How physicians can reduce sick leave

- evidence from a natural experiment

Simen Markussen[1]

Ragnar Frisch Centre for Economic Research

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Abstract

A Norwegian reform was launched on July 1st 2004, changing regulations for sick-leave certification by primary care physicians, and coincided with a drop in the sick-leave rate of more than 20 percent. Using individual sick-leave records matched with data covering primary care physicians, this paper evaluates this reform and concludes that it reduced sick leave by 23 percent. The drop in sick leave is decomposed and linked to the various reform elements. Measures targeted at physicians and aimed at reducing sick leave may be cost-effective and – as opposed to reductions in workers' replacement rates – politically easy to implement.

Keywords: Labor market policies, sick leave, physicians' gatekeeping

JEL classification: H53, I18, J28

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1. Introduction

On July 1st 2004 Norwegian authorities launched an apparently minor reform, changing the regulations for physicians' sick-leave certification while leaving economic incentives for workers, workplaces and physicians unchanged. At the same time, sick leave fell by around 20 percent, constituting a 1.34 percentage point increase in labor supply, as shown in Figure 1. This is a lot compared to any sickness insurance reform, and if a causal link can be established between the reform and the sudden drop in sick leave, the reform should be relevant for researchers as well as policy makers.[2]

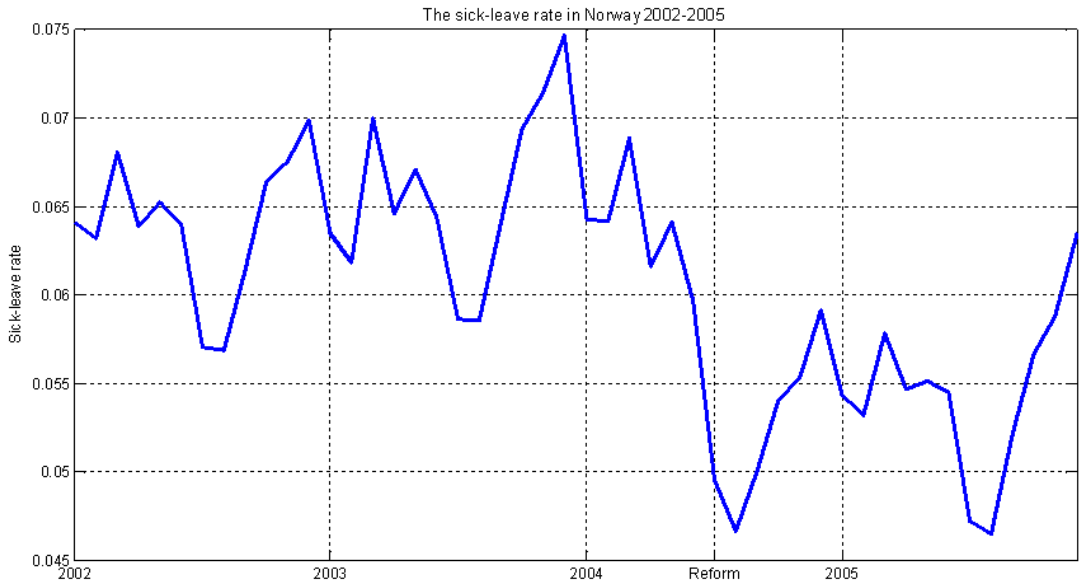

Figure 1: The sick-leave rate per month in Norway 2002-2005

The figure shows certified sick leave as a fraction of contracted working time per month.

Sources: Norwegian administrative data (sykefraværsregisteret fra NAV) and own calculations.

[2] For comparison: Johansson and Palme (2005) evaluates a Swedish reform where the replacement rate for sickness was cut from 90 percent to 65 and 80 percent, depending on spell length, and report a reduction of .93 percentage points, Lindbeck et al. (2006) studies the effect of reduced employment protection and find that sick leave was reduced by 0.2-0.3 days per year (around 0.1 percentage point).

The Norwegian reform was built on the notion that inactivity (not working) is unsuitable as treatment for many kinds of illnesses, such as back pain or mental illness. Therefore, this reform was "activity oriented" in the sense that primary care physicians were instructed specifically that workers should remain active unless their medical status clearly made working impossible or not recommendable. To promote *activity as treatment*, physicians were instructed to encourage the use of part-time sick leave for workers with health problems and, if the worker had not performed any work-related activity after eight weeks of sick leave, to file a report to the Social Security Administration stating why – on medical grounds – inactivity is necessary. The reform is presented in detail in section 2.

Primary care physicians and sickness certification

Primary care physicians play an important role as guardians of the public purse in many welfare states (Scott, 2000). In Norway they are asked to certify sick leave, disability, referrals to specialist treatment, drug prescriptions, driver's-licenses and more. In this respect, physicians are gatekeepers to a substantial part of publicly provided welfare.

There is existing literature on physicians' gatekeeping, both in health economics and public health (Scott, 2000; Brekke et al., 2007; Dusheiko et al., 2006). Typically, the physician-patient relationship is seen as one of agency, where "patients (principals) are less informed than physicians (agents) about the relationship between health care and health status" (Scott 2000, p.1178). Some describe physicians as "double agents"; serving two lords at the same time – their patients and the insurance provider (Blomqvist, 1991). This has led researchers to analyze the potential tension

inherent in physicians being both their patients' advocate and the welfare state's gatekeeper (Lindes, 1988).

Serious concerns have been raised over physicians' ability to serve as gatekeepers (Carlsen and Nyborg, 2009; Stone, 1986). The typical argument is that the private nature of health information makes it impossible for physicians to observe the true health status of their patients. When a patient claims to suffer from an unobservable illness, the physician must choose whether or not to trust his patient's self-assessment. Most physicians will be more eager not to reject genuinely sick patients than to reduce moral hazard (Stone, 1984, p.150). This has led authors to conclude that the gates are virtually wide open (Carlsen and Nyborg, 2009). Note that the agency problem has now changed. Following this line of thought, patients are the ones with private information (agent) and the insurance provider is the principal. The physician serves as a screening mechanism – a lie detector – to reduce the informational asymmetry.

Markussen et al. (2009), with the help of several variance decomposition exercises, report that physicians differ in "strictness". They find that moving from the 10^{th} to the 90^{th} percentile in the distribution of physicians' strictness increases the average employee's absence rate by 58 percent. Large variations among physicians are also found by Wilkin (1992) regarding referrals to specialist treatment. The fact that there is variation indicates that gatekeeping is possible. However, we do not know whether the variation is mainly due to permanent differences in practice style between autonomous physicians, which would be almost impossible to change by policy, or if physicians actually can be instructed to adjust the strictness of their gatekeeping.

Question, method and summary of the results

The purpose of this paper is to investigate how the reform launched on July 1st 2004 affected sick leave, in order to provide policy makers and researchers elsewhere with new knowledge. However, the paper also addresses the question of whether physicians can actually act as gatekeepers in the first place, and whether their gatekeeping can be influenced by public policy. Since the reform is entirely physician-directed, it would have no effect if gatekeeping were impossible. Finally, this paper investigates *which physicians* were, and which physicians were not, affected by the reform. In particular, it is interesting to test if factors that can be influenced by policies, such as physicians' business conditions (e.g. number of patients or payment schemes) or whether physicians work alongside others in medical centers or on their own, can help explain physicians' reform responses.

