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The effect of sick-leaves on earnings

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Abstract:

This paper assesses the causal effect of sick-leaves on subsequent earnings using an administrative dataset for Norway linking individual earnings, sick-leave records and primary care physicians. The leniency of a worker's physician - certifying sickness absence - is used as instrument for sick-leaves. Sick-leaves have a substantial impact on future earnings, reducing earnings by .3 percent per day of absence. When conditioning on full-time employment also two years after sickness the effect is .06 percent per day of absence. These effects are persistent over time and work mainly through wages not hours.

Keywords: sickness absence, wage formation, IV estimation, wage regression

JEL classification: C31, J22, J24, J31, J33, J71

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1. Introduction

Numerous studies have shown strong correlations between economic status and health.[2] Several theories are suggested to explain this and causality is suggested to go both ways, from health to economic outcomes, and from economic status to health. This relationship is also of great interest to policy makers fighting what has become known as *the social gradient in health*. This paper investigates a very specific component of this relationship: the relationship between sick-leaves and subsequent earnings. With help of detailed register data, linking workers to their primary care physicians responsible for certifying sickness absence, the effect of sick-leaves on earnings are estimated. A causal relationship from sick-leaves to earnings can explain parts of the correlation between economic status and health. Such a relationship is also useful to learn more about wage setting and employment relations in general and can shed light on topics such as earnings inequality, variations in sickness absence across different groups of workers and the gender wage gap.[3]

The relationship between sick-leaves and wages are investigated theoretically by - among others - Weiss (1985) and Coles and Treble (1993, 1996). In their models, firms in which absences are costly are willing to pay more - either so that their workers are less absent or to attract better workers. Examples are firms with assembly line production and firms where team production is important. Firms' costs of sick-leaves are studied by Nicholson et al. (2006) who use a survey of 800 managers in 12 industries. They find that

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[2] See e.g. Handbook of Labor Economics, ch.50, 1999, edited by Currie and Madrian.

[3]Several studies are linking the gender differential in sickness absence to the gender wage gap, see Ichino and Moretti (2009), Hansen (2000) and Pfeifer and Sohr (2008).

the cost "varies across jobs according to the ease with which a manager can find perfect replacement for the absent worker, the extent to which the worker functions as part of a team, and the time sensitivity or the worker's output" (Nicholson et al. 2006, p.111). The study estimates the cost of the median firm to be 28 percent of wages.

In addition to any direct of loss of income, sickness absences may have several indirect costs. One is that sick-leaves may increase the probability of being fired or laid off. This is studied by Hesselius (2007) who shows that workers with high sickness absence rates are more likely to become unemployed at a later date. The Hesselius study is however not able to separate the possible causal effects of sick-leaves from possible unobserved characteristics correlated with both productivity and sick-leaves. Henningsen and Hægeland (2008) study mobility in downsizing firms and find that workers with a sick-leave history are more likely to leave.

Sick-leaves may also affect (future) wages. It is well known that there is a negative correlation between wages and sick-leaves (see e.g. Barmby et al. 1991, Markussen et al. 2009). There are however only two former studies that try to assess the causal effect of sick-leaves on wages. The study most comparable to this paper is Hansen (2000) that exploits a policy change in Sweden as instrument for sick-leaves and finds substantial costs of sick-leaves for women but not for men. Hansen finds that, for women, one additional day on sick-leave reduces the wage rate by 0.2 percent. He finds however no effects on wages from being home with a sick child, and interprets this as support for a signaling argument, rather than connected to human capital accumulation (Hansen 2000, p.51).

In a recently published paper Ichino and Moretti (2009) find that absences of women below the age of 45 tend to follow a 28 day cycle not present for older women or for men. They interpret this as absenteeism caused by the menstrual cycle, reflecting biological differences and not different propensities for taking occasional days off. From a model of statistical discrimination they hypothesize that the relationship between absences and earnings should be weaker for females than for males - because biologically caused cyclical absences make sick-leaves a less informative signal of productivity and effort for females than for males - a proposition they find support for in their data. They estimate that one additional day of cyclical absences cost male workers about 2.5 percent of earnings whereas the cost for female workers is 1.5 percent. Finally, they find that biological differences in cyclical absences can account for at least 14 percent of the gender wage gap. Differences in cyclical absences between workers should be interpreted as permanent – or at least long-lasting – worker heterogeneity. When workers with one additional day of cyclical absences earn 2.5 percent less, this is not the causal effect of one additional day on sick-leave – but a manifestation of how heterogeneity in health (or effort) affect earnings over time. Ichino and Moretti (2009) are convincingly illustrating the importance of biological differences in health and their importance in the labor market. However, they are not directly answering the question of this paper: what is the cost of one additional day on sick-leave? Evidently, the existing literature is both conflicting – Hansen (2000) finds only wage effects for females not males, Ichino and Moretti (2009) finds wage effects to be largest for males – and limited. This paper adds to this literature by: (1) clearer identification of causal effects using primary care physicians' strictness as instrument variable, (2) investigating the persistence of such causal effects of sick-leaves on earnings and (3) estimating effects for several subgroups such as men, women, old, young, public sector, private sector, high and low education to better understand the mechanisms driving the results.

There are several reasons to expect sick-leaves to affect earnings and we can expect to find effects on employment, wages and hours worked. There is strong duration dependence in sickness absence such that when on sick-leave the probability of not returning to work is sharply increasing over time (Markussen et al. 2009). The effect of sick-leaves on future employment is not the primary target of this paper. It will nevertheless be an issue when interpreting the estimation results. Through hours worked sick-leaves may affect labor supply also for workers that remain employed. I will thus separate effects on hours worked from effects on wages. The effect on earnings, through changes in wages is the primary focus of this paper.

Sick-leaves are costly for employers. It could be that employers "punish" the sick in order to provide incentives to work, e.g. they may tie bonus arrangements or wage punishments to their employees' attendance. We can also imagine more subtle processes. Consider a firm thinking about promoting a worker. The firm has limited information regarding the worker's health status. It seems plausible that a worker's sick-leave history matters as it serves as a predictor for future health - and possibly also of effort. Hence, if sick-leaves matter for promotions and promotions imply a wage rise, sick-leaves will affect future wages negatively. A third explanation is that a relationship between sick-leaves and wages works through the process of specific human capital accumulation or depreciation. While one worker is away on sick-leave, other workers may carry out important projects, making workers otherwise of equal value to the firm, different. In practice, however, it seems unlikely that being away from work for a week or two should affect skills in such a way that subsequent earnings are affected.

This paper assesses the causal effect of sick-leaves on earnings. As wages adjust slowly the effect of sick-leaves in year *t* are estimated on earnings two years later. Sick-leaves and wages may however be correlated through several unobservable variables such as general health and motivation. This problem is handled by using the leniency of a worker's primary care physician as instrument variable for sick-leaves. In the Norwegian panel doctor system each worker is listed with a specific primary care physician. When ill, if not acute or on weekends, this physician is the place to visit when a sickness certificate is needed.

