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Sickness absence, sick leave pay and pay schemes

by

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Abstract

Utilising Norwegian linked register- and survey-data, while exploiting a discontinuity in public sick pay legislation, I show that the public sick pay compensation level causally affects male performance pay workers' sick leave days. Both male and female performance pay workers experience longer sick leaves when provided private supplementary sick pay compared to those being eligible for public sick pay only. This differential impact of the replacement rate on workers' sick leave rates reveals heterogeneous behavioural changes following public sick pay cuts, and this heterogeneity will be reinforced by the provision of employer-provided sick pay to attractive worker groups.

Key-words: Absenteeism, compensation, performance pay, regression-discontinuity approach, IV-Poisson regressions

JEL-Codes: H31, J22, J28, J32

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1. Introduction

In many redistributive welfare economies, such as Norway, absenteeism is expensive measured by the direct cost¹, and even more so if one takes into account the indirect production disruption costs. Under a different welfare regime, such as the U.S., the costs of presenteeism also raise quite a concern (Goetzel et al., 2004; Hemp, 2004).² Since sick workers at work might transmit contagious diseases to other workers, sick pay can provide sick workers incentives to take time off to stop the spread of sickness (Skatun, 2003). Thus absenteeism and presenteeism are associated with costs, and firms and the society should be concerned about the relative costs and benefits of sick pay. However, under redistributive welfare regimes with generous public sick pay the danger of moral hazard is greater than when the cost of sick leaves is covered to a larger extent by the individual worker.

Most empirical evidence clearly indicates that incentives matter for absenteeism, regardless whether they are provided publicly (Johansson and Palme, 1996; Johansson and Palme, 2002; Henrekson and Persson, 2004; Ziebarth and Karlsson, 2010; Dale-Olsen, 2013) or privately (Barmby et al., 1995; Brown et al., 1999; Dale-Olsen, 2012). For example, Johansson and Palme, (2002) utilise joint changes in the Swedish tax system over time and sick pay cuts (which provides variation in the price of leisure), to show that as the cost of absenteeism increases for the workers, absenteeism drops. Similarly, when comparing the incidence rate and the number of sick

¹ According to Norway's National Budget 2010 publicly paid sick pay constitute 1.5 per cent of GDP(http://www.statsbudsjettet.dep.no/upload/Statsbudsjett_2010/dokumenter/pdf/summary_national%20budget_2010.pdf) (or 37.5 billion Nok), in addition comes the privately paid sick pay. Even in the UK, with less generous sick pay systems, it has been estimated that the direct cost of absence to the UK economy in 2003 was £11.6 billion (Barham and Begum, 2005). The European sickness absence insurance schemes are quite similar to the US temporary disability insurance, and temporary disability insurance benefits amounted in California in 2005 to \$4.2 billion, just slightly less than the amount spent on unemployment insurance (Ziebarth and Karlsson, 2010).

² Business web-pages, such as this <http://www.businessknowhow.com/manage/presenteeism.htm>, argue that the costs of presenteeism out-weight the costs of absenteeism.

leave days during pre- and post-periods (1996-8 and 2003-5) for a sample of 300 000 male workers and 800 workplaces, Dale-Olsen (2012) finds that the introduction of performance pay is significantly associated with lower incidence rates and fewer long-term sick leave days. On the other hand, the recent study of Ziebarth (2013) indicates that long-term sick individuals are not very responsive to monetary labour supply incentives.

Public sick pay reforms have different motivations depending on the established system and economic situation. In many of the Swedish reforms referred to in the analyses above (Johansson and Palme, 2002; Henrekson and Persson, 2004) the intentions of the reforms have been by reducing the compensation levels to cut public expenditures and to speed up individual recovery rates. Ziebarth and Karlsson (2010) explicitly state that the German reform in 1996 was “to reduce the degree of moral hazard in sickness absence insurance and to reduce labour costs in order to foster employment creation”. However, other sick pay reforms, for example those introducing public sick pay or increasing the compensation rates, might also be motivated by a desire to reduce or avoiding poverty among poor workers.

When the public sick pay legislation is supplemented by additional privately funded sick pay, as is seen in many welfare countries (Barmby et al., 2002), this may severely limit the effect of public sick pay reforms when it comes to speeding up individual recovery and reduce moral hazard. Cuts in the public sick pay might potentially be offset by the private sick compensation, which firms provide to attract and to retain valuable workers, and incentive-related remuneration might interact with the public sick pay legislation, thus influencing the absenteeism of workers. The heterogeneous effects of monetary labour supply incentives on absence found by Ziebarth (2013) might thus be explained by offsetting private pay schemes and sick pay schemes. Except for Barmby (2002), unfortunately, the literature is very scant on the interaction of public and private sick pay.

In this paper I exploit a discontinuity in the Norwegian public sick pay legislation to identify the causal impact of the sick pay compensation on workers' number of physician-

certified sickness absence days. In Norway publicly provided sick pay is capped at a level called 6G, where G expresses the National Social Service baseline figure (in 2004 around 4711£/8625\$), which is adjusted May 1st each year. 45 and 35 per cent of all private sector Norwegian male full-time workers and female full-time workers, respectively, receive pay above this limit, thus while a considerable number of workers experience capped public sick pay, these workers are not low-paid impoverished workers. Thus workers receiving pay closely below or closely above the cap are actually rather representative as typical full-time workers. At least a priori, there is no reason to anticipate that these workers deviate in any particular direction when it comes to sick leave behaviour.

If only publicly provided sick pay existed, one should be able to identify a causal impact of sick pay on absence by just comparing the absence behaviour of workers earning just below and just above this threshold. This strategy would follow from a regression-discontinuity approach, which has been successfully been shown to reveal causal relationships requiring mild assumptions (Hahn et al., 2001; Lee and Lemieux, 2009) compared to for example the IV-approach. Since the location just below or just above the earnings threshold and the relationship to sickness absence can hardly be related to motivation, fairness or responsibility perspectives, or to the issue of cost alignment between employers and workers, the RD-approach will highlight one mechanism; the incentive effect associated with the public sick pay legislation.

Since employers might provide additional sick pay as well, this strategy needs additional information to be successful. If a considerable number of the private sector employers provide supplementary sick pay this might occlude the sharp distinction between being just above or just below the sick pay threshold. However, by utilising Norwegian employer-employee register data for 2003-2004 linked to workplace questionnaire data information on privately provided sick pay, I am able analyse how the sick pay threshold affects workers' sickness absence days under two different sick pay regimes. This allows me to shed light on the causal impact of public sick pay on absenteeism, as well as how this impact will be affected by private sick pay schemes.