Unfortunately the reform was not designed as a social experiment with randomized control and treatment groups. All physicians and workers were affected at the same time and by all elements in the reform package. We are thus left to (1) compare various aspects of sick leave just before and just after the reform under various assumptions regarding trends and seasonal patterns, (2) examine alternative explanations for the sudden changes, (3) study the *exact timing* of these changes and (4) assess the nature of these changes in detail to see whether they can be directly related to the different reform elements.

The results show that the reform caused a reduction in sick leave spell length and frequency. In total, the reform reduced total sick leave by as much as 23-24 percent. Perhaps most importantly, return probabilities from full-time sick leave improved during the first 8 weeks of each spell, leading to a persistent reduction in total sick leave of more than 10 percent. This suggests that the extended

documentation requirements imposed on physicians really had a substantial impact. The robustness checks also indicate that the increase in return probabilities occurred at the time of reform implementation, supporting the notion that these results are in fact reform effects. One possible explanation why this measure had such an impact is that it put weight behind physicians' arguments. Tired of patients demanding sick leave – reducing them to "social benefit providers" – physicians could now point to the reform and their hands being tied by the extended documentation routines, and encourage a return to work.

The reform also encouraged physicians to "phase out" the use of an arrangement called *active sick leave* whereby workers could choose when to work and when to be absent. The Social Security Administration paid the worker's wages – not the employer. The duration of these spells tended to be very long (around 200 days on average) and misuse was suspected as the incentives for ending these spells were small for workers as well as employers. The reduction in active sick leave also led to a reduction in total sick leave of more than 10 percent.

As it turns out, there was a substantial heterogeneity between physicians in how they responded to the reform. Physicians with specialist education (specialization in general practice) responded less to the reform than others, while physicians located together with other physicians, i.e. in medical centers etc., managed to cut down more on sick leave than others. Physicians' age or gender seems to have no bearing on their response. Interestingly, physicians who had not been allocated their desired number of patients were substantially less responsive to the reform than those who had. The absolute number of patients is also relevant, as physicians with many patients were less responsive to the reform. This is relevant in policy terms, as it suggests that the Norwegian market based system for patient allocation has certain undesired side-

effects. The physicians' gatekeeping role may come into conflict with their business interests. This *externality* of the market based system should be weighed against any benefits from it, such as improved service, freedom of choice and reduced waiting periods. Limiting the maximum number of patients per physician will reduce the number of patients for physicians who have many, and – since these patients have to get another physician – make patient shortage less of a problem for the remaining physicians. Taking the results in this paper at face value, such a policy may pave the way for a further drop in sick leave.

2. The reform and economic environment

The Norwegian sickness insurance system

Almost all Norwegian workers enjoy full wage compensation during sick leave for a period of up to one year. During the first 16 days of absence, the expenses are covered by the employer, after which the social security system foots the bill[3]. The general rule is that absence spells lasting more than three days must be certified by a physician, although certification is not required until the 9th day for employees in firms participating in the so-called IA-agreement.[4] Frequently absent employees, whose quota of self-reported sick leave is used, need certification from the first day of absence Workers can be absent for a maximum period of one year. If a worker does not return to work after a year of sick leave, the employment contract is terminated

[3] There is an upper limit to the sickness insurance benefits paid out by the social security system (corresponding to a yearly income of around 60,000 USD), but employers typically cover the gap between the maximum social security payment and the employee's normal earnings.

[4] Workers employed by firms not party to the tripartite agreement ("Agreement on an Inclusive Labor Market (IA)") between the government, trade union and employers' organization are entitled to three non-monitored days of sick leave before they must visit a physician to get the spell certified. Workers in firms that are member of IA have eight such days per spell. In 2004 around half of all employees worked in such firms.

and the worker is entered into a rehabilitation program and may eventually apply for disability.

The reform

On March 26th 2004, the Norwegian cabinet proposed a change in the legislation regulating sickness insurance. Formally, the reform consisted of six parts, all of which are presented below.[5] Several of these parts are, however, fairly vague and/or they were also in place prior to the reform (see below for a brief discussion).

(A) Documentation and evaluation of work ability: Physicians were explicitly required to evaluate whether workers were able to perform work or work-related activities when sickness certificates were discussed. Physicians were, however, already required to do this prior to the reform. The change merely consisted of this being explicitly required by law. The sickness certification-form was also slightly amended in this regard.

(B) Part-time sick leave as default treatment: Physicians were instructed to consider part-time sick leave as the "default treatment", so that workers with "some" work ability would work part-time.

(C) Extended sick-listing documentation for long-lasting inactive spells: Physicians were required to provide an extended (medical) certification for workers with absence spells lasting longer than eight weeks if no work-related activities were performed, documenting that inactivity was part of the treatment.

[5] This section is based on Ot.prp nr.48 2003-2004, a proposal from the Ministry of Social Affairs to change the law regulating the social security system (folketrygdloven).

(D) Targeting the "active sick leave" system: An alternative to ordinary sick leave is so-called "active sick leave" (AS). AS lets the employee – when sick – choose whether or not to work, and how much, on a day-to-day basis. Regardless of whether the employee worked or was absent, he received sickness benefits from the social security administration. As a general rule it was meant not to last longer than 12 weeks. However, in practice it was widely used and the spells lasted for a long time (around 180 days on average). It was thought to be frequently misused as the physician, the employee and the employer lacked an incentive to terminate the absence spell. The reform restricted the use by giving part-time sick leave preference over AS if the employee was able to perform *some* of his regular tasks. The length of AS was also restricted to four weeks as a general rule, and eight weeks in exceptional cases.

(E) Slight reduction in self-reported absence days: The Norwegian system gives employees a quota of four times three days of self-reported absence days, i.e. a certification from a physician is not required. Prior to the reform, employees who received a certificate on the fourth absence day did not use their "quota" of self-reported absence. The reform changed this so that self-reported days prior to certified absence counted as self-reported absence.

(F) Sanctioning physicians who violate regulations: The law authorized revoking a physician's "privilege" of certifying sick leave and other social security related conditions such as disability etc., if the physician repeatedly failed to comply with the rules and regulations.

In my view, the reform has two potentially important implications for sick-leave certification. The first is that physicians are asked to use part-time sick leave as a default and only consider active sick leave in particular cases. Several papers have shown the importance of default alternatives, in as different settings as organ donation decisions (Johnson and Goldstein, 2003; Abadie and Gay, 2004), car insurance plan choices (Johnson et al., 1993), car option purchases (Park et al., 2000), consent to receive e-mail marketing (Johnson et al., 2003) and retirement savings (Beshears et al. 2006). Thus, it is plausible that changing the default treatment may affect physician behavior.

Also of potential importance is the requirement under (C) whereby physicians must provide medical documentation justifying the lack of work related activities if workers have been on sick leave for eight weeks. Providing such documentation on false grounds can be uncomfortable for both physician and patient. Knowing that such documentation must be provided after eight weeks may affect a physician's behavior, not only when a full-time spell has lasted eight weeks, but in general as well.