Markussen et al. (2009) estimate a practice style indicator - a "strictness/lenience measure" - for each primary care physician in Norway. This indicator is estimated jointly with a rich set of observables for each worker (earnings, education, age, family situation, county of residence etc.) as well as a workplace dummy. Hence, the physicians' practice style indicator (PPI) should be a suitable instrument for sick-leaves, capturing the variation in absence propensity that is due to differences in leniency between doctors.

I find sick-leaves to have a substantial impact on earnings two years later. One additional day on sick-leave leads to a reduction in mean earnings two years later by .3 percent. This is partly because wage growth is reduced and partly because sick-leaves reduce future employment. When I restrict the sample to workers employed full-time the year before, during and two years after sick-leave, the estimated effects are much smaller. Still, even among these "core workers", one additional day on sick-leave reduces earnings two years later by .06 percent. The effect is stronger for males, young workers, private sector employees and high earners. I also show that the effect - conditional on continued employment - works through both wages and hours worked, but that the wage channel dominates. The results also indicate that the first day on sick-leave is more costly for workers than additional sick-leave days when the worker already has been absent. If we consider sick-leaves a measure of productivity or effort, the highest possible effort is when absences are zero. Hence, sick-leaves are a censored signal and starting an absence spell makes the worker no longer associated with the high effort – zero absence – group.

This rest of this paper is organized as follows. Section 2 presents the necessary details about the Norwegian sickness insurance system, the data in use, the empirical strategy and the identifying assumptions. The main results are presented in section 3 and section 4 contains a series of robustness checks. Section 5 discusses the results before conclusions are drawn in section 6.

2. Data and estimation strategy

Earnings regression

In most jobs, wages are fairly rigid in the sense that they do not change from week to week. Normally wage changes occur through wage negotiations. In Norway there are two types of such negotiations, central and local. The central, or national, wage negotiation is irrelevant in this setting, as wages are set on a collective basis for large groups of workers. However, every year there are local wage negotiations where workers potentially are given pay rises on individual basis. These negotiations, together with promotions and job-changes are the relevant channels for sick-leave to affect wages.

In order to capture the wage effect of sick-leaves, we need a dynamic model. A sick-leave today, will not affect wages immediately. It will, in some jobs, affect earnings immediately, but this is just because the worker looses bonuses, overtime payment etc. That does create incentives to work, but is not the scope of this paper.

Wages may not adjust immediately. If there are wage effects of sick-leave, this effect should work through one of the three channels mentioned above; individual wage negotiations (annually), promotions (sluggish) and job-changes (also sluggish).
The approach here is thus to estimate the effect of sickness in year *t* on earnings in *t+2* , controlling for earnings in *t-1* as well as several individual and job characteristics. The estimation equation is given by (2.1). Individual subscript *i* is suppressed to simplify the notation.

$$\log y_{t+2} = \delta_1 \log y_{t-1} + X_t \delta_2 + \beta a_t + \varepsilon_{t+2} \qquad (2.1)$$

y_{t+2} is earnings in the second year after a sick-leave, X_t is a collection of several individual characteristics such as age, education, gender, sector of employment etc. a_t is sick-leave in period *t*, either measured as the share of work-days lost to sick-leave, or as an indicator for having any sick-leave spell in year *t*. Earnings the year before sickness is also included as a control variable to capture permanent productivity/earnings differences.

Data and population of study

This paper makes use of Norwegian administrative register data, provided by Statistics Norway and the social insurance administration (NAV), and comprises starting dates, stopping dates and diagnoses (ICPC-2) for all certified sick-leave spells in Norway from January 2001 through 2005. The dataset also includes the (encrypted) identity of the physician responsible for its certification. Diagnoses and certifying physicians are going to be the key to identification in this paper. The data on absence spells are merged with other administrative data registers containing information about individual employees, such as their age, sex, education, sector of work and county of residence.

The year of sickness will be denoted year $t = 2001,\ldots,2004$. This study covers all employees aged 25-59 years old, being employed full-time and earning at least 1.85G - approximately 16 000 USD – in t. Key variables will be earnings and employment status the year before sickness $(t-1)$, diagnosis and earnings and employment status two years after sickness $(t+2)$. As the logarithm of earnings in $t+2$ is used as dependent variable and the logarithm of earnings in $t-1$ as control, workers with zero or negative earnings in these years are excluded. Workers with implausible combinations of work-hours and earnings in the years $t-1$ and $t+2$ are also excluded.

Throughout the paper, results from three different data samples will be presented and discussed. The first is referred to as the *Full sample*, selected exactly as described in the preceding paragraph. To emphasize wage effects and distinguish these from earnings changes originating from workers leaving the labor force a subset of the Full sample is used, only consisting of workers full-time employed also in the years $t-1$and $t+2$. This is referred to as the *Restricted sample.* Unfortunately, the data for 2006 are incomplete, as the only information available for 2006 is earnings. The Restricted sample is consequently available only for $t = 2001,\ldots,2003$. For robustness, I also add information from another dataset containing wage and working hours for a sample of Norwegian workers, produced by Statistics Norway. As this sample is considerably smaller than the two others I use this for robustness checks only. This sample is referred to as the

Wagestat sample. Table 1 summarizes some key feature of the three data set used.

Table 1: Sample descriptives

	Full sample	Restricted sample	Wagestat sample
No. of worker/year obs.	4 464 364	2 766 901	1 783 015
Percent females	38.3	35.1	40.1
Age (mean)	42.6	42.7	44.2
Education			
< 10 years	6.2	5.8	5.0
> 13 years	36.8	36.8	45.0
Employment			
not employed in t-1	3.5	0.0	1.8
part-time in t-1	4.2	0.0	2.7
not employed in t+2	4.7	0.0	0.8
part-time in t+2	4.4	0.0	2.3
Sick-leave			
percent with sick-leave	40.0	38.5	38.8
sick-leave days	21.8	17.6	17.1
sick-leave days if > 0	54.5	45.7	44.2

Note: The table compares the full- to the Restricted sample containing full-time workers in t-1 and t+2 only, and the Wagestat sample in which wages and hours are observed.

The Full sample consists of nearly 4.5.million observations, considerably more than the Restricted sample of (more) permanent full-time workers and the Wagestat sample. When we compare the Full sample to the Restricted sample we see that among the workers excluded from the Restricted sample, i.e. those with a looser connection to the labor market, there are more females, less educated and more absent workers. The Wagestat sample is somewhat different, consisting of more females, older and much higher educated workers. Keep in mind that this is a selected sample, consisting of sampled firms, while the Full sample contains all workers full-time employed and earning more than 100.000 NOK in year *t*. The Restricted sample and the Wagestat sample are however very similar regarding sick-leaves.

The Norwegian sickness insurance system

All Norwegian employees are entitled to a 100 percent replacement ratio during sickness lasting shorter than 1 year for all earnings below 6G (roughly 70 000 USD). In practice many workers have the same insurance for earnings above 6G provided by the employer. The first 16 days of an absence spell the expenses are covered by the employer after which the social security system foots the bill. The general rule is that sick-leaves 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 inclusive workplace agreement which covers around half the labor force.

Workers unable to return to work after 1 year of absence are offered rehabilitation help and benefits to qualify for other types of jobs or - if return to work is not possible - disability benefits. Replacement rates for rehabilitation and disability are substantially lower than for sick-leaves, approximately 66 percent of former earnings.