Since private sick pay schemes might be related to private pay schemes, and both schemes are associated with worker sorting and selection issues, this could invalidate the approach outlined above causing biased estimates regarding the effect of sick pay compensation. The method, however, hinges crucially on workers being unable to sort above and below the discontinuity, and such sorting cannot be achieved, regardless of pay scheme or sick pay scheme. Thus *conditional on pay and sick pay scheme* the estimates are clearly unbiased. On the other hand, we know that the effect of financial incentives on absence behaviour might reflect productivity and personality sorting, enhanced motivation, fairness and responsibility issues, or an alignment of the cost of absenteeism between employers and workers, either directly or related to increased awareness of the costs of absenteeism and the following consequences or selection of workers sharing such efficiency concerns.³ Thus the existing sorting makes it difficult to address how pay schemes affect workers' sick leaves, but then again, this is not the topic of this paper. Still, when drawing inferences *across pay and sick pay schemes* one must acknowledge the possibility that such inferences might be affected by sorting and selection issues.

To test the importance of variation in the sick pay scheme I also apply an IV-approach to study how the sick leave days of workers with earnings above the threshold differ depending on employers providing supplementary sick pay. First, if one worries that the utility differences arising at the discontinuity in the RD-analyses is too small to yield behavioural changes, sick

³ For discussion of motivation, fairness and responsibility, see Hassink and Koning(2009), Engellandt and Riphahn (2011), Rotemberg (2006) and Cappelen et al., 2010). As children grow older and become adolescents, their views concerning equality, equity and efficiency change (Almås et al., 2010), and similarly, workers' attitudes might change while working under a performance pay regime. Dohmen and Falk (2011) found that the higher output achieved under performance pay compared to fixed schemes, was primarily driven by productivity sorting. Furthermore, such a sorting reflect differences in risk attitudes, i.e., performance pay schemes attract workers of greater risk tolerance (Dohmen and Falk, 2011; Cornellissen et al., 2011). Since women, on average, tend to be more risk averse than men (Croson and Gneezy, 2004), Dohmen and Falk also found that women are less likely to sort to variable payment schemes than men.

leaves for these workers imply on average an income loss of 20 per cent if they receive public sick pay only, and this should clearly be large enough to induce different behaviour under the two sick pay regimes. Second, the IV-approach takes into account worker selection into workplaces depending on sick pay scheme.

Is it then possible to generalise my results to the general population? As pointed out above the workers receiving pay levels close to the sick pay gap can be interpreted as ordinary full-time workers, and part of the analyses takes into account the sorting of workers across sick pay regimes. However, inferences across pay regimes might still be affected by sorting. For example, workers employed under performance pay might be driven by extrinsic motivation, while workers employed under fixed pay could be primarily influenced by intrinsic motivation.⁴ Thus the results for performance pay workers cannot be generalised to the general population, but only to the population of workers employed under performance pay. Similarly, for fixed pay workers the results can be generalised to the fixed pay worker population. Evidently this restricts the overall generalisation of my results, but I argue that even if the my results are contingent on pay regimes, information on how workers react to differences in compensation rates will still be highly valuable for public policy makers. Their policies will most likely be chosen contingent on private pay schemes, i.e., it seems to me difficult for public policy makers in most modern economies to fully determine private sector pay setting regimes.

The structure of the remainder of the paper is as follows: Section 2 briefly presents an algebraic motivations for how remuneration schemes and private sick pay schemes affect workers' sickness absence behaviour. Section 3 describes the sick pay legislation and compensation system in Norway. Section 4 presents the empirical strategies. Data is presented in Section 5. The main empirical results are presented in Section 6. Section 7 briefly concludes.

⁴ See Deci (1975) and Benabou and Tirole (2003) for a discussion on extrinsic and intrinsic motivation.

2. Theoretic motivation

It is easy to assume that a causal impact of public sick pay on absenteeism should be most visible under a fixed pay regime with public sick pay only, where the number of sick days should drop when sick pay during sick leaves are no longer fully compensated. However, sick pay is only capped at $6G$, it does not drop to zero, thus if wages are continuously distributed around the threshold $6G$, we might not observe a big drop in absenteeism at $6G$. Performance pay may act as an enforcer of such a mechanism, by making the loss caused by absenteeism greater.

When the public sick pay is supplemented by private sick pay, it is reasonable to expect that no clear relationship will be observed. This is easily seen by considering the following simplified model, related to the “shirking”-models previously successfully applied to analyses of the relationship between sick leaves and pay (Barmby et al., 1994; Heywood and Jirjahn, 2004). In this framework, sick leaves related to shirking can directly be reinterpreted as effort (shirking implies less effort), a term quite commonly used in the agency and efficiency wage framework.

By choosing a sick leave level, a , when facing imperfect monitoring, N identical workers maximise expected utility. a can also be interpreted as the sick leave probability. By showing up on work a worker agrees to put up the contracted effort. As is common in “shirking”-models, work effort is assumed to be associated with disutility, i.e., one can derive a cost of effort function, C , expressed as a function of the presence probability $(1-a)$. We assume that C is a convex function, i.e., $C'(1-a) > 0$ and $C''(1-a) > 0$. Let the monitoring probability be $0 < m < 1$. An absent and monitored worker is fired. Each worker then maximises:

$$1) \quad (1-a)U(W) + (1-m)aU(S) + amU(R) - C(1-a),$$

where U denotes the Von Neumann-Morgenstern utility function, W is the current wage, S is sick pay, and R is the outside options. The first order condition is then given by:

$$2) \quad U(W) - U(S) + m[U(S) - U(R)] = C'(1-a),$$

i.e., workers choose their absence level equalising marginal benefits marginal effort costs.

When private sick pay supplements public sick pay workers are more likely to be fully covered, and thus only the monitoring intensity determines the absence level. The monitoring intensity might differ depending on the provision of private sick pay, but it is hard to argue that this will differ markedly just below and just above $6G$ (particularly when we argue that even workers cannot completely determine their earnings). When only public sick pay is provided, the absence levels will increasingly drop as earnings pass the $6G$ -limit. If sick pay only covers fixed pay and bonuses are increasing with the pay levels, then we should observe a stronger drop at $6G$ under performance pay than under fixed pay.