Timing and implementation

According to the legislative proposal, Ot. prp. nr 48 (2003-2004), preparations for the reform commenced in December 2003. On February 13th 2004 a draft was sent to the relevant organizations and interest groups for a public hearing. The deadline for responding was March 5th. The final legislative proposal was sent to the Parliament on March 26th, and approved on June 18th 2004. The reform was to be implemented on July 1st 2004. The legislative amendment was not the subject of much parliamentary debate, and it was approved unchanged.

Prior to the reform there was some public debate. As far as I know, the first mention of the reform in the press came after a public lecture for medical students in Trondheim on February 2nd 2004. The reform caught some media attention throughout the spring of 2004.

Following the new regulations, a new sickness-certification form was introduced on July 1st. Illustratively, the new form was entitled "Medical evaluation of work ability during sickness". The new form changed the default outcome from being sick-listed full-time to remaining in work – either full-time or part-time – but with certain workplace adjustments to enable this. This form included (new) questions such as: - *Which measures should be taken such that the worker can remain in part-time work/a work-related activity? - Suggestions, if any, for practical adjustments in the workplace? - Are there any important medical or treatment related reasons why the person can not participate in work-related activities?*

The economic environment

There is a large literature showing that sick leave tends to be pro-cyclical (Arai and Skogman Thoursie, 2005; Askildsen et al., 2005; Nordberg and Røed, 2006). Fortunately for the evaluation of this reform, business cycle conditions around reform implementation were stable. From 2000 to 2003 employment rates dropped and the unemployment rate increased. 2003, 2004 and the spring of 2005 saw only minor changes in employment and unemployment, before the Norwegian economy entered a boom starting in the autumn of 2005. Employment and unemployment rates are displayed in panels (b) and (c) in Figure 2.

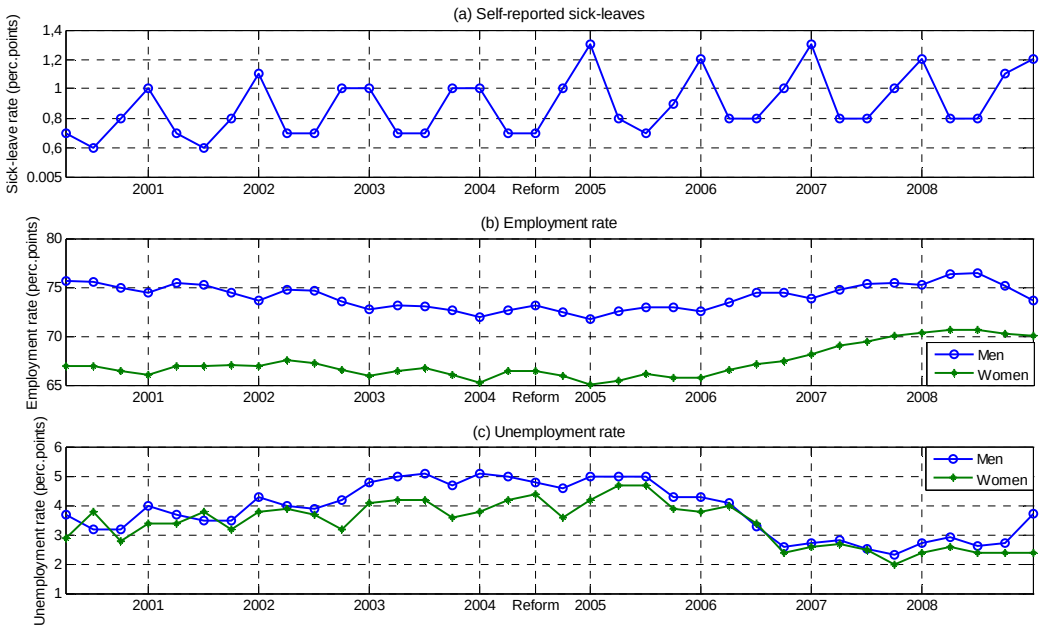

Figure 2: Non-certified (self-reported) sick leave and business cycle conditions
Panel (a) displays self-reported sick leave as a fraction of contracted hours. Panel (b) displays the quarterly employment rate defined as the fraction of population in working age that is employed. Panel (c) displays the quarterly unemployment rate defined as the fraction of population in working age seeking a job. Source: Statistics Norway.

Imagine that an exogenous factor other than the reform was affecting absence behavior at exactly the same time. If this were a health shock of some kind or a change in economic incentives, we would expect it to reduce both certified and non-certified – i.e. short-term – sick leave, maybe even by roughly the same amount. This is not the case, however. Panel (a) in Figure 2 shows quarterly non-certified pre- and post-reform sick-leave rates. As is clearly shown, non-certified sick leave was not reduced at the time of the reform. If anything, there seems to be a slight increase throughout the whole period. Part of this can be related to the reform's part *E* which made self-reported sick leave prior to certified sick leave count from workers' quota of self-reported absence. Nevertheless, we can rule out all factors that should affect

non-certified and certified sick leave simultaneously as candidate explanations for sick leave fell as much at the time of the reform.[6]

To my best judgment, there are no obvious candidate explanations – in the labor market or elsewhere – that can account for the drop in sick leave displayed in Figure 1.

3. Data and descriptive statistics

This paper is based on Norwegian administrative register data. The data sample used covers all Norwegian employees aged 20 to 65. To enable pre-reform and post-reform comparisons, the data set starts in January 2002 and ends in December 2005. I have data from the Social Security Administration in Norway (NAV) covering all sick-leave spells in this period that were certified by a physician. All absence spells lasting more than three or eight days – depending on the type of firm – must be certified by a physician. Upon certification, this is registered and the absence spell occurs in our data set. Using these data I am also able to match each worker with his primary care physician. The data are described in Tables 1 (employees), 2 (sick-leave spells) and 3 (physicians).

Table 1 displays the number of workers in our sample and some of their characteristics. Obviously, the year-to-year changes in a sample like this are small. The population of study is around 1.8 million workers each year. 77 percent of these are in full-time employment.

[6] Note that the modest increase in self-reported sick leave (from 0.88 to 0.9 percentage points) are of a completely different magnitude than the drop in certified sick leave (1.34 percentage points) meaning that the effects of the reform is not a history of substitution from certified to self-reported sick leave.

Table 1: Descriptive statistics, employees

	2002	2003	2004	2005
No. of workers	1 806 031	1 799 484	1 803 160	1 808 960
Mean age	41.3	41.6	41.9	42.2
Share of females	0.47	0.47	0.47	0.47
Education				
…< 10 years	7.5	7.2	6.9	7.5
…10-13 years	59.6	59.2	58.6	57.5
…14+ years	32.9	33.6	34.4	35.0
Mean earnings	313 521	325 498	336 616	350 987
Full-time employment	0.77	0.77	0.77	0.77

Notes: Table 1 provides some descriptive statistics regarding the dataset used for estimation below. The dataset cover all Norwegian employees aged between 20 and 65 years.