Identification

Sickness absence is hardly a random event. We would thus expect it to be an endogenous regressor in equation (2.1) such that estimation of (2.1) directly using OLS will violate the assumptions needed for OLS be an unbiased estimator. One obvious cause for such a problem is that omitted variables, such as motivation or health, may affect both job-productivity and absence propensity. To obtain exogenous variation in endogenous sickness I make use of the Norwegian panel-doctor system. Since 2001 (nearly) all Norwegian citizens are registered with a primary care physician who serves as the gate to the health care system. Physicians are, in addition to provide care, advice and diagnosis, providing sick-leave certificates, drug prescriptions and referrals to specialist care. For a worker to obtain a sick-leave certificate he must visit his primary care physician.

Markussen et al. (2009) estimate how various covariates affect the probability of beginning and leaving a sick-leave spell for Norwegian workers in the years 2001-2005. This is done in the framework of a multivariate proportional hazard model and they make use of an extensive set of covariates. Most important, for this purpose, is that fixed effects for workplace, physicians and counties are estimated. Fixed physician-effects are estimated for all physicians with more than 400 patients, in total 3 522 individual physicians, and the estimates control, non-parametrically, for worker characteristics such as income, wealth, marital status, age, number of children, business cycle conditions, family "shocks" such as birth/pregnancy, decease of family members, separation/divorce and more. Using the limiting distribution of a Markov transition matrix (see e.g. Taylor and Karlin, 1998 p.1999) these physicians' dummy coefficients are mapped into one single number, the physicians’ lenience indicator (PPI) for each Norwegian physician, measuring each physician's relative strictness or leniency. This indicator can be interpreted as the expected sick-leave rate of a worker, conditional on nothing else than his primary care physician.

By construction, this practice style indicator seems like a suitable instrument for sick-leaves. Since it is estimated jointly with individual and workplace characteristics, it should be uncorrelated with the error term in (2.1), which is the first criterion for a good instrument. There is however, at least one, potential pitfall for the use of the leniency of the primary care physician as instrument variable for sick-leaves. In the Norwegian panel doctor system, workers are able to change primary care physician using a simple internet service as long as the physician has free capacity. If poorly motivated workers tend to register with the most lenient physicians the instrument is no longer valid. Using several robustness tests in section 4 I conclude that the instrument variable is suitable.

The second criterion for an instrument variable, that it is correlated with sick-leave, can easily be tested. It turns out that the PPI is highly correlated with sick-leaves, and is consequently a strong instrument.

Physicians' leniency is consequently a suitable instrument variable for our purpose. $a_{i,t}$ in (2.1) is then replaced by $\hat{a}_{i,t}$ which is the predicted values from estimation of (2.2).

$$a_t = \eta_1 \log y_{t-1} + X_t \eta_2 + \eta_3 PPI_t + u_t \qquad (2.2)$$

The practice style indicator is available for 89.1 percent of the total sample used in this paper. By comparing workers with and without available instrument, as well as estimating (2.1) for the sub-samples with and without available instrument using OLS, it seems like the selection problems related to this are negligible.

There is substantial variation in physicians' leniency within the sample, making predicted sick-leave rates of a worker - conditional on physician only - vary from below 4 and up to 14 percent. Most of the workers are however registered with physicians that imply expected sick-leave rates of 6 to 10 percent. The variation is illustrated in Figure 1 which draws a histogram for the PPI.

[FIGURE 1]

There is a discussion whether primary care physicians actually are able to act as gatekeepers to public services and benefits or not. The typical argument is that there is private information in health such that physicians are unable to observe the true health status of their patients. When seeing a patient claiming unobservable sickness, the physician must choose whether or not to put faith in his patient's self-assessment. A typical physician will put more weight on avoiding declining truly sick patients than on reducing moral hazard (Stone 1984, p.150). This has led authors to conclude that the gates are virtually wide open (Carlsen and Nyborg, 2009). A recent paper, studying the effect of a reform in the criteria for sick-leave certification, keeping all incentives and regulations for workers constant, documents that such a physician directed reform reduced sick-leaves by nearly 20 percent (Markussen, 2009). The pessimistic view taken by Carlsen, Nyborg and Stone takes the informational asymmetry to the extreme as if all workers decide on their desired treatment (absence) before visiting the doctor - and the doctor has no other choice but to give in. The view taken in this paper is that physicians influence sickness certification, not only by refusing to certify sick-leaves, but also by convincing workers that absence is not necessary treatment for many illnesses or medical conditions. To what extent the physician recommends rest or work as treatment is what determines whether the physician is strict or lenient.

3. Results

Main results

The model (2.1) and (2.2) is estimated on the Full sample as well as on the

Restricted sample consisting only of workers employed full-time also in the years *t-1* and *t+2* presented in Table 1. It turns out that this distinction is very important for the results which indicate that part of the effect of sick-leaves on earnings comes from a reduction in labor supply. The main results are presented in Table 2.

In the first row, to the left in Table 2, the estimated costs of sick-leaves are shown for the Full sample. On average, one day on sick-leave reduces future earnings by approximately .3 percent. This effect is 50 percent stronger than the effect reported by Hansen (2000), but only 1/8 the cost reported by Ichino and Moretti (2009). The latter report however the cost of cyclical absences which – I believe – must be interpreted as a measure of permanent health or productivity differences rather than the causal effect of just one additional day on sick-leave.

The estimated effect using instrument variable regression is the same as the coefficient obtained from ordinary least squared. This is perhaps surprising as IV removes the selection bias we would expect to bias the OLS estimate upwards. There are however at least two other mechanisms that may work in the opposite direction. The first is that IV-regression typically captures the effect of the treatment - sickness absence - for those workers who will take treatment if connected to a lenient physician but not otherwise. Hence, the IV-estimator captures the local average treatment effect - LATE (Angrist, 2001). If the effects are heterogeneous, LATE may differ from the average treatment effect. Second, if the effects of sick-leaves on earnings are non-linear in absence duration IV and OLS may differ. Whereas the whole distribution of sick-leaves is used to identify β^{OLS}, the narrower distribution of predicted sick-leaves from the first stage regression is used to identify β^{IV}. The linear estimator is a weighted sum of the marginal effects of a model that is non-parametric in the regressors, and the frequencies of each spell length are the weights. Since these weights differ between OLS and IV, this will bias the IV estimator (Mogstad and Wiswall, 2009). If the marginal effect of sick-leaves on earnings is decreasing in the number of absence days – which is indicated by the results below – this non-linearity may bias the results upwards. Hence, the reason why $\beta^{OLS} \approx \beta^{IV}$ can be that these three effects roughly cancel out.