This can be seen as follows. Assume that $C(1-a)=c(1-a)^2$, where c expresses a positive parameter indicating the cost of providing effort. For $W \leq 6G$ full compensation is provided, thus $da/dW = -mU'/c < 0$ and the optimum absence levels for workers is $(1-a)^* = m[U(W)-U(R)]/c$, where $W \leq 6G$. For $W > 6G$ then $da/dW = -U'/c < 0$ and the corresponding optimum absence level for workers is given by $(1-a)^{G*} = \{U(W)-U(S)+m[U(S)-U(R)]\}/c$. $(1-a)^* < (1-a)^{G*}$, but the difference equals $[U(W)-U(S)]/c$ and thus might be small for an infinitesimally increase in W above S .

The outline above might be interpreted as a fixed pay case. Assume instead that W can be split in two parts: a fixed part, k , and a part depending on the absence level, $b(1-a)$, where b expresses the piece-rate on performance. Under full compensation, $W \leq 6G$, then under certain assumptions: $da/dW = [mU' + (1-a)bU'']/[bU' - c] < 0$, with the optimum absence level for workers expressed as $(1-a)^* = m[U(W)-U(R)]/[c-bU']$, where $W \leq 6G$.

For $W > 6G$ then $da/dW = [U(W)-U(S)+mU' + (1-a)bU'']/[bU' - c] < 0$ and the corresponding optimum absence level for workers is given by $(1-a)^{G*} = \{U(W)-U(S)+m[U(S)-U(R)]\}/[c-bU']$. Once again we see that $(1-a)^* < (1-a)^{G*}$, but this drop is larger than the similar drop at $6G$ under fixed pay (by a factor of $(c+bU'(6G))/c$). Thus even if wages are continuously distributed around the threshold level $6G$, both under fixed and performance pay absence levels drop at the threshold. However, performance pay acts as an enforcer of the discontinuity at $6G$, strengthening the drop in absence level at the discontinuity.

Finally, one should note that if one just compares the sick leave level differences between sick pay regimes, they should be most easily seen under fixed pay. Under fixed pay only the monitoring intensity varies between the sick pay regimes. Under performance pay the piece-rate on performance as well as the monitoring intensity might differ between the sick pay regimes. Let subscripts p_u and p_r denote public sick pay only and private supplementary sick pay, respectively. The differences in absence levels for workers earning above $6G$ under fixed pay can then be expressed as: $(1-a_{p_u})-(1-a_{p_r})=(1-m_{p_r})U(W)-(1-m_{p_u})U(S)-(m_{p_u}-m_{p_r})U(R)$. It is reasonable to assume that employers providing additional sick pay monitor more intensively than other employers even when monitoring is costly (to offset costly shirking). If this difference in monitoring intensities is not too large then $(1-a_{p_u})-(1-a_{p_r})>0$, i.e., the realised sick leave rate under private supplementary sick pay is higher than the sick leave rate under public sick pay only.

3. The Norwegian sick pay legislation

The public sick pay system in Norway is prototypical of a generous Scandinavian welfare state. Workers on fixed pay contracts are provided “complete” compensation for 1 year if their salaries are less than $6G$ (G =baseline level public social insurance system, increasing from 51 360 NOK (May 1, 2001), via 54 170 (May 1, 2002) and 56 861 (May 1, 2003) to 58 778 NOK in May 1, 2004). The employers are free to offer “top-up” compensation for workers earning more than $6G$. The employer-provision of top-up sick pay compensation is a practice that is not particular to Norway, but is seen in other welfare countries as well (Barmby et al., 2002).

No public statistics in Norway exist documenting the prevalence of privately provided sick pay. However, based on similar data as mine (see Section 4 for further details on data), Blekesaune and Dale-Olsen (2010) presented national and industry-specific representative figures for 2003 based on the population of workers employed in private sector workplaces with more than 11 employees. In Table 1, based on Blekesaune and Dale-Olsen (2010), we see that roughly 40% of the private sector workplaces offer such top-up in 2003. At the same time, the table

reveals quite large industry differences. While less than 20 per cent of the construction workplaces provide additional sick pay, in finance more than 50 per cent of the workplaces employing 60 per cent of the workers provide such additional pay.

[Table 1 around here]

6G is close to the average yearly salary for a full-time male manufacturing worker. In 2003 40 per cent of all private sector (excluded public administration, health care and education) workers earned more than 6G. In Table 1 we see that the average 2003 proportion of workers earning above the threshold level in private sector workplaces with more than 11 employees was 25 per cent. Once again industry differences are revealed, but less so than for the prevalence. Finance workplaces are really the ones that differ, where the proportion of workers in workplaces that earn above the earnings threshold for publicly provided sick pay (above 6G) is over 40 per cent. Finally, note that governmental employees receive complete compensation of fixed pay regardless of salary level.

While one uses the phrase “complete compensation”, it is important to note that this does not mean that all pay is compensated fully, i.e., the replacement ratio is not 100 per cent. First, for workers operating under a performance pay regime based on individual performance, bonuses will often not be compensated. If the absence is detrimental to team performance, then bonuses can be lost also under a regime with team incentive devices. Across all performance pay schemes Dale-Olsen (2012) reports that the average fraction of pay based on performance is slightly less than 10 per cent. Close to 50 per cent of Norwegian private sector workers operate in 2003 under a regime of performance pay (Barth et al., 2008), thus a substantial proportion of Norwegian workers cannot be said to experience complete compensation. Furthermore, when you are absent from work you lose out on opportunities at work providing other extra pay, for instance overtime payment, and sick leaves may also affect your future career opportunities

negatively and thereby be associated with a loss in earnings which is not compensated through the sick pay.⁵

Finally, each worker may be absent up to 4 spells during a 12 month period based on an own declaration of illness. All sick leaves lasting at least 4 days have to be physician-certified (in which case the first 3 days are registered in the sick leave spell) and for those workers experiencing more than 4 spells during the last 12 months, all sick leaves regardless of duration have to be physician-certified. Statistics Norway estimates that self-certified absences constitute one fifth of the physician-certified absence rates in 2008. As is described in the data section, my data comprise information on private-sector workers 20-60 years of age, but only physician-certified absences are registered. Thus we cannot differentiate between workers with no sick leaves and those with self-declared sick leaves.