Table 2 displays some descriptive statistics for sick-leave spells of employees in this data sample. All sick-leave spells are grouped into one of three categories: full-time (FT), part-time (PT) or active sick leave (AS). This is appropriate since the reform aims to reduce AS, promote PT, and reduce the duration of FT spells. Note that distinguishing between FT and PT is not always easy. Some spells start out as FT and end as PT, and vice versa. In this paper, spells are grouped the following way: If spell intensity is below 100 percent at the outset, it is categorized as part-time (PT). The remaining spells are denoted as full-time spells (FT).

Full-time sick leave is the predominant category, accounting for around 80 percent of total workdays lost due to sickness. Counting the number of spells, FT dominates even more. More than 9 out of 10 new sick-leave spells are FT spells. On average these spells last a bit longer than 30 days. Part-time and active sick-leave spells are substantially longer, lasting around 80 and 200 days respectively. Even if possible reform effects are somewhat hidden in this table since the reform entered into force midway through 2004, some changes really seem to have occurred. The number of full-time sick-leave spells fell by around 13 percent in 2004 compared to the

preceding year. At the same time, the number of part-time spells rose whereas the number of active sick-leave spells fell. These two latter changes seem to roughly cancel each other out. However, the net effect on labor supply of this shift is probably a reduction in sick leave, as part-time spells tend to last much shorter than active sick-leave spells.[7] Mean spell duration for full-time spells fell from 32.7 days in 2003 to 31.7 days in 2005, a reduction of 3 percent. In the analysis below, these suggested reform effects will be estimated and quantified.

Table 2: Descriptive statistics, sick-leave spells

	2002	2003	2004	2005
Mean sick-leave rate				
…Full-time	0.051	0.052	0.047	0.046
…Part-time	0.003	0.003	0.003	0.004
…Active sick leave	0.010	0.011	0.008	0.005
No. new spells, based on starting date				
…Full-time	1 155 323	1 117 673	972 995	1 034 981
…Part-time	50 003	49 757	61 747	71 144
…Active sick leave	34 012	36 192	22 555	17 246
Mean spell length, based on starting date				
…Full-time	30.7	32.7	32.1	31.7
…Part-time	80.9	83.4	78.0	80.5
…Active sick leave	208.7	195.6	187.8	203.0

Notes: Table 2 provides descriptive statistics concerning the sick leave of the workers in the estimation sample presented in Table 1.

Table 3 displays some descriptive statistics for the physicians involved, i.e. the physicians with whom the worker in our data sample are registered as patients. The number of primary care physicians remains fairly consistent during this period at around 3850. *List vacancy* is the fraction of a physician's capacity that is vacant and is calculated from these physicians' number of patients and their reported maximum number of patients they are willing to serve. The panel doctor system was established

[7] The exact labor supply effect of such a shift remains unclear, as the actual number of working hours for workers on active sick leave is unknown.

in 2001 and the statistics for list vacancy indicate that as time goes by, more and more doctors are getting closer to their desired number of patients. The system is such that physicians' earnings are directly related to their number of patients. The physician receives a fixed amount per patient per year (capitation fee, 326 NOK ≈ 50 USD in 2006). In addition, physicians obtain fees for services, partly from the public and partly from the patient. The *patient turnover* rate is calculated as the mean of patients gained or lost each month, implying that around 1 percent of patients are new each month. On average, there are almost 470 employees registered with each physician. There is substantial variation in sick leave among physicians.

Table 3: Descriptive statistics, physicians

	2002	2003	2004	2005
# of physicians	3844	3848	3841	3867
Mean age	46.3	46.8	47.2	47.5
Female share				
Fraction of list vacancies (x100)	8.7	8.07	7.58	7.65
…25th percentile	16.5	14.27	11.29	10.87
…50th percentile	3.29	1.67	1.27	1.3
…75th percentile	-0.07	0.19	0.22	0.25
Monthly patient turnover rate	0.0097	0.0091	0.0085	0.0095
# of employees in our sample (mean)	469.8	467.6	469.5	466.7
…25th percentile	329	330.0	335	332
…50th percentile	455	456	457	455
…75th percentile	587	583	586	585
Sick leave (mean)	0.063	0.065	0.057	0.053
…25th percentile	0.0516	0.0536	0.0467	0.0435
…50th percentile	0.0617	0.0638	0.0558	0.0519
…75th percentile	0.0731	0.0754	0.0657	0.0607

Notes: Table 3 provides descriptive statistics concerning the physicians of the workers in the estimation sample presented in Table 1.

4. Empirical analysis: the effects on sick leave

Outcomes of interest and choice of models

The most common measure of sick leave is the *sick-leave rate* of a person or a population within a time interval, measured as the share of working time spent on sick leave. The sick-leave rate is a stock variable, with flows in and out. Any given period contains spells starting at different dates – spells of various age groups so to speak. If the reform has an impact on either the inflow to or the outflow from sick leave, the effect will be dynamic. A permanent five percent reduction of the inflow to sick leave will immediately reduce the absence rate. However, the full effect will not become apparent until twelve months later. For the same reasons, a temporary shock in the inflow to sick leave will also have lasting effects, albeit not permanent ones. Below, the reform's potential impact will be evaluated with regard to all of these three outcomes: the monthly sick-leave rate, the incidence rate of sick leave and the hazard rate or the probability of ending a sick-leave spells once it has begun.

A linear regression model is applied for both the monthly sick-leave rate and the incidence rate. In order to reduce potential selection problems, all these models are estimated with *individual fixed effects* (within estimator). The equation fitted on the data is then, generally as (1.1). The dependent variable *abs* is the sick-leave rate of worker *i* in month *t* or a dichotomous variable equal to 1 if a spell began in period *t* for worker *i*, and zero otherwise. α is the individual intercept, *F(time)* is a set of time dummies to be described below, and *ref* is the variable of interest, taking zero prior to the reform and 1 after implementation.

$$abs_{i,t} = \alpha_i + F\left(time\right) + ref + u_{i,t} \qquad (1.1)$$

When estimating effects on the incidence rate, workers already on sick leave at the beginning of the month are excluded. The population *under risk* is simply the workers *not* on sick leave on the last day of the preceding month. With a less strict approach the population at risk would have been slightly larger, as some workers may return to work early in a given month and be at risk of starting a new spell later that month. I do, however, believe the errors caused by this to be negligible and non-systematic in any sense that could affect our estimates.

In order to investigate whether the reform had an effect on the probability of ending a sick-leave spell, I follow Johansson and Palme (2005) and estimate a Cox regression model for each of the three sick-leave categories. The model specification is given by (1.2)

$$\lambda(t) = \lambda_0(t)\exp\begin{Bmatrix} I^R(1,8)\beta_1 + I^R(9,23)\beta_2 + I^R(24,52)\beta_3 \\ +F(time) \end{Bmatrix} \qquad (1.2)$$

$I^R(r_i, r_j)$ is an indicator function taking the value 1 if calendar time is after the reform was launched (July 1st 2004) and the spell has lasted between r_i and r_j weeks, otherwise $I^R(r_i, r_j)$ equals zero. Again *F(time)* is a set of time dummies to be described below. The appealing feature of this model is that, apart from the proportional hazard assumption made by most duration models, no assumptions regarding the hazard function are needed as the *partial likelihood function* used for estimation is independent of the baseline hazard *λ(t)*.