Table 2: The effect of sick-leaves on earnings: Main results

	Full sample		Restricted sample	
Dep.var: $\log y_{t+2}$	OLS	IV	OLS	IV
sick-leave duration*	-.0030	-.0029	-.0005	-.0006
	(.0000)	(.0001)	(.0000)	(.0001)
sick-leave duration if > 0	-.0030	-.0024	-.0004	-.0003
	(.0000)	(.0001)	(.0000)	(.0001)
sick-leave indicator***	-.1481	-.3390	-.0373	-.0648
	(.0007)	(.0114)	(.0003)	(.0059)
# obs.	4 361 829	**	2 766 901	**

IV-regression: first stage results

Dep.var: a_t	Duration	Duration if > 0	Indicator	Duration	Duration if > 0	Indicator
PPI	257.08	342.98	2.235	215.73	300.42	2.114
	(1.85)	(3.97)	(.018)	(2.03)	(4.51)	(.022)
# obs.	3 887 461	1 546 934	3 887 461	2 468 533	951 431	2 468 533

Notes: Results from estimation of (2.1) and (2.2). All estimates control for log earnings t-1, sector of work, years of education, gender, age, country or origin, year and marital status. Robust standard errors clustered on individuals are reported in brackets. *A worker's number of sick-leave days in year t. ** The number of observations is displayed below the first stage results. ***An indicator taking 1 if the worker had any sick-leaves in *t*, zero otherwise.

The second row of estimates in Table 2 shows the effect on earnings of one additional day on sick-leave, conditional on being on sick-leave for at least one day in year *t*. Hence, the effect of sick-leaves on earnings is now estimated on a restricted sample only consisting of workers with at least one day on sick-leave. The IV estimate of a 0.24 percent *additional day cost* is significantly lower that the *overall cost* of 0.29 percent. This indicates that that the effect in fact is non-linear. This is also confirmed by the third line of estimates showing the effect of having any sick-leave, regardless of the number of days. Sick-leave duration is now replaced by a dummy variable taking one if sick-leave is positive and zero otherwise. When we compare workers with and without sick-leaves using OLS, workers with sick-leaves earn 14.8 percent less two years later. The IV-estimate is as high as 33.9 percent. One should however keep in mind that this estimate is not directly comparable to the OLS estimate as it is the effect of going from zero to one probability of sick-leave. More realistically, we can compare workers connected to a physician on the 10th percentile and 90th percentile in PPI-distribution. Workers with physicians at the 10th percentile are, on average, absent 6.4 percent of the time, whereas those with physicians at the 90th percentile are absent 9.7 percent of the time. This implies an earnings reduction of 1.12 percent.

To quantify the *first-day cost* of sick-leave I rescale the estimate for the sick-leave indicator shown in the third line of estimates in Table 2 by dividing the coefficient with the mean number of sick-leave days for workers with at least one day of sick-leave – 54.5 days. Going from zero to one day of sick-leave reduces earnings by 0.62 percent two years later. When a sick-leave is started, one additional day on sick-leave cost 0.24 percent. The *overall-*, *first-day-* and *additional day cost* are summarized in Table 3.

Table 3: The per-day cost of sick-leaves (in percent)

Full sample	Full sample	Restricted sample
Overall cost	0.29	0.06
	(0.01)	(0.01)
First-day cost	0.62	0.14
	(0.02)	(0.01)
Additional day cost	0.24	0.03
	(0.01)	(0.01)

Note: The estimated costs of sick-leave, based on the Instrumental Variable estimator. The first-day cost is the estimated coefficient when an indicator variable for sick-leaves are used as explanatory variable, divided by the mean number of days on sick-leave if at all, displayed in Table 1. Robust standard errors in brackets.

A non-linear cost of sick-leaves seems intuitive. If we consider sick-leave a measure of effort, zero sick-leave is the maximum effort one can provide. Hence, as a measure of effort sick-leave is censored. The marginal cost of going from zero to a positive amount of sick-leave may thus be higher than when the number of sick-leave days are increased from an already positive level, as the worker no longer is associated with the "max-effort/no sick-leave" pool of workers.

When I restrict the sample to contain only full-time workers, also in the years *t-1* and *t+2*, the effects are substantially smaller. One additional day on sick-leave reduces future earnings by .06 percent. This indicates that sick-leaves affect earnings not just through wages but also through future employment prospects. One way to interpret this is that absence may be an (partly) absorbing state. Markussen et al. (2009) estimates a duration model showing that there is strong negative duration dependence for transitions out of sick-leave. In other words, when you first get in - it might be hard to get out. This is probably particularly important for workers in the fringes of the labor market. For these workers, transiting between different types of employment, unemployment insurance and rehabilitation benefits or welfare-to-work programs, labor supply may be particularly sensitive to sickness certification leniency. Hence, the group of "core workers" in the restricted sample constitutes an interesting comparison as these effects originate from a change in wages or hours worked – within a full-time position. The overall cost of a 0.6 percent earnings reduction per day of sick-leave is still highly substantial.

Persistence and economic significance

So far, the effects of sick-leaves on earnings two years after sickness are estimated. However, the choice of timing is to some extent arbitrary. Whether these effects are significant from a substantial point of view will depend on their persistence. To investigate this I restrict the model to a sub-sample based on the 2001 vintage of the dataset. We can then follow these workers' earnings up to 2006 and estimate the effect on earnings in all these years. Again this is done for two groups of workers. The upper panel of Figure 2 presents the results for the Full sample, i.e. all full-time workers in 2001 and the lower panel presents the results for the Restricted sample.[4]

[FIGURE 2]

Figure 2 shows the effects of sick-leaves in year *t* on earnings in years *t* to *t+5*, for the full and the Restricted sample. In the upper panel, results for the Full sample are

[4]For robustness I have also estimated the effect of sick-leave in *t* on earnings in *t*, *t+1*, *t+2* and *t+3* on the 2003 vintage of the data. The estimated coefficients are remarkably similar to those presented in Figure 2.

presented. The first year the effect is almost negligible. Effects on earnings in *t+1* are modest whereas they increase to *t+2*. Thereafter β^{OLS} are slightly lowered and β^{IV} increases first and then remain constant. In the lower panel, the same exercise is carried out on the Restricted sample. Again there are no effects on earnings in *t*. The effects from OLS and IV are similar for the years *t+1* and *t+2*. Thereafter the effect estimated with OLS grows slowly over time whereas the effects from the IV estimator become substantially stronger in the years *t+3* to *t+5*. Keep in mind that the Restricted sample consists of workers employed full-time in *t+2*. There are however no conditioning on labor market status after this. Hence, in year *t+3,* some of these workers may have reduced their labor supply. Interestingly, this effect is the strongest for the IV estimator, indicating that signaling and its consequences for motivation and labor market possibilities, not sorting, is the dominating mechanism.

The IV estimator indicates that the effect is not just persistent, it is amplified over time. One should interpret this with some caution. Most workers do not change primary care physician from year to year such that the growth over time may be due to higher absence propensity even in subsequent years. Overall, I conclude from this that the effect is persistent but that the IV estimator may overshoot the long-term effects of a particular absence spell, as most workers stay with the same physician from year to year.