4. The empirical strategy

The main empirical strategy is based on the regression-discontinuity approach (RD). In a RD design one exploits an observable discontinuity in the level of treatment related to an assignment variable, i.e., the level of treatment jumps discontinuously at some specific value or threshold. This value is often called the cut-off. In my case, under public sick pay only, the treatment variable – sick pay providing complete compensation – rises continuously with earnings, until it reaches the earnings level 6G – the cut-off or threshold – and then stay fixed at 6G. Thus albeit we do not observe directly a discontinuous drop in sick pay, complete compensation as the treatment variable experiences a drop from one to zero at 6G. In the neighbourhood of 6G complete compensation can be treated as randomly assigned. I do not argue that individuals are unable to manipulate earnings, however, a worker is seldom completely able to individually determine his or her earnings. For example, in wage bargaining economies, central or local wage

⁵ In a recent paper, Markussen (2012) finds that increasing the absence leave by 1 percentage point implies an earnings loss of approximately 405 \$, indicating that pay is not compensated fully.

bargaining is usually affecting groups of workers. Under certain performance pay schemes colleagues' behaviour may influence your reward. When employers due to demand reasons require workers to provide extra work hours, this may be associated with compensation. The key assumption of a RD-design is that individuals have imprecise control over the assignment variable (Lee and Lemieux, 2009), i.e., earnings in my case. As pointed out by McCray (2008) partial manipulation is typically not associated with identification problems.

In the RD-design one estimates the size of a discontinuous jump in variables by comparing the means of small bins on both sides of the cutoff, or by estimating regressions comprising various polynomials around the cut-off, a dummy for being above the cut-off and interactions. In this paper I have decided to primarily use non-parametric estimation, based on local linear regressions.⁶ Thus I have to resolve the issues related to the choices of bandwidth and kernels. Nichols (2007a) points out that the triangle kernel is boundary optimal (as shown by Cheng et al., 1997), thus having good properties in a RD-context. Bandwidths are determined following Imbens and Kalyanaraman (2009) where the default bandwidth is designed to minimize MSE, or squared bias plus variance, in a sharp RD design. To test the sensitivity of my results to the choice of bandwidth, I follow the suggestions from Lee and Lemieux (2009) and Nichols (2007a) and repeat the analyses using bandwidths of half the size and twice the size of the default bandwidth.

In supplementary analyses I estimate Poisson and IV-Poisson regressions of the number of sick leave days (which take into account the discrete or count nature of sick days) for workers

⁶ Nichols (2007a) advocates the use of local linear regression, since this arguably minimizes bias (Fan and Gijbels, 1996), but Lee and Lemieux (2009) emphasize that non-parametric and parametric estimations are complements. Although the number of sick leave days constitutes a discrete measure, which implies count regressions (for example, Poisson), the normal distribution is suited as an approximation to the Poisson distribution when the number of successes, i.e., number of sick leave days, is large (more than 10)(the sick leave days extends to maximum 366 in 2004 with an average of 15 days).

with earnings above the threshold differ depending on employers providing supplementary sick pay. The motivation for estimating these IV-regressions is two-fold. First, the utility differences arising at the discontinuity might be too small to yield identifiable behavioural changes in the RD-analysis, but for these workers, receiving public sick pay only implies an income loss of 20 per cent, and this should clearly be large enough to affect behaviour.

Second, in the discussion in Section 1 we pointed out that sick pay regime was associated with selection and sorting of both firms and workers. For example, as is seen in the theoretical discussion in Section 2, employers' monitoring intensity affects the sick leave rates. Since this intensity is unobserved, this might bias the results. Thus, depending on the cost of monitoring, employers providing additional private sick pay might monitor more intensively to offset costly shirking, thus keeping the sick leave rates down. A simple comparison might then not identify any differences in the sick leave pattern between workers employed under the two sick pay regimes. Similarly, it is reasonable to assume that workers sort to workplaces depending on their health and the employers' sick pay regimes. Unfortunately, a worker's health is unobserved. Thus, if less healthy workers sort to workplaces providing full compensation, while more healthy workers sort to workplaces which do not provide additional private sick pay, then this sorting aggravates the differences in the sick leave pattern between workers employed under the two sick pay regimes, i.e., it bias the results upward indicating that providing full compensation will cause workers to return to work later than what is actually true.

In both these cases, our estimates will be biased due to unobserved variables. These unobserved variables can be interpreted as measurement errors associated with the dummy for additional private sick pay. One way of eliminating this bias, is to exploit variation in variables not related to these measurement errors, but correlated to the provision of supplementary sick pay, i.e., to apply an IV-approach. An IV-Poisson regression model based on the IV/GMM-framework has been developed by Nichols (2007b).

As is pointed out in the data section, the questionnaire data provide information on employers' attitudes towards and thoughts on non-wage benefits. The responses of the employers were limited to 4 alternatives: i) Non-wage benefits are important to retain and to recruit workers, ii) Workers prefer non-wage benefits to money wages, iii) Employers save payroll tax, and iv) Employers achieve lower prices than workers. Additional private sick pay can be considered as a non-wage benefit. It is reasonable to expect that the 3 first responses are positively correlated with the provision of supplementary private sick pay. The relationship between the last response and the provision of private sick pay should be negative or zero, since the employer cannot expect to achieve lower price than workers regarding sick pay, i.e., the response is primarily related to other non-wage benefits. On the other hand, it is difficult to see how these attitudes should reflect monitoring intensities or the health of workers.

5. The data

The *first* data set comprises a questionnaire, the Norwegian Workplace and Employment Relationship Survey 2003 (NWERS2003), answered winter/spring 2003 by the daily manager or personnel manager of roughly 2300 Norwegian workplaces from public and private sectors. These establishments are sampled from the population of workplaces with more than 10 employees (roughly 35000 workplaces). The NWERS- workplaces employ over 350 000 workers, i.e., nearly a fifth of the Norwegian workforce. The sampling procedure and the questionnaires (NWERS2003 only) are described in Holth (2003). The questionnaires cover topics such as compensation, employer motivation for benefit strategies, work practices and organisation issues, and are similar to questionnaires found in many countries (for example, WERS in the United Kingdom).

The *second* data set, or more precisely, data system, is based on public administrative register data comprising *all* firms, workplaces and employees (incl. executives) in Norway 2003–2004 (roughly 140000 firms, 180000 workplaces and 2000000 employees each year) employed May 15th each year. This linked employer-employee data set provides information on workers (sickness

absence spells and publicly sick pay spells, diagnoses, gender, educational qualifications), and jobs (seniority, spell-specific earnings and fringe benefits, working hours, hourly wage, and firm-and establishment identifying numbers, industry (5-digit NACE), sector and municipality).

