turned negative towards the end of the pregnancy. But the standard errors were large for this exercise, and none of the estimates were statistically significant.

5 Conclusion

The findings reported in this paper show that employees' sick leave behavior is highly responsive towards the employer's wage costs during their absence. If the costs can be passed on to a public insurer, employees tend to be more absent than if the costs are paid for by their own employer. In most countries, the employer is responsible for the costs associated with short-term absence, while the public insurer covers the costs arising from long-term absence. We have shown that this system reduces the incidence of absence spells and also raises the probability of a quick recovery. At the same time, however, it also significantly reduces the recovery rate at durations exceeding the pay liability period. We conclude that responsibility for short-term absenteeism *only* undermines the firms' incentives to prevent long-term absenteeism; not only because they have little pecuniary incentives to avoid long-term absence per se, but also because a long-term absent worker's return to work entails the risk of costly short-term relapses (for which the employer is again financially responsible). As a result, employers

exert too little effort to prevent long-term absence. The evidence presented in this paper indicates that the unintended side effect of restricting pay liability to short-term absence spells only – in terms of raising longer term absenteeism – is substantial. According to the findings presented in this paper, they are sufficiently large to nullify the favorable impacts of a 16 day pay liability period for short-term absenteeism. Hence, despite making the employers responsible for a large fraction of overall sickness absence costs, pay liability for short-term absence achieves virtually nothing in terms of reduced overall absenteeism. We simply get less short-term absenteeism and more long-term absenteeism. Given that long-term absence spells is the typical gateway to permanent disability benefits in many countries (including Norway), insufficient employer incentives to prevent long-term absenteeism may be very costly from a social point of view.

Why, then, have so many countries designed their sickness insurance systems such that employers do not face *any* of the costs associated with long-term absenteeism? We see two possible explanations. The first is that absence behavior has typically been considered to be determined by the worker – not the firm. Hence, the focus has been placed on worker-incentives, rather than firm-incentives. Second, there may be significant administrative costs associated with reimbursing firms for the large number of short-term absences, implying that even a modest pay liability for longer absence spells is difficult to implement without at the same time raising the overall sick-leave costs for firms, and consequently undermine employment prospects for job seekers with high expected absence propensity. The results presented in this paper show that the first argument is not valid; firms' do have a lot of influence on their employees' absence behavior, and they respond to economic incentives. The second argument, however, is apparently valid. Our findings indicate that firms may respond to sick leave pay liability by being less willing to employ workers expected to be much absent due to sickness. Hence, by raising pay liability for long-term absences without at the same time reducing it for shorter spells, policy makers may undermine incentives to employ marginal workers. A possible solution to this dilemma may be to restructure firms' pay liability, so that they cover a smaller fraction of the short-term absence costs and a larger fraction of the long-term absence costs. If only transaction costs (associated with a larger number of insurance reimbursements) stand in the way of such a solution, one may hope that modern technology will reduce or even eliminate this obstacle.

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