To illustrate the economic significance of these effects, consider the following example. Earnings are often considered a persistent autocorrelated process with a drift. If so, a reduction in earnings one year will leave a scar for all future periods. This is also supported by the strong persistence of the earnings effects shown in Figure 2. Consequently, a seemingly small reduction on earnings two years after sickness may sum up to a large amount over time. If one assumes an AR1 process for earnings and that interest rates, earnings growth, and discounting cancels out, the present value of such an earnings loss measured as the share of current earnings can be written as in (2.3). The *gross* cost *C* is a function of the remaining *T* years on the labor market, the estimated earnings reduction from sick-leave β and the coefficient of autocorrelation ρ.

$$C(T,\beta,\rho)=\beta\sum_{t=1}^{T}\rho^{t-1} \tag{2.3}$$

In Table 4, these gross costs of sick-leave are illustrated for workers with various numbers of years left on the labor market and for different levels of ρ. As these costs are conditional on that the workers actually remain on the labor market I use the cost of sick-leave from the Restricted sample.

Table 4: Gross costs of one day sick-leave (illustration)

Working years left	$\rho = 0$	$\rho = .5$	$\rho = .75$	$\rho = .9$	$\rho = .99$
1	.0006	.0006	.0006	.0006	.0006
10	.0006	.0009	.0019	.0036	.0058
20	.0006	.0009	.0020	.0049	.011
30	.0006	.0009	.0020	.0053	.0157

Note: The gross marginal cost of sick-leave, using (Gross cost) and $\beta = .0006$, measured as the share of yearly earnings.

For a worker earning USD 60.000 a year and with 10 years left on the labor market, each day on sick-leave will cost him from 36$to 348$ depending on the persistence of earnings. Without any sickness insurance payments, the loss of current earnings from one day of sick-leave would be around 190$ after taxes.[5] Hence, for reasonably high values

[5]60 000$ divided by 220 working days, subtracted 30 percent taxes.

of ρ , the reduction in future earnings weighs up for the lack of incentives resulting from the generous sickness insurance arrangements in Norway.

Heterogeneous effects

Hansen (2000) reports that while females' wages are reduced following sick-leaves, wages of male workers are not affected. Contrary, Ichino and Moretti (2009) find that sick-leaves are less costly for females than for males. To investigate this as well as whether the effects differ between old and young, public and private sector jobs, high and low educated workers, the model is estimated on sub-samples only containing parts of the workers. The sample is divided in two after age, calling workers aged 39 or less "young" and workers aged 40 and above "old". Workers with more than high school education, i.e. more than 13 years of schooling, are grouped as "high education" whereas those with 13 or less years of schooling are grouped as "low education". The sample is also divided in four groups, after which income quartile the workers' belong to in year *t*. The results are shown in Table 5.

The effects of sick-leave on earnings are fairly similar between young and old workers. In the Full-sample, effects are strongest for the old whereas effects are strongest for the young in the Restricted sample. This indicate that subsequent wage changes are relatively more important for the young while changes in employment are more important for the old.

Table 5: Different effects for different workers

	Full sample		Restricted sample	
	OLS	IV	OLS	IV
Young	-.00303**	-.00255**	-.00062**	-.00070**
Old	-.00295**	-.00302**	-.00041**	-.00053**
Men	-.00346**	-.00369**	-.00054**	-.00097**
Women	-.00253**	-.00210**	-.00044**	-.00018**
Low edu.	-.00328**	-.00304**	-.00048**	-.00054**
High edu.	-.00223**	-.00274**	-.00052**	-.00097**
Private	-.00353**	-.00341**	-.00057**	-.00068**
Public	-.00216**	-.00225**	-.00038**	-.00060**
Earnings quartiles in year *t*				
Q1	-.00331**	-.00300**	-.00033**	-.00006
Q2	-.00264**	-.00247**	-.00035**	-.00035**
Q3	-.00238**	-.00211**	-.00041**	-.00045**
Q4	-.00235**	-.00372**	-.00069**	-.00245**

Notes: Results from estimation of (2.1) and (2.2) using 2SLS. All estimates control for log earnings t-1, sector of work, years of education, gender, age, country of origin, year and marital status. Robust s.e., clustered on individuals in brackets. Stars indicate statistical significance: $* = p < 0.05$, $** \; p < 0.01$.

The effects are substantially smaller for females than males. Two years after sick-leave, male workers' earnings are reduced by .4 percent per day on sick-leave. The same figure for females is just .2 percent. This difference is even larger in the Restricted sample. This is contrary to the findings by Hansen (2000) but in line with the findings of Ichino and Moretti (2009). The differences between men and women are smaller when using OLS than IV, indicating that gender differences follows from the firms' response not the sorting of employees.

The effects are substantially larger for workers in private sector jobs than for workers employed in the public sector. This is in line with a notion that wage setting is less regulated in the private sector.

In the Full sample, sick-leaves are the most costly for low educated workers. In the Restriced sample it is opposite. This pattern is confirmed by the lower part of Table 5 showing the effects of workers in each income quartile in year *t*. In the Full sample, sick-leaves are the most costly for workers in the tails of the income distribution. However, in the Restricted sample, there is no longer any effect of sick-leaves on subsequent earnings for workers in the lower tail. The effect is however strong for high earners. This indicates that sick-leaves affect different workers differently. For “marginal workers” in the lower tail of the income distribution, often low educated, sick-leaves reduce subsequent labor supply. However, if employment is maintained, their earnings are not much affected. For the high earners, sick-leaves are not that important for subsequent employment – but their earnings are substantially reduced.

Figure 3 displays the estimated coefficients in Table 5 together with the mean number of sick-leave days for workers in these sub-groups, both using the Restricted sample to focus on punishment effects and not sorting. The graph shows that sick-leave and its' cost seem strongly related, the higher cost - the lower sick-leave. However, any causal relationship between the two remains to be verified.

[FIGURE 3]

4. Robustness

The first-stage regressions of (2.2) show clearly that physician leniency is strongly correlated with sick-leaves and hence is a strong instrument variable. The second fundamental requirement to an instrument variable is that it is uncorrelated to the error term in the second stage equation (2.1). There is no straight forward econometric test of this crucial assumption but I will in this section show the results from three different exercises that all supports the validity of the instrument variable.

Two concerns are addressed regarding the IV-estimator. The first is whether the instrument capture other local (geographically) characteristics such that its effects are spurious. The second is whether there is selection of workers into physicians as workers' may choose their physicians "strategically".

In the last section of this chapter I estimate the effects of sick-leaves on wages and hours worked, using the Wagestat sample described in Table 1.

Local characteristics captured by the instrument

Jointly with the physician fixed effects estimated in Markussen et al. (2009) are also fixed effects for all workplaces with at least 100 employees estimated. Hence, for most workers, it will be estimated both physician effects and workplace effects simultaneously. However, for workers in small firms and workers connected to physicians with few patients, the physicians' leniency indicator may capture features not captured by other covariates in the estimated model. To test if this biases the results the IV-model is re-estimated with dummies for each of Norway's 430 municipalities. The left side of the upper panel of Table 6 shows the estimated effects are unchanged when municipality dummies are included.

I also restrict the sample to only consist of workers in large firms (more than 100 employees) and physicians with large patients list (more than 400 patients), such that I only use observations were physician and workplace effects are estimated simultaneously. These results are shown in the right part of the upper panel of Table 6. Neither of this changes the estimates much. Hence, it seems like the estimates are not driven by other local factors causing spurious regression.

Selection of workers into physicians

Another potential pitfall for the use of physicians' leniency as instrument for sick-leaves is that patients may choose physicians "strategically". Poorly motivated workers may prefer lenient doctors. If empirically important, this will make the instrument correlated with the error term in (2.1) and unsuitable as an instrument. There is no simple econometric test for this problem.