Each individual, each establishment and each firm are identified by unique identifying codes (separate number series) (in both questionnaires and register data). Since we are to employ a RD-approach we also discard all observations of workers earning less than 3G (G=baseline level public social insurance system, increasing from 54 170 (May 1, 2002), via 56 861 (May 1, 2003) to 58 778 NOK in May 1, 2004), and who is employed the whole year. In practice this discards observations of part-time workers. This is to our benefit, since our discontinuity is at 6G, and the group of workers earning around this threshold work full-time as well. We focus on workers between 20 and 60 years of age to avoid measurement problems associated and retirement decisions.⁷

When we link the questionnaires and the register data, and focus on the private sector questionnaire workplaces and their employees, this yields roughly 1512 and 1462 private sector workplaces and their male and female employees in 2003 and 2004, respectively. Thus the linking yields 2 different samples of workers, one for each year, but since sick pay schemes and pay schemes were established winter/spring 2003 (when the survey interviews were conducted), however, worker behaviour should really be studied after this to avoid any influence of reversed causality, i.e., analyses should be conducted later in that year or for the next year. We argue that 2004 is preferable, since we then know if the earnings and sick leaves the previous year were accrued under fixed or performance pay. We refrain from pooling the data, since panel data do not yield any considerable gain in a RD-approach.

⁷ Public retirement age in Norway is 67 years. This restriction regarding worker age discards observations workers between 16 and 20 years of age and those between 60 and 67 years of age. This constitutes only roughly 5 per cent of the workers, so the age range cannot be defined as narrow.

Let us return briefly to the key measures. The number of sickness leave days is measured as the number of physician-certified sick leave days during a year (aggregated over all spells within a year), thus it is not related to how the sick pay is financed. Thus our sick leave measure comprises short and long-term sick leaves (in most cases, at least 4 days). However, data do not comprise self-declared sick leaves. Although self-declared sick leaves seem more strongly related to shirking behaviour, physician-certified sick leaves will also contain such, since as in the other Scandinavian countries, Norwegian physicians seldom deny sickness certificates (Wahlström and Alexanderson, 2004; Carlsen and Nyborg, 2009), thus leaving considerable scope for worker discretion regarding sick leaves. In the analyses of female workers, we discard observations related to sick leaves related to birth.

Our earnings measure is based on total earnings reported to the tax authorities, defined as W . This measure comprises all wages, sick pay and taxable fringe benefits (such as a company car, stocks and stock options) received in a specific job during the year. We transform the earnings measure so it is centralised around G , i.e., $\ln E = \ln(W) - \ln(G)$.

From the questionnaire, the questions regarding employer provided sick pay, worker pay compensation and determination are particularly relevant. The questionnaire provides information on whether workers' wages are performance related (explicitly differentiating between surplus or profit sharing regime, individual- and group piece-rate, commissions, group bonuses, individual bonuses, stocks or stock options), or if they are fixed. While the performance pay schemes are related, they also differ along important characteristics. For example, commissions often imply that pay is solely performance related, while the bonus schemes are always combined with a fixed pay element. The piece-rate schemes usually make pay more sensitive to performance than the bonus schemes.

We end this section with a brief overview of the data in the form of descriptive statistics. In Table 2 we present averages (and standard deviations) separately for men and women based on the 2004-sample of workers employed under public sick pay only and public and supplementary

private sick pay combined, respectively. These averages are not representative for the Norwegian worker population in general, since public sectors, small workplaces (less than 11 employees), low-wage part-time workers and workers whose job spell does not cover the whole year are excluded from the data.

In the data workers employed under public sick pay only experience slightly longer sick leaves, they receive slightly less pay and have educations of shorter durations compared to workers employed under public and private sick pay combined. When it comes to other personal characteristics such as seniority, experience, and age the difference is negligible.

[Table 2 around here]

However, we see clear differences when it comes to pay regimes, revealing as indicated by the discussion in Section 1, that pay and sick pay regimes are related. Workers employed under public sick pay receive fixed pay more often than the other group (35.7 per cent vs. 21.9 per cent). We also see that piece-rate is more common among workers receiving public sick pay only, while individual bonuses as well as stocks are more common among those receiving both public and private sick pay.

In Table A2 we present descriptive statistics on the sample arising when discarding observations considered to outside a chosen bandwidth around the threshold and the discontinuity. In the table the chosen bandwidth is the default bandwidth reported in Models 1 and 4 in Table 3. From a RD-perspective predetermined characteristics should not exhibit a discontinuity at the cut-off of 6G. Thus we would be worried if the descriptive statistics just below or just above the cutoff differed drastically. Table 2 shows that for most variables these are quite similar. The exceptions are of course, related to the number of absence days and earnings, but since they express outcome and assignment variables they are not important in this respect. However, we also see that pay schemes differs when comparing workers just below and just above the cut off. More workers are employed under fixed pay contracts just below the cut off than just

above the cut off, while the opposite is true for performance pay. Group bonuses, stocks and piece-rates are more common just above the threshold than just below.

6. Empirical results

6.1 The RD-analysis

In this sub-section we then turn to our main question: the relationship between the number of absence days and sick pay around the earnings threshold of 6G. Figures 1-2 and Table 3 present the main results from the RD-analysis. For male workers Models 1 – 3 report the results for workplaces where employees only receive public sick pay, while models 4 – 6 report the results for workplaces where employers provide sick pay in addition to the publicly provided. The results from separate analyses for performance pay workplaces only are reported in models 2 and 5, while the similar results for fixed pay workplaces are reported in models 3 and 6. Models 7-12 report the similar results for women.

[Table 3 around here]

The results for Table 3 are striking. When only public sick is provided, we observe for men a drop of 2.9 days at the discontinuity, but this is really related to performance pay workplaces. No significant RD-effect for men is found for fixed pay workplaces, but for performance pay workplaces the number of sick days drops by 5 days at the discontinuity. When supplementary private sick is provided, the estimates for men are clearly weaker and not statistically significant. These latter results are as expected since the provision of additional private sick pay disrupts the discontinuity at 6G, i.e., we should not expect to observe strong RD-effects for these workplaces and their employees. For women, we observe no significant RD-effect, regardless of pay scheme or sick pay regime.