Identification of the reform effects

The reform affected all workers simultaneously and will be represented by a dummy variable taking 0 before the reform was implemented and 1 after.

Unfortunately I am unable to separate its effect from other time-related effects occurring at the same time. I have two identification strategies: The first is to use separate year and month dummy variables. Hence, the year dummies capture any time trends, linear or non-linear, whereas the month dummies capture seasonal variations, which are known to be substantial. In such a model, the reform effect is identified as the difference in sick leave between the first 6 months in 2004 and the last 6 months in 2004 – relative to this difference for other years (2002, 2003 and 2005). Any long-term effects of the reform visible in 2005 will be captured by the year dummy for 2005 so that this formulation gives us the "short-term effect" of the reform. In order to capture the "long-run effect" of the reform, the model is estimated with a linear time trend (year) and the same seasonal dummies (months) as above. Hence, this estimator will also capture effects of the reform for 2005, but the effects of the reform may be contaminated by non-linear time patterns unrelated to the reform.

Results

The estimated effects of the reform on the monthly sick-leave rate are displayed in Table 4.

Compared to the other years covered by the data sample (2002, 2003 and 2005), sick leave per worker fell substantially at the time of reform. Using the short-run estimator, giving us the effects strictly *within* the year of the reform, i.e. 2004, I find that full-time sick leave fell by 17.4 percent, part-time sick leave increased by 14.8 percent and active sick leave fell by 24.7 percent. Using the long-run estimator,

and a linear time trend instead of year dummies, the effects are stronger for all sick-leave categories.[8]

Table 4: Effects on the monthly sick-leave rate

	Full-time		Part-time		Active sick leave	
	Short-run	Long-run	Short-run	Long-run	Short-run	Long-run
Reform effect (pp)	-.900	-.928	.045	.063	-.225	-.357
(standard errors)	(.008)	(.007)	(.002)	(.001)	(.004)	(.003)
% change	-17.4%	-17.8%	14.8%	20.8%	-24.7%	-39.2%
Year dummies	yes		yes		yes	
Month dummies	yes	yes	yes	yes	yes	yes
Time trend		yes		yes		yes
Individual fixed effects	yes	yes	yes	yes	yes	yes
# obs.	86 611 620		86 611 620		86 611 620	
# workers.	2 150 212		2 150 212		2 150 212	
R-squared	.0001	.0001	.0002	.0002	.0007	.0007

Notes: Table 4 displays the estimated effects of the reform on the monthly sick-leave rate in three categories: full-time, part-time and active sick leave. The short-run estimates compares the six months before the reform to the six months after the reform, adjusted for seasonal differences. The long-run estimates compares sick leave before and after the reform (2002-2005) using monthly dummy variables and a linear trend.

As mentioned above, we encounter some problems when studying effects on a stock variable as the reform really affects the incidence rate and the hazard, which are *flow variables.* The estimated effects of the reform on the monthly incidence rate are displayed in Table 5.

The monthly incidence rate of full-time sick leave fell by 7.7 percent immediately after the reform. Part of this effect was short-lived, however. The long-run estimator, also using 2005 data to identify the effect, reports a drop of 4.8 percent. The rise in part-time spells almost equaled the fall in active sick-leave spells, indicating a substitution from active sick leave to part-time sick leave in accordance with the reform makers' intentions. The incidence rate of active sick leave was more

[8] Keep in mind that the sick leave is a stock variable, thus any changes in the flows in and out of sick leave will manifest themselves, with a delay, in the sick leave rate.

than halved, and the long-run effects are even stronger. The quantitatively most significant result, however, is the drop in the incidence rate of FT spells. This result cannot be directly related to any of the reform elements and we are left to speculate. One possible explanation is that the added focus on the need to reduce sick leave made workers and/or physicians more averse to all types of sick leave. Another explanation could be that after the reform, workers were more often offered part-time sick leave rather than full-time, and faced with a choice between the two, some chose the latter.

Table 5: Effects on the monthly incidence rate

	Full-time		Part-time		Active sick leave	
	Short-run	Long-run	Short-run	Long-run	Short-run	Long-run
Reform effect (pp)	-.0037	-.0023	.0007	.0008	-.0007	-.0009
(standard errors)	(.0001)	(.0001)	(.0000)	(.0000)	(.0000)	(.0000)
% change	-7.7%	-4.8%	28.3%	32.4%	-57.0%	-73.2%
Year dummies	yes		yes		yes	
Month dummies	yes	yes	yes	yes	yes	yes
Time trend		yes		yes		yes
Individual fixed effects	yes	yes	yes	yes	yes	yes
R-squared	.0014	.0014	.0002	.0002	.0002	.0002
# obs.	81 021 501		81 021 501		81 021 501	
# workers.	2 149 391		2 149 391		2 149 391	

Notes: Table 5 displays the estimated effects of the reform on the monthly incidence rate to sick leave in three categories: full-time, part-time and active sick leave. The short-run estimates compares the six months before the reform to the six months after the reform, adjusted for seasonal differences. The long-run estimates compares sick leave before and after the reform (2002-2005) using monthly dummy variables and a linear trend.

We now turn to the effects on spell duration, measured as the probability (hazard) of ending a sick-leave spell. Table 6 displays the results.

The hazard from sick leave increased substantially after the reform. The probability of returning from sick leave immediately increased by 5.4 percent per week during the first 8 weeks of a FT spell, as intended by the reform. The return

probabilities also increased after 8 weeks of FT spells. It should, however, be kept in mind that the return probabilities from sick leave once a spell has lasted longer than 17 weeks are less than 5 percent per week. Hence, we are dealing with significant relative changes in small absolute probabilities.

Table 6: Effects on the hazard rate (hazard ratios)

	Full-time		Part-time		Active sick leave	
	Short-run	Long-run	Short-run	Long-run	Short-run	Long-run
Ref. effect: Weeks 1 – 8	1.054	1.065	1.133	1.145	1.247	1.180
(standard error)	(.0025)	(.0021)	(.0113)	(.0102)	(.0287)	(.0260)
Ref. effect: Weeks 9 – 23	1.041	1.048	1.029	1.032	1.043	.978
(standard error)	(.0037)	(.0035)	(.0098)	(.0091)	(.0158)	(.0138)
Ref. effects: Weeks 24-52	1.142	1.150	1.117	1.111	1.038	.938
(standard error)	(.0057)	(.0053)	(.0161)	(.0149)	(.0148)	(.0121)
Year dummies	yes		yes		yes	
Month dummies	yes	yes	yes	yes	yes	yes
Time trend		yes		yes		yes
# obs.	21 186 578		2 768 435		3 170 580	
# spells.	4 280 972		232 651		110 005	

Notes: Table 6 displays the estimated effects of the reform on the hazard rate from sick leave in three categories: full-time, part-time and active sick leave. The short-run estimates compares the six months before the reform to the six months after the reform, adjusted for seasonal differences. The long-run estimates compares sick leave before and after the reform (2002-2005) using monthly dummy variables and a linear trend.