One possible path to follow is to search for changes in the matching of workers and physicians that are exogenous to the worker. One such reason could simply be that every year some physicians retire and new ones establish new practices. Another reason is that physicians move to other parts of the country. By using the matching between physicians and workers I can identify physicians whose complete list of patients in year *t* is changed from year *t-1*. For each physician I calculate the share of his stock of patients at the end of year *t* who also had this physician at the end of year *t-1*. The median rate of change in the patient stock is, in my sample, 5.6 percent, the average is 8.1 percent and the 99th percentile is 50 percent. I then estimate the model in (2.1) and (2.2) on a sample consisting *only* of workers whose physician changed his stock of patients completely from year *t-1* to *t*, i.e. that the change rate is 1. Hence, this sample consists of workers whose physician is new, i.e. did not practice as primary care physician the year before, or have moved to another part of the country. The possibility for poorly motivated workers to choose lenient physicians is much more limited in this case as the physician have had much less time to build a reputation.

The sample is substantially reduced when conditioning on workers connected to such new or relocated physicians and the number of observations where the instrument *PPI* is available is 18 373 in the Full sample and 13 912 in the Restricted sample. We should thus expect the estimates to be much less precise. The results in the center part of Table 6 show that, in the Full sample, the estimated coefficients when we restrict the data to only consist of workers whose physician are either new or relocated, are very similar to the main results presented in Table 2. The same is also true in the Restricted sample, but the coefficients are estimated with less precision and are not statistically significant from zero. However, the overall impression from this exercise is that it seems less likely that the estimated effects in Table 2 are caused by selection.

In addition to the preceding exercise I also suggest a second, simple exercise. Intuitively, the problem we are investigated is how the leniency of a worker's physician affects his earnings. We think of two potential channels for such an influence; through sick-leaves and through some unobservable variable we can think of as *motivation*. To address this problem I do the following: First, replace sick-leave by the physicians' leniency indicator and estimate (2.1) using OLS. The estimated coefficient, displayed in the lower part of Table 6, capture both potential channels at work. Secondly I estimate (2.1) again but this time *PPI and* sick-leave is included among the regressors. Intuitively, if the effect of *PPI* works through sick-leaves only, physicians' leniency should no longer affect earnings when sick-leave is included in the model. Due to a potential non-linear relationship between *PPI* and sick-leaves, non-linear transformations of sick-leaves are also included in the equation. Hence, this serves as a loose test of this potential problem.

The two lower panels of Table 6 show that the instrument has a strong impact on earnings in *t+2* when sick-leave is not included in the regression. When sick-leaves are included the instrument has no impact on earnings in the Full sample. In the Restricted sample, the coefficient for the instrument drops dramatically when sick-leave is included, even if it is still non-zero. When non-linear terms for sick-leave are included the instrument has no effect on earnings. Hence, it seems as the instrument works as it is supposed to, as a predictor for sick-leaves but not for earnings - except through sick-leaves.

Table 6: IV robustness tests

Test for local characteristics – captured by the instrument				
	Municipality dummies		Only large firms	
Dep.var: $\log w_{t+2}$	OLS	IV	OLS	IV
Sick-leave, a_t	-.0030**	-.0030**	-.0029**	-.0032**
#obs.	4 361 829	3 887 461	1 909 365	1 766 321
Instrument validity 1: physician induced changes in worker-physician matching				
	Full sample		Restricted sample	
sick-leave	OLS	IV	OLS	IV
..duration	-.0034***	-.0034**	-.0006***	-.00095
..indicator	-.1652***	-.3801**	-.0447***	-.0976
# obs.	23 946	18 373	17 186	13 912
First-stage IV regression: effect of *PPI* on sick-leave				
..duration	-	223.9***	-	206.7***
..indicator	-	2.055***	-	2.020***
Instrument validity 2: the effect of physician leniency works through sick-leaves				
Full sample	(1)	(2)	(3)	(4)
GP leniency	-.757**	.015	-.037	-.023
Sick-leave	-	-.003**	-.002**	-.003**
..squared	-	-	-4.4e-6**	5.8e-6**
..cubic	-	-	-	-.25e-08**
Restricted sample	(1)	(2)	(3)	(4)
GP leniency	-.137**	-.029*	-.012	-.006
Sick-leave	-	-.001**	-.001**	-.0013**
.. squared	-	-	1.7e-06**	6.7e-06**
..cubic	-	-	-	-1.3e-08**

Notes: Results from estimation of (2.1) and (2.2) using OLS/2SLS. All estimates control for log earnings t-1, sector of work, years of education, gender, age, country of origin, year Robust s.e., clustered on individuals in brackets. Stars indicate statistical significance: $* = p < 0.05,\ ** p < 0.01, *** \ p < 0.001$

Effect on wages or hours?

A final test is to check whether the effect on earnings really originates from changes in wages and not a change in hours. To do so, an additional dataset provided by Statistics Norway, Wagestat, containing monthly wages and hours for a subset of the workers in our panel is used. This sub-sample oversamples workers in large firms. To be able to calculate wages and hours, such information must be available also in $t+2$ and $t-1$. Hence, this sample is closer to the Restricted sample and effects should not be compared to the coefficients from the Full sample presented in Table 2. From these monthly earnings and hours, wages and work hours per day for each worker in this subset are calculated. By estimating the effect on log earnings, log hourly wage and log hours for the same sub-sample of workers the change in earnings can be decomposed into changes in wages and hours. This is shown in Table 7.

Table 7: Effects on earnings, hours and wages

	log(Earnings)	log(Wages)	log(Hours worked)
OLS	-.0005**	-.0002**	-.0002**
IV	-.0007**	-.0006**	-.0001

Notes: Results from estimation of (2.1) and (2.2) using OLS/2SLS. All estimates control for log earnings t-1, sector of work, years of education, age, gender, country of origin, year and marital status. Robust s.e., clustered on individuals in brackets. The sample used (Wagestat) differs from the other estimations as wages and hours are available only for a subset of the data. Stars indicate statistical significance: $* = p < 0.05$, $** p < 0.01$

Recall from Table 2 that conditional on nothing more than being full-time employed in year *t*, one additional day on sick-leave reduces future earnings by as much as .3 percent. When the sample was restricted to only cover workers in full-time employment in the years *t-1* and *t+2*, the effect of one day sick-leave on earnings is .06 percent. In the Wagestat sample, this effect is .07 percent per day. The second and third columns of Table 7 show the effect of sick-leaves on log wages and log hours worked. Reduced wages are the main component of the reduction in earnings caused by sick-leave, accounting for 50-70 percent of the change in earnings. There is also a small, but statistically insignificant, effect on hours.