These baseline estimates are based on default bandwidths calculated using the method of Imbens and Kalyanaraman (2009). To test how robust our results are to the choices of bandwidth we repeat the analyses using bandwidth half or twice the size of the default

bandwidth. We see that for performance pay workplaces under public sick pay the RD-effect is still significant and only drops slightly. In these cases the number of sick days drops by 4 days at the discontinuity. In no other cases we find significant results.

For an RD-design to be valid certain assumptions have to be satisfied. First, we clearly would prefer that no discontinuities of the assignment variable existed around the cut-off. To shed light on this, we have studied kernel densities of the assignment variable around the cut-off for our different populations. These figures show that, while clearly most of the densities deviate from the picture of a smooth normal density, they do not reveal strong evidence for discontinuities around the cut-off. Furthermore, to formalise this somewhat, I have followed the methods of McCrary (2008) and tested for each model in Table 3 whether we can reject the notion of continuity in the density at the cut-off (based on 100 simulations and two different bandwidth sizes). In no case we can reject this (average t-values in each model ranging from -1.0 to 1.1).

It is the discontinuity of complete compensation at 6G, dropping from 1 to 0, that makes our identification possible. This identification is ensured regardless of how the distributions of earnings and sick pay are elsewhere. However, it will be reassuring if we do not discover discontinuous treatment effects elsewhere, since few should really be found (statistically one cannot exclude the possibility of randomly identifying an erroneous cut-off). To test this, I have randomly assigned 100 placebo cut-off points (located elsewhere than 6G), and repeated my analysis. For each model in Table 3 I identify 8-10 erroneous cut-offs. The average estimates of these pseudo-RD effects are also presented in Table 3. As we see, no estimates are significantly different from zero, and in most cases the point estimates are also small. Thus I find few evidences of discontinuous treatment effects elsewhere.

Next, it is desirable that a test of the measured jump in each predetermined potential controls is zero at the cut-off. Otherwise one could worry about non-random sorting around the cut-off. Thus I have estimated the treatment effect of the discontinuity at 6G for a wide range of

control variables (see Table A2), where the tests are based on default bandwidths calculated using the method of Imbens and Kalyanaraman (2009). In most cases these estimates are not significant, and with few exceptions, a test of joint significance based on SUR regressions (not shown)(as suggested by Lee and Lemieux, 2009) does not reveal significant sorting.⁸ Thus we find little support of the notion that our results follow from non-random sorting around the cut-off.

Finally, the reported analyses are based on labour market experiences in 2004. Is 2004 different than 2003? To address this question and as robustness checks I have repeated these analyses for 2003 and thus a different population, and even for different absence measures (focussing on absences occurring after May 1st). The results from these analyses (not reported) are that, albeit being not as strong, they do not qualitatively differ from the 2004 results. We have also repeated the analyses excluding long term absences (more than 30 per cent lost work days)(not reported), and while the overall effects become somewhat weaker, the results are qualitatively unchanged.

The first temporary conclusion we can infer is that for male workers employed under performance pay the sick leave days drop by 4-5 days when sick pay is no longer completely compensated. On average, the male workers experience 15 days of sick leave, thus the reduction is considerable. On the other hand, for female workers I found that when sick pay is no longer completely compensated, this did not significantly affect these workers' sick leaves days.

Why then do male workers employed under performance pay reduce their number of sick days when sick pay is no longer fully compensated? To answer this question I repeated the analyses for male workers only, but differentiate between different forms of performance pay. We differentiate between piece rate, group bonus, individual bonus, surplus sharing, commission,

⁸ The exceptions are found related to surplus sharing workplaces providing public sick pay only for males and years of education for male workers receiving public sick pay only. Excluding the former kinds of workplaces from the analyses does not qualitatively change our conclusions. The educational effect is solely related to 200 workers being educated at Norwegian Folk High Schools. Excluding these workers does not alter the conclusions either.

stocks, and stock options. Table 4 presents the results from these analyses for men only. Column headings denote the different performance pay schemes.

[Table 4 around here]

Once again we see a striking pattern. Most of the performance pay schemes are associated with drops of 1-2 sick days when sick leave is no longer fully compensated, but these drops are not significant. The exceptions are piece rate pay and commission pay, where the number of sick days drop 7.6 – 8.5 days when sick leave is no longer fully compensated. Piece rate workers are on average absent due to sickness around 18 days, i.e., once again the reduction in sick days is considerable. For commission pay the estimate is not significant, probably due to a limited number of observations.

The implication of these results is that when we observe that male workers under performance pay react to cuts in the public sick pay by reducing their number of sickness absence days, these reductions are primarily born by piece rate and commission workers.⁹ These pay schemes clearly are among those that highlight production related performance targets. For some of the pay schemes analysed in Table 4 the level of pay is highly dependent on performance. On average across male workers and workplaces, the performance element constitutes 30 and 45 per cent of total pay among commission pay workers receiving public sick pay only and additional private sick pay, respectively. However, the similar figures for piece-rates are 17 and 10 per cent, which thus group them among the pay schemes least sensitive to performance. Thus the results indicate that they reflect not just the potential income loss, but also reflect how this loss is perceived.

A second temporary conclusion is that for workers employed under fixed pay contracts no reduction occurs when sick pay is no longer completely compensated. One interpretation of this latter result implies that at these earnings levels there is no causal impact of the sick pay on

⁹ The analyses have also been conducted for women. Partly due to the limited number of observations, no significant relationship between the compensation level and sick leave days is found for any of the performance pay schemes.

the number of sick leave days. Thus the potential public savings from cutting the sick pay is for these workers limited to the reduction in the direct sick pay expenditures per sick day, and not by behavioural changes causing additional saving due to a reduction in the number of sick days. In other words, reduced sick pay compensation rates do not speed up individual recovery for these workers.

Another interpretation of these results is that, except for male performance pay workers receiving public sick pay only, the utility difference at the threshold is not large enough to induce differences in the sick leave levels. To differentiate between these two explanations, will the topic of the next section.

6.2 No behavioural changes or too small utility differences at the threshold?

In this section I study closer the sick leave patterns of workers earning above the threshold (6G), i.e., those workers whom will not be fully compensated by the public sick pay. For male and female worker receiving no other compensation than the public sick pay, sick leaves may potentially imply a severe loss of income. For male workers, we observe on average a compensation rate of 78 and 81 per cent for those employed under performance pay and fixed pay, respectively. However, under both pay regimes the compensation rate varies between 10 and 99 per cent. For women the compensation rate is on average 84 per cent regardless of pay regime, and it varies between 25 and 99 per cent.