To illustrate the quantitative importance of these effects it is useful to calculate how these coefficients affect spell duration. When doing so it is convenient to use the concept of *expected duration*. Denote the exit probability from sick leave during week i λ^i. Exit in period i requires that the spell has *survived* all periods up to i. This probability is denoted by the *survival function* $S(i) = \prod_{j=1}^{i-1}\left(1-\lambda(j)\right)$. Expected duration in days, conditional on week being the time unit, can then be expressed as (1.3).

$$E(D) = \sum_{i=1}^{52} h_i \frac{\lambda(i)}{S(i)} \tag{1.3}$$

S(0) equals 1 since an exit cannot occur before the spell has started. $\lambda(52) = 1$ since no spells are allowed to last longer than 1 year. h_i is a sequence of contributions from each week $h_i = 3.5, 10.5, 17.5, ..., 360.5$. The midpoint of the week is used, since the exit probability is assumed constant within a given week. This parametric approximation of mean spell duration is quite good. To calculate the baseline hazard I use observed durations for all spells starting between July 2002 and July 2003. Hence, none of these spells were affected by the reform. The results are displayed in Table 7.

Mean spell duration for full-time spells fell by 11.4 percent immediately after the reform. The long-run effect is even stronger, with a reduction of 13.3 percent. The partial changes are calculated by finding expected duration, changing only the hazard rates corresponding to one of the estimated parameters while keeping the other hazards constant. It becomes clear that the reduction in spell duration is mainly due to the increased hazards during the first eight weeks of a spell.

Table 7: Reform effects on duration, measured in days

Pre-reform	Full-time		Part-time		Active sick leave	
Observed duration	33.2		86.1		202.8	
Expected duration	32.7		85.3		201.3	
Reform effects	Short-run	Long-run	Short-run	Long-run	Short-run	Long-run
Total change (in days)	-3.7	-4.4	-8.2	-8.7	-7.7	-0.5
Total change (in percent)	-11.4	-13.3	-9.6	-10.2	-3.8	-0.2
Partial changes (in days)						
…Weeks 1 – 8	-3.0	-3.6	-6.4	-6.9	-4.6	-3.4
…Weeks 9 – 23	-0.4	-0.5	-1.0	-1.1	-2.1	1.1
… Weeks 24 – 52	-0.4	-0.4	-1.0	-1.0	-1.1	1.8

Notes: Table 7 displays the effects on the hazard rates from Table 6 measured in days using (1.3) to calculate changes in expected durations. Duration specific partial changes are found by holding two out of three duration specific coefficients (from Table 6) constant while the remaining coefficient is changed when expected duration are calculated.

Robustness

Since the reform affected all workers simultaneously the estimates are vulnerable to aggregate shocks affecting sick leave and occurring around the same time as the reform took place. Insofar as aggregate shocks occurred at the *exact* same time, there is no solution. However, if these shocks do not occur at the time of the reform, a test procedure based on repeated estimations with various *placebo reform dummies* can be used to identify the time at which the estimated effect occurred.

Note that this problem is most relevant when using the long-run estimator. The short-run estimator essentially compares the first six months of 2004 (pre reform) to the last six months of the same year (post reform). Shocks occurring before or after 2004 will not be registered by this estimator. Yet, shocks occurring near reform implementation will also here be misinterpreted as *reform effects*.

To investigate whether the estimated effects occurred at "the right time", i.e. at the time of the reform, I estimate the long-run version of the models (linear time trend and calendar month dummies) for the incidence rate and the hazard rate. The model is exactly the same as above, save for the reform dummy which is replaced by $I\left(t \geq p\right)$, an indicator function taking 1 if calendar time t is equal to or has passed date p and zero otherwise. Hence, for p = *July 2004*, this is the reform dummy and the model is equivalent to the *long-run model* estimated above. The model is estimated several times for different values of p, and the estimated coefficients are plotted in a figure which visualizes whether the estimated change actually occurred at the time of the reform or at some other time around the reform. The results are displayed in Figure 3.

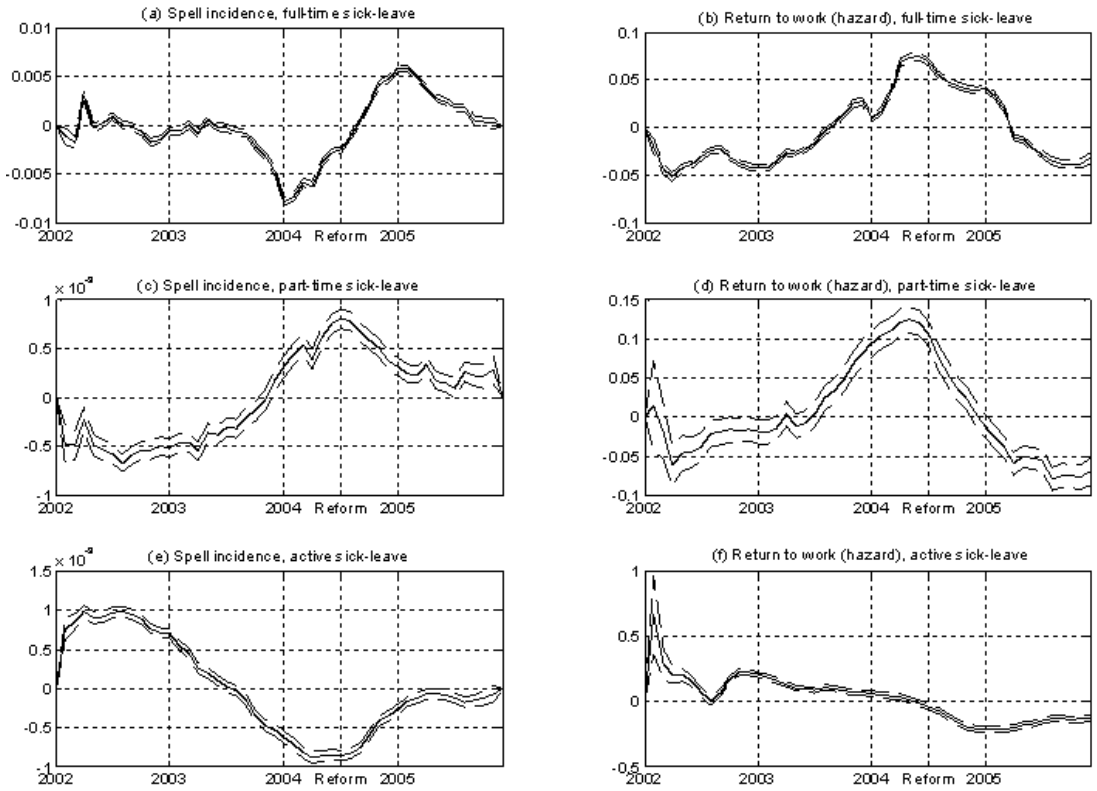

Figure 3: Timing of changes in spell incidence and return probabilities
The figure draws the estimated placebo reform coefficients for each month (see text). These effects should "peak" at the time of reform if the changes in sick leave took place when the reform was launched.