Discussion: behavioral mechanisms and implications

A striking observation so far is that sick-leaves seem to affect female workers' earnings far less than male workers. We have also seen that sick-leaves are more costly for high earners in the private sector than others. A natural question is then whether the reason why sick-leaves are more costly for males is just because men are overrepresented among high-earners in the private sector. To investigate this I have estimated the IV-model, (2.1) and (2.2), conditional on workers' position in the earnings distribution, gender and sector of employment. In total, the dataset is divided into 16 cells; 4 earnings quartiles, females and males, public and private sector. Regardless of whether I estimate on the full or Restricted sample or consider the "first-day cost" or the "per day cost", sick-leave is more costly for male workers.[6] Hence, the reason why sick-leave is more costly for males is not just that men earn more and work in the private sector.

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[6]There is one exception: sick-leave is more costly for females than males for public sector employees in the third earnings quartile. The difference is not statistically significant. The complete results are available on request.

Another potential explanation why sick-leaves are less costly for females is that females' absences are more tolerated because they are partly related to pregnancy and caretaking for small children. For these reasons sick-leaves may be less of a signal of low effort for females, and should hence not be punished the same way as for males. This is also argued by Hansen (2000) who finds that own sickness reduces wages but not sick-leave due to sick children. Unfortunately, the data used in this paper do not cover sick-leave due to sick children.[7] To investigate whether children can be the reason why sick-leaves are less costly for females, all female workers below 45 years of age are divided in sub-samples with and without children, employed in the private and the public sector, and earnings costs are estimated in each of these 4 groups. Again this is done both on the Full and the Restricted sample, and for "first-day costs" and "per day costs", using the IV-estimator. Despite some variation due to the smaller samples in use there are no evidence indicating that sick-leaves for females without children are more (or less) costly than for females with children. Hence, it seems unlikely that children are the reason why sick-leaves are less costly for female workers.

It is well known that females at all ages have higher sick-leave rates than males (see e.g. Ichino and Moretti 2009 or Markussen et al. 2009). An explanation why sick-leave is less costly for females may be found by linking the gender wage gap with the gender differential in sick-leave. Consider an employer deciding which employees are to be given a wage increase. When doing so the employer must have an expectation about

[7]Workers with small children have a quota of ten sick-leave days a year to take care of sick children. These absence spells are not covered by this dataset.

the future productivity of his workers. This expectation can be thought of as a mix of the signals the employer has got from the employees behavior over time (a learning process) and the employer's perception of how workers with certain characteristics - such as gender - behave. The latter is what is called statistical discrimination. The employer knows, by experience or otherwise, that female workers, in general, have more sick-leave than males. Hence, particularly when the employers' knowledge about their employees are limited, such as when the employment relation is fairly new, it can very well happen that such group characteristics may influence employers' expectations. If such discrimination takes place, the potential wage increase is smaller for female workers than for males. In many cases wages are, for all practical purposes, downward rigid, such that when the potential wage increase is reduced, due to statistical discrimination, the scope for sick-leaves to affect earnings are smaller for females than for males. Such an argument could (at least partly) explain why females earn less and why their earnings are less affected by sick-leave. A familiar argument, in the context of education, is made by Altonji and Pierret (2001) who hypothesize and find support for a claim that statistical discrimination decreases over time, as employers learn to know their employees. In the context of sick-leaves and earnings we should expect female workers to be treated more like men as they get older and as employers learn about their individual productivity. I test this by dividing the sample into six groups after age and gender. The estimated earnings cost of sick-leave are very different between men and women for the youngest group, whereas they become increasingly similar as the workers get older. Further testing of the empirical relevance of such an argument is left for future research.

The findings in this paper of a persistent wage cost from sick-leaves can provide new explanations for - and is in accordance with - several stylized labor market facts. First, such a cost provides young workers with much stronger incentives than old workers, fitting well with how absence rates are increasing by age. Second, it is also well known that female workers earn less than males and the gender wage gap is often considered to increase the most in the presence of young children. Females also have substantially more sick-leave than males, and especially when caring for small children. Hence, sick-leaves may account for parts of the gender wage gap as well as the earnings gap between females with and without children. Third, sick-leave is punished the most for high earners, highly educated and private sector workers. These groups of workers are also less absent. So far these observations are just that - observations - and their possibly causal relationship not in any sense investigated. This is however interesting questions for future research.

Conclusion

Using detailed Norwegian administrative data covering all employees, their sick-leaves and diagnoses, earnings and primary care physicians, this paper estimates the causal effect of sick-leave on earnings, and find - in accordance with previous studies by Hansen (2000) and Ichino and Moretti (2009) - that such effects exists. The first day on certified sick-leave, i.e. starting an absence episode, reduces future earnings by .6 percent. Thereafter, each additional day on sick-leave reduces earnings two years after by .3 percent. It turns out that the estimated effects are strongly persistent. Hence, even if the coefficients may seem small, the costs of sick-leave, especially for young workers with a long working career ahead, are substantial.

This paper raises serious concerns on the limitations of generous welfare states. The sickness insurance system attempts to insure workers against sickness by providing income fully or partly during sickness. Based on the results of this paper it makes sense to ask whether this insurance payment in fact is a loan - not a payment. Workers away with sickness do get their insurance payment during sickness, but they end up "paying it back" in all subsequent working years as their wages are reduced. An interesting question left entirely for future research is whether the results of this paper also hold for countries with less generous welfare arrangements. A hypothesis is that the wage cost of sick-leave is highest exactly in countries with the most generous insurance schemes as the lack of intra-periodic incentives gives need for inter-periodic incentives to discipline workers.