If the employers had not provided additional sick pay, the similar figures for workers receiving private sick pay would have been around 70 and 78 per cent for men and women, respectively. So the income levels are clearly not particularly lower in workplaces providing additional private sick pay compared to the other workplaces.

Thus by comparing the sick leave behaviour of workers employed under the two sick pay regimes, conditional on pay regime, we should observe clear differences if sick pay compensation matters. To test this notion I therefore estimate Poisson- and IV-Poisson regressions of the

number of sick leave days on a dummy for additional private sick pay and other controls. Considering the differences in compensation rates, we clearly expect such regressions to reveal that providing additional private sick pay implies that workers extend their sick leave days. The IV-Poisson regressions will take care of biases arising due to unobserved monitoring and the sorting of workers to different sick pay regimes depending on their health.

In Table 5 we see the results from the Poisson- and IV-Poisson regressions of the number of sick leave days on the dummy for additional private sick pay and a control vector. The control vector for male workers comprises $\ln(W)-\ln(G)$, union member (dummy)¹⁰, years of education (and squared), years of experience (and squared), years of seniority (and squared), occupation (9 dummies), workforce size (and log), workplace age (and log), and industry (7 dummies). The control vector for female workers is similar to that of men's, but all workplace-level variables are dropped due to the limited number of observations of female full-time workers.

Table 5 reports the estimated coefficient (and standard error) from the Poisson- or IV-Poisson regression. It also reports the estimated average marginal effect associated with providing additional private sick pay, and in the IV-Poisson regressions, Table 5 reports information on the instruments.

[Table 5 around here]

In the IV-Poisson regressions, the dummy for additional private sick pay is instrumented by dummies expressing the employers' attitudes towards non-wage benefits. As is seen in Table 5, these instruments are strong (as indicated by the LR-ratio test) and they mostly have the expected signs (as indicated by the estimated parameters from separate Logit-regressions). Thus I am quite confident that these instruments work as intended.

¹⁰ It is important to control for union membership, since several studies have shown that union workers are more absent-prone than non-union workers (Goerke and Pannenberg, 2012; Mastekaasa, 2013)

The results reported in Table 5 are once again striking. The simple Poisson regressions reveals that, except for fixed pay women, additional private sick pay is correlated with longer sick leaves. For men, when employers pay additional private sick pay workers' sick leave days increase by 1.4-2.3 days. This increase, which when one takes into account that the average predicted number of sick leave days for these workers are 7-8 days when they do not receive private sick pay, has to be considered substantial. For women employed under performance pay, we see that private sick pay implies that sick leaves are extended by 4 days, which relative to the predicted sick leave days of 15 days when they do not receive public sick pay, once again indicates a substantial increase in sick leave days is associated with paying additional sick leave days.

However, when we in the IV-Poisson regressions take into account sorting and unobserved monitoring intensity, both for men and women employed under fixed pay any significant causal impact on the number of sick leave days from providing additional private sick pay disappears (the point estimates even become negative). Thus any positive causal impact from paying additional private sick pay is limited to performance pay workers and workplaces, but here among the performance pay workplaces the effect is indeed considerable. For men the impact of providing additional private sick pay is even significantly different between the performance pay workers and the fixed pay workers. The limited number of observations of women (and thus the limited variation) makes the IV-results for women less robust, but the tendency is similar to what we observed for men.

The results presented in this section thus provide a highly consistent picture compared to the results in Sub-section 6.1. One does observe a causal response following sick pay variations among male workers employed under performance pay, but not under fixed pay. Thus neither the analyses in Sub-section 6.1 nor the analyses in this sub-section reveal that behavioural sick leave responses occur for male fixed pay workers, even (as shown in this section) they face an average income loss of 20 per cent.

6. Conclusion and Discussion

In economies worrying about absenteeism and public sick pay expenditures one suggested solution has been to cut public sick pay. The arguments for such a reform is that not only would this reduce the direct sick pay expenditures per sick day, but it would also cause behavioural changes, thus inducing additional savings by reducing the total number of sick days as well. In this paper I have shown that this, at least for medium and high paid workers, is not necessarily true. By exploiting a discontinuity in the Norwegian sick pay legislation at 6G I find that only the sick leave days of male workers under performance pay drop, when pay is not fully compensated when absent. The drop is identified for all performance pay male workers, but is particularly strong for piece-rate and commission workers, cut back their sick leave days significantly when sick leave is no longer fully compensated.

This begs the question why? My model shows that performance pay workers compared to fixed pay workers experience higher utility differentials at the discontinuity thus inducing bigger sick leave differences. We can also reject the possibility that what we are seeing here is that illnesses make piece-rate workers experience an additional earnings loss as well, and we can even exclude the possibility that this is related to injuries and compensating wage differentials.¹¹ The reasons why we can exclude these explanations is that my results followed from *within-pay scheme* analyses, and the analysis of pseudo-cut-offs did not reveal *any* other significant cut off, which we would have expected if the two previous explanations had been correct. Thus this is explicitly related to different male worker behaviour around the cut-off at 6G.

For performance pay workers I find no regression discontinuity effects for workers receiving additional sick pay compensation. This is clearly as expected. When it comes to performance pay women, no significant behavioural change is observed at the cut-off, regardless

¹¹ Piece-rate workers are more productive and receive higher earnings than fixed pay earnings (Brown, 1992; Lazear, 2000; Haley, 2003; Pekkarinen and Riddell, 2008), but not necessarily more profitable (Freeman and Kleiner, 2005). Piece-rates might also incorporate compensating wage differentials for increased injury risks (Bender et al., 2012).

of sick pay regime. This is slightly surprising, but studies exist showing that women respond less to economic incentives, to competitive environments and choose less risky contracts (Gneezy et al., 2003; Croson and Gneezy, 2009; Dohmen and Falk, 2011).¹² However, when applying IV-Poisson regressions I find that the sick leaves of performance pay workers non-eligible for public sick pay, but receiving additional private sick pay, are considerably longer than the sick leaves of performance pay workers not being eligible for additional private sick pay.

Since it is hard to argue that performance pay workers under the two regimes are very different in how concerned or aware about how pay and effort are related, this supports the notion that performance pay workers adapt their sick leaves to the sick pay compensation level. For the fixed pay workers, I find in the RD-analyses and in the IV-Poisson analyses that, both for men and women, sick leaves are not affected by pay no longer being fully compensated, i.e., a considerable number of workers do not adapt their sick leaves to the sick pay compensation level.¹³ In other words, the propensity for these workers to enter sick leave is not very sensitive to the replacement rate.