Panel (a) displays the placebo-coefficients for the incidence rate of full-time sick leave. Interestingly, the major change in spell incidence appears to occur around January 2004, some 6 months before the reform was implemented – and while the new regime was "in the pipeline". Note that the short-run estimator, controlling for each calendar year, will register this since the peak is at the start of the year. Still this estimator finds a substantial effect on the incidence rate of full-time sick leave. One way of interpreting these results is that the reform had two effects on the incidence rate of full-time sick leave. First, the growing public debate and attention had a separate effect – by either making physicians stricter or workers somehow less absence prone. Then, when the reform was launched, this also reduced inflow to full-time sick leave. We should, however, exercise caution before claiming that the reform

had long-run effects on the incidence rate of full-time sick –leave, as any "attention effect" is likely to be short-lived. Panel (b) paints a similar picture for the return probabilities, using the Cox regression model from above (1.3), but replacing the *duration specific* variables $I^R(1,8), I^R(9,23), I^R(24,52)$ with $I(t \geq p)$, which is independent of duration. The figure shows that the return probability from full-time sick leave occurs just before reform implementation, supporting the notion that the results from Table 6 are caused by the reform rather than by something else.

Panels (c) and (d) display the results from the same exercise for part-time sick leave and support the results from above, indicating that the reform had a profound effect on part-time sick leave. The last two panels (e) and (f) display the results for active sick leave. The reduction in the incidence rate to active sick leave seems to peak at the time of the reform implementation while Panel (f) clearly displays that any changes in the hazard rate from active sick leave are unrelated to the reform.

Summary of the results

In order to illustrate the quantitative importance of each of the estimated parameters, Table 8 provides an aggregation and decomposition of the results presented above.

In total, the reform reduced sick leave by 22.9 percent over the subsequent six months. The most important factor was reduced spell duration for full-time spells, which led to a reduction in total sick leave of 8.8 percent. This can be explained by the extended documentation requirements imposed on physicians for full-time spells. The second most important factor, accounting for an 8.7 percent reduction in sick leave, is the dramatic reduction in the number of active sick-leave spells. Third, the

incidence rate of full-time spells also dropped at the time of the reform, accounting for a 5.8 percent reduction in sick leave.

Table 8: Total reform effects, decomposed

Short-run	FT	PT	AS	Total
Total change in the sick-leave rate (pp)	-1.01	0.05	-0.63	-1.59
Percentage change	-18.2%	16.0%	-58. 6%	-22.9%
Partial contributions				
Incidence rate	-5.8%	1.2%	-8.7%	-13.3%
Duration	-8.8%	-0.5%	-0.4%	-9.7%
Total	-14.6%	0.7%	-9.1%	-22.9%
Long-run	FT	PT	AS	Total
Total change in sick-leave rate (pp)	-0.97	0.06	-0.79	-1.70
Percentage change	-17.5%	18.9%	-73.3%	-24.5%
Partial contributions (share of total reduction)				
Incidence rate	-3.6%	1.4%	-11.3%	-13.5%
Duration	-10.4%	-0.5%	0.0%	-10.9%
Total	-14.0%	0.8%	-11.3%	-24.5%

Notes: The estimated effects on the incidence rate (Table 5) and on expected duration (Tables 6 and 7) are used to calculate the total effect of the reform. The total change in sick leave is decomposed in partial contributions defined by type of effect (incidence rate or duration) and type of sick leave (full-time, part-time or active sick leave).

When the comparative period after the reform is extended to 2005, the long-run estimates suggest an even stronger reform effect, reducing sick leave by 24.5 percent. The long-run estimates suggest that the drop in the incidence rate of FT spells was (partly) temporary, and this was confirmed by the robustness checks referred to in the previous section. On the other hand, the reduction in spell duration persists and turns out to be even more important in the long-run, accounting for a total reduction in sick leave of 10.4 percent. The reduced number of active sick-leave spells accounts for a total sick-leave reduction of 11.3 percent.

5. Physician-specific reform responses

Was the drop in sick leave uniformly distributed across physicians or did physicians respond differently? If they responded differently, can these differences be explained and interpreted in order to learn more about policies directed at physicians? To answer these questions physician-specific reform responses are needed. Such a physician-specific reform response is denoted by ref^d . Note that the (homogeneous) reform response in (1.1) is related to the physician-specific reform effect in the following manner, where D is the number of physicians, subscript d denotes individual physicians, N^d is the number of workers who are patients of physician d, and N is the total number of workers used in estimation:

$$ref = \frac{1}{N}\sum_{d=1}^{D} N^d ref^d$$

The estimated common reform effect is a weighted average of the physician-specific reform responses where each physician's number of patients (in our sample) constitutes the weights. In the homogeneous reform-effect model (1.1) any heterogeneity in physicians' response to the reform ends up in the error term $u_{i,t}$. The idea of the procedure described below is to use these error terms to construct the physician-specific reform responses using the estimator in (1.4) where $t=s$ is the time of reform.

$$\hat{ref}^d = ref + \left[\frac{1}{N_d^{t<s}} \sum_{t<s, i\in d} u_{i,t,d} - \frac{1}{N_d^{t\geq s}} \sum_{t\geq s, i\in d} u_{i,t,d} \right] \qquad (1.4)$$

To make the periods before and after the reform as similar and comparable as possible, I will use the error terms from July 1st 2003 until June 30th 2005 only when constructing the physician-specific reform effect. Estimating a physician-specific reform effect only makes sense if the number of patient/month observations per

physician is sufficiently high. Physicians with less than 100 workers in my sample or less than 2400 worker/month observations will thus be excluded. This reduces the number of physicians by almost 10 percent. Since the effects on part-time sick leave and active sick leave are somewhat mechanical and seem to, at least to some extent, nullify each other, I will here focus solely on the effects on full-time sick leave. These are also the quantitatively most important ones. Hence, the starting point of this exercise is the estimation of (1.1), using the monthly sick-leave rate per worker as the dependent variable and the long-run estimator, where the reform is estimated to reduce the monthly sick-leave rate by 0.923 percentage points (see Table 4).

When correlating the physician-specific reform response with physician characteristics, the data set contains one observation per physician. Table 9 displays descriptive statistics for the physicians in each quartile of the distribution of physician-specific reform effects.

Table 9: Physician-specific reform responses

Total # of physicians: 3351	Q1:	Q2	Q3	Q4:
Mean reform change (pp)	-1.99	-1.16	-0.68	0.03
Mean age	47.1	48.0	47.8	47.1
Female share	31.3	27.0	28.4	31.5
Specialist education share	46.4	49.9	51.3	59.6
Co-practice (medical center) share	9.7	7.5	7.8	8.4
Number of patients				
…June 2004	1220.5	1308.8	1311.4	1204.5
…June 2005	1222.1	1316.0	1310.0	1212.9
Number of working patients (in sample)				
…June 2004	463.8	520.3	527.8	475.2
…June 2005	462.1	520.8	525.4	476.6
Fraction of list vacancies (x100)				
…June 2004	4.87	4.88	5.59	7.79
…June 2005	4.39	4.83	5.34	6.82

Notes: Table 9 displays descriptive statistics for each quartile of physicians, sorted after how their patients' sick leave was changed by the reform. Q1 are the quartile of physicians whose patients' sick leave fell the most, Q4 are the quartile whose sick leave fell the least.