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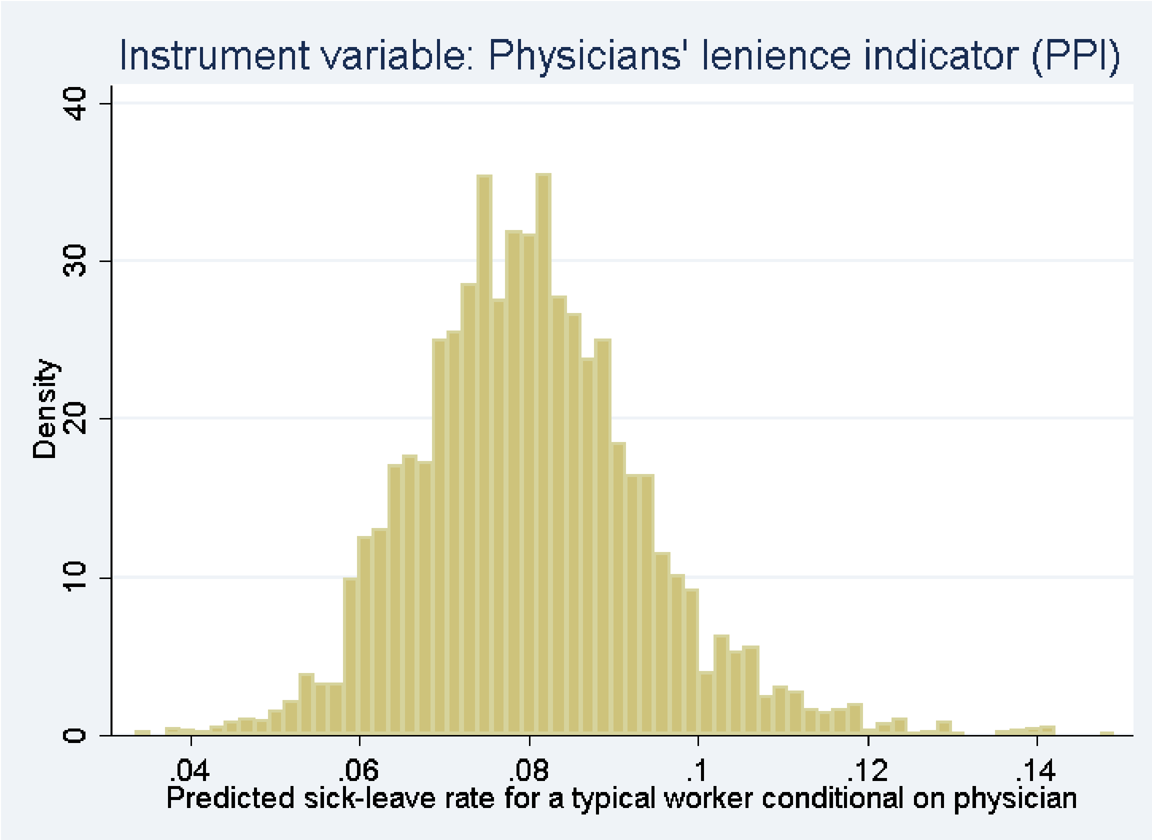
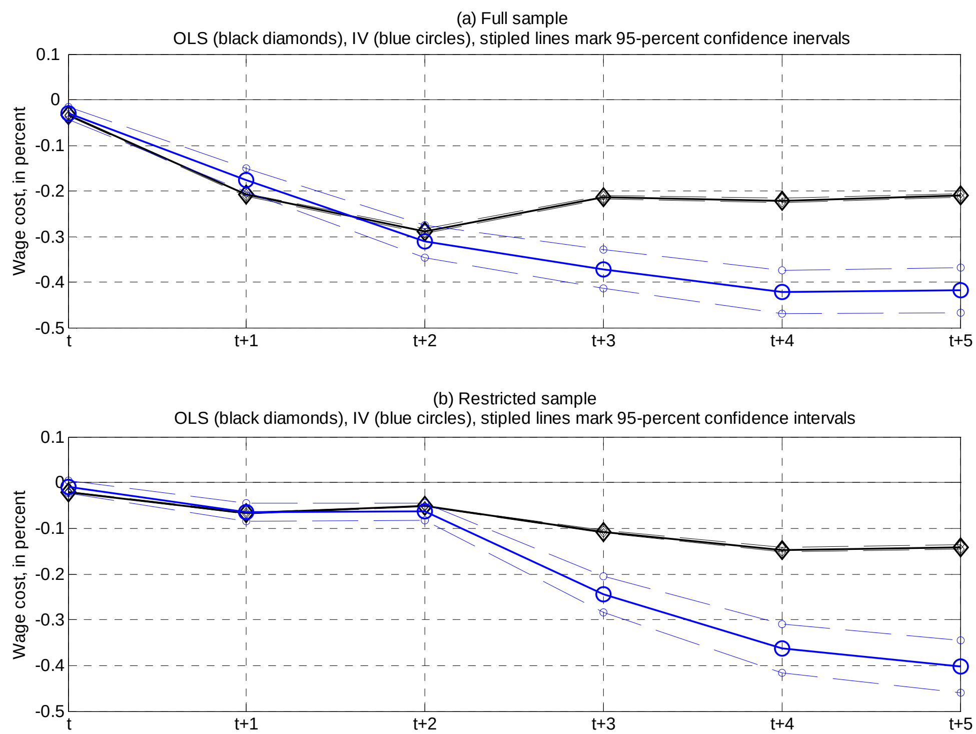
Figures

Figure 1:



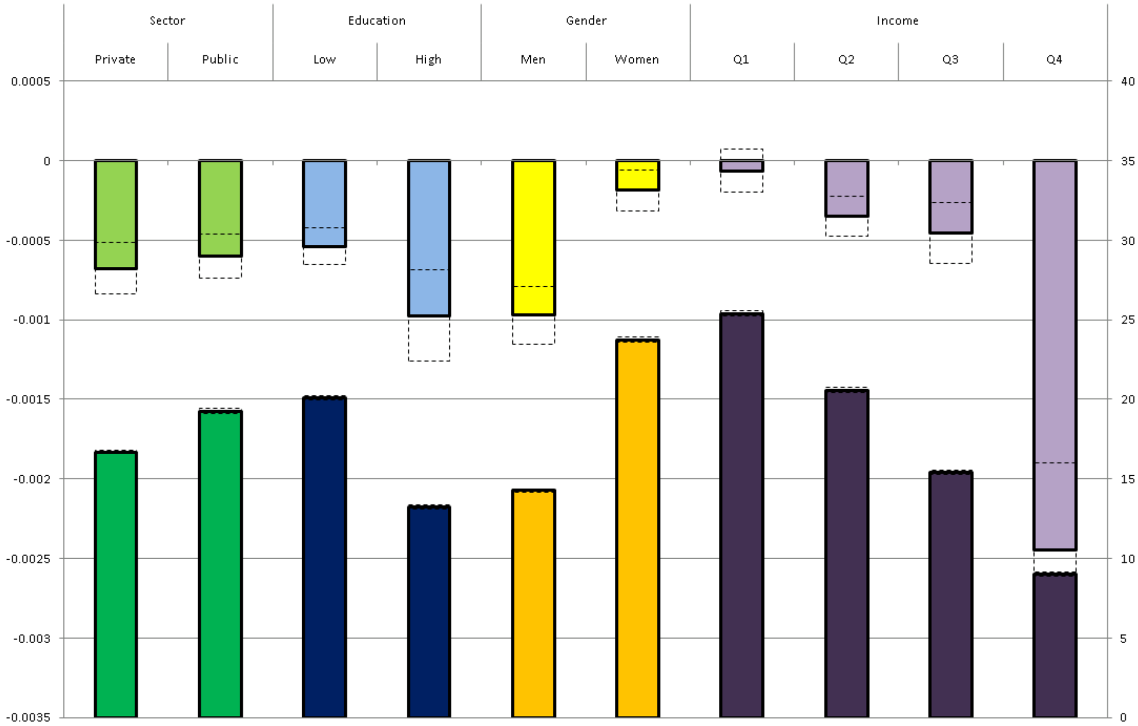
<u>Text:</u> Distribution of the instrument variable PPI – the primary care physicians' lenience indicator. The instrument can be interpreted as the expected absence rate (share of working time spent on sick-leave) conditional on physician.

Figure 2:



Text: The upper panel draws the estimated coefficients for sick-leaves on earnings in the years t to t+5 for the two estimators. The lower panel draws the same coefficients, but for the Restricted sample consisting of workers employed full-time also in the years t-1 and *t+2*. Note that as the conditioning on full-time is surpassed the estimated effects becomes substantially stronger.

Figure 3:



Text: The figure displays the estimated coefficients from Table 5 together with mean days of sickness absence in the corresponding sub-groups of workers – both from the Restricted sample consisting of workers employed full-time also in the years t-1 and t+2. The upper bars show the estimated earnings costs on the left axis. The lower bars show the groups' mean sick-leave rates on the right axis. The 95 percent confidence interval is drawn with the dashed lines.