There is substantial variation between physicians in how they responded to the reform. This should however be interpreted with caution as the measure is sensitive to pure randomness in individuals' sick leave. It is thus only meaningful to draw any conclusions if we are able to explain this variation. No systematic relationship seems to exist between age or gender and reform response. Physicians without a specialist education and working in a co-practice (medical center) are overrepresented among the most reform responsive physicians. Taking into account the cautionary note on sample noise we note that physicians with few patients are overrepresented on the tails of the distribution. Interestingly, list vacancies prior to the reform and reform responsiveness appear to be correlated. Physicians with fewer patients than desired are overrepresented among the least reform responsive physicians.

In order to investigate the suggested relationships a model can be estimated where the physician-specific reform response is explained by physician characteristics such as age, gender, pre-reform number of patients and pre-reform list vacancies. The results are displayed in Table 10.

Physicians' age or gender or co-practice fails to explain any of the variations in physicians' reform response. Physicians with a specialist education were less responsive to the reform than others, reducing sick leave by about 0.06 percentage points less than others. The same goes for physicians working alone. Physicians working in a medical center (co-practice) reduced sick leave by approximately 0.09 percentage points more than others.

Table 10: Which physicians were most/least affected?

Dep.var: ref^d (measured in percentage points)	(1)	(2)	(3)	(4)	(5)
Age	-.002	-.001	-.001	-.001	-.001
Female	.005	.025	.030	.042	.047
Co-practice (medical center)	-.078	-.100**	-.097*	-.084*	-.087*
Specialist education	.054*	.068**	.066**	.062**	.062**
List vacancies June 2004		.492***	.518***	.568***	
< -7.0% (p1-p5)					-.042
-7.0% to -3.2% (p5-p10)					-.090
-3.2% to 0.1% (p10-p25)					-.016
0.1% to 1.0% (p25-p50)					reference
1.0% to 7.9% (p50-p75)					.083**
7.9% to 23.7% (p75-p90)					.084*
23.7% to 33.9% (p90-p95)					.170**
> 33.9% (p95-p100)					.281***
No. patients June 2004 /100			.003		
No.empl.patients June 2004/100				.023***	.026***
Constant	-.886***	-.460***	-.472***	-.506***	-1.018***
R-squared	.0018	.0075	.0077	.0096	.0109
No. observations	3351	3351	3351	3351	3351

Notes: Table 10 displays the estimated coefficients for physician characteristics used to explain the variation in reform response between physicians.

Interestingly, the fraction of list vacancies prior to the reform seems to be important. Physicians who had their desired number of patients prior to the reform (June 2004) reduced sick leave among their patients significantly more than those suffering from lots of vacancies. In column (5) we can see that patients of the 5 percent of physicians with the most vacancies (p95-p100) reduced their sick leave by almost 0.323 percentage points less than those whose physicians had fewest vacancies (p1-p5). Clearly, physicians with fewer patients than desired did not contribute as much as the other physicians to the overall reduction of sick leave. Physicians with many patients were less responsive to the reform than others. Since the number of patients and list vacancies are positively correlated, these two effects counteract each

other, but only to a certain extent, as the effect of list vacancies is quantitatively much more important.

These results suggest that the market system for primary care physicians' patient allocation has certain undesirable side effects. Physicians' business interests can come into conflict with policy instructions given by the government and make policy implementation difficult. Any positive effects from this market system, such as improved services or reduced waiting times, should be weighed against the limits this market system puts on the gatekeeping role of physicians. More research is needed, and policy recommendations based solely on the results in this paper should obviously be taken with caution. However, taking the results at face value, a cap on the maximum allowed number of patients seems to have positive effects on gatekeeping – both for physicians with (too) many patients and for those suffering from patient shortage.

6. Discussion: why did it work?

How such a modest reform, not altering economic incentives of any kind, could have such a great impact on sick –leave is quite puzzling. One possible explanation is that physicians got stricter in fear of a government crackdown. Several public statements from the Ministry of Social Affairs contained threats, stating that physicians could lose their right to issue sick leave if they did not "improve". In retrospect this was probably mostly "tough talk", a criticism also voiced in the public press at the time. I find it hard to believe that physicians really feared losing their right to issue sick-leave certificates. The question remains how the reform had such a great impact if the physicians did not want to be "reformed". In my view, one credible explanation is that the reform provided physicians with a welcome "excuse". I believe

many physicians were (and indeed are) frustrated by patients demanding sickness-certificates, sometimes even "shopping" for lenient physicians. Hence, these physicians could now refer to the reform and tell their patients that "I would like to, but my hands are tied", when asked for dubious sickness certificates or to prolong sick leave for recovered patients. Hence, the pressure laid on physicians may actually have increased their bargaining power towards their patients. This is a variant of "two-level games", used for understanding the role of domestic opinion in international negotiations (Putnam, 1998). By blaming the reform (which plays the role of domestic opinion), physicians could strengthen their gatekeeping role, without being the ones to blame.

The idea of physicians "wanting to be reformed" fits nicely with the second result in this paper; that physicians suffering from a shortage of patients reduced sick leave among their patients by much less than others. These physicians' bargaining power was still weak since they could not afford to lose patients.

7. Conclusion

A Norwegian reform of the regulations governing sick-leave certification by primary care physicians turned out to have a major impact, reducing sick leave by more than 20 percent. Some of the measures introduced are linked to a particular Norwegian context, while others are of a more general kind. The most important component of this successful reform was the requirement to report to the Social Security Administration stating why – on medical grounds – inactivity was necessary, if the worker had performed no work-related activity after eight weeks of sick leave. This led to significantly higher return probabilities from full-time sick leave and reduced the total amount of sick leave by 10.4 percent. One hypothesis why this had

such an impact is that physicians could point to the reform and the new requirements as justification. Physicians, tired of patients demanding sick leave – reducing physicians to "providers of social benefits" – could blame the reform and the extended documentation routines and encourage a return to work.

A second finding in this paper is that there was substantial heterogeneity between physicians in how they responded to the reform, i.e. in how much they reduced sick leave among their patients. Interestingly, physicians with substantially fewer patients than desired had a far weaker response to the reform than physicians with their desired number of patients. This suggests that in a market based system for allocation of physicians and patients, a physician's gatekeeping role may sometimes come into conflict with his business interests. This conflict of interest should be taken into serious consideration and weighed against any benefits from such a market based system.

In general, this paper suggests that physicians play an important role as gatekeepers, and that their gatekeeping can be affected by public policy. Stronger monitoring makes it possible to maintain a more generous sickness compensation scheme without increased moral hazard. Reforms strengthening the bargaining power of physicians, by forcing them to undertake medical evaluations and provide part-time sick leave when appropriate, appear to be a useful way of reducing sick leave in generous welfare systems.

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