The two important lessons to be learned from my study this is firstly that firms, for recruitment and to retain workers, provide additional benefits to workers, and these benefits may limit the potential effects of a sick pay legislation reform. Thus a cut in the public sick pay might be offset by employers providing additional sick pay, and this will reduce the behavioural responses. Secondly, my study raise doubt that the behavioural changes following a cut in the sick pay conditional on sick pay scheme will be as significant as one could desire. Some workers' sick leave behaviour react strongly to economic incentives (as is seen in Johansson and Palme, 2002; Ziebarth and Karlsson, 2010; Dale-Olsen, 2013), while other do not. Thus my results support

¹² For example, paying people for giving blood, does not affect men's intrinsic motivation for giving blood, but crowds out women's intrinsic motivation (Mellström and Johannesson, 2008).

¹³ As documented by Barth et al. (2008) fixed pay is quite common in Norway (close to 50 per cent of all private sector employers with more than 10 employees provide fixed pay in 2003).

Ziebarth (2013) in identifying heterogeneous replacement rate effects on sick leaves. In my study the major difference is related to being employed by performance pay or by fixed pay workplaces, wherein the former sick leaves are highly sensitive to the replacement rate while in the latter, no response is found. Dohmen and Falk (2011) found that more productive and less risk averse workers sorted into variable pay schemes compared to those sorting into fixed pay schemes. Even personality mattered for this sorting difference, and some performance pay schemes were associated with less reciprocal workers. Thus one interpretation of my result could be that performance pay workers are more concerned or aware about pay and how their effort influences pay, and thus explicitly adjust their behaviour when facing different replacement rates.

For policy makers my results imply that public sick pay cuts might not induce the behavioural responses desired if policy makers are interested in speeding up individual recovery in addition to reducing directly the expenditures on public sick pay. My results are contingent on pay regime, but sick pay reforms by public policy makers will most likely not involve private sector pay setting regimes. When some workers' propensities for taking sick leave days are not affected by sick pay, and employers additionally introduce supplementary sick pay offsetting caps and limitations in the public sick pay schemes, this generally impairs the public sick-pay replacement rate effectiveness as a possible absence-reducing device.

Finally, cutting public sick pay expenditures by reducing the replacement rate affects the income of low-paid workers relatively more strongly than highly paid workers, since the latter group is more likely to receive additional private sick pay. Low pay, poverty and poor health are strongly linked (CSDH, 2008). Keeping the replacement rate high would thus ensure valuable recuperation possibilities for impoverished workers with bad health, and contribute positively to their health. On the other hand, while it is not obvious whether workers' inherent propensities to shirk are related to their positions in the income distribution, i.e., whether moral hazard varies with income, most classical efficiency wage models imply a negative relationship between the propensity for shirking and the income level (Shapiro and Stiglitz, 1984; Barmby et al., 1994).

Therefore the problem of moral hazard might be greater for low wage workers than average or high wage workers. My study, however, cannot be used to draw inferences on the behaviour of low wage workers, thus potentially this still leaves scope for additional cost savings following cuts in the sick pay, through altered sick leave behaviour for these workers. This will, however, be a topic for future research.

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Table 1 Proportion of workplaces and workers 2003 employed by employers providing higher replacement rate during sickness absence than what is provided by the public sick pay legislation. In percent. Population of private sector workplaces with at least 10 employees.

	All	Manufacturing	Construction	Trade	Transport and communication	Business and finance
Workplaces						
All	38.8	34.8	16.5	38.5	38.6	52.2
	(26.5)	(5.6)	(2.1)	(7.1)	(2.3)	(3.5)
	[24.7]	[27.7]	[29.5]	[19.8]	[25.9]	[41.1]
Workers						
All	53.1	59.5	30.5	45.4	49.8	63.9
	(984)	(329)	(62)	(172)	(92)	(147)

Note: Number of workplaces and workers in 1000 are presented in parentheses. Proportion of workers in workplaces that earn above the earnings threshold for publicly provided sick pay (above 6G) is denoted in brackets (in percent) (G=National Social Service baseline figure, which is adjusted May 1st each year. In 2004 it equaled on average 58139 NOK or 4711£/8625\$). See also Blekesaune and Dale-Olsen (2010: Table 12.1).

Table 2 Descriptive statistics

	Men			Women		
	All	Public sick pay only	Additional private sick pay	All	Public sick pay only	Additional private sick pay
Private sick pay	0.75 (0.43)	-	-	0.76 (0.43)	-	-
Sick leave days	13.15 (39.75)	16.22 (42.85)	12.07 (38.44)	21.59 (53.63)	23.21 (52.76)	21.05 (53.83)
ln(W)-ln(G6)	0.22 (0.37)	0.03 (0.29)	0.28 (0.38)	-0.01 (0.32)	-0.16 (0.24)	0.06 (0.34)
Fixed pay	0.25 (0.43)	0.36 (0.48)	0.22 (0.42)	0.21 (0.41)	0.32 (0.47)	0.20 (0.40)
Piece-rate	0.09 (0.29)	0.18 (0.38)	0.07 (0.25)	0.06 (0.24)	0.13 (0.34)	0.04 (0.20)
Individual bonus	0.21 (0.41)	0.13 (0.34)	0.23 (0.42)	0.24 (0.43)	0.13 (0.34)	0.28 (0.45)
Group bonus	0.31 (0.46)	0.29 (0.45)	0.33 (0.47)	0.28 (0.45)	0.23 (0.42)	0.32 (0.47)
Surplus sharing	0.12 (0.32)	0.10 (0.30)	0.13 (0.33)	0.12 (0.33)	0.13 (0.34)	0.13 (0.34)
Commission	0.04 (0.21)	0.04 (0.20)	0.05 (0.21)	0.07 (0.25)	0.09 (0.29)	0.07 (0.25)
Stocks	0.36 (0.48)	0.23 (0.42)	0.41 (0.49)	0.31 (0.46)	0.18 (0.38)	0.38 (0.48)
Stock options	0.10 (0.30)	0.10 (0.30)	0.10 (0.30)	0.10 (0.30)	0.10 (0.30)	0.10 (0.30)
Average bonus (%)	5.98 (13.76)	6.75 (14.82)	5.86 (13.58)	6.93 (16.55)	7.26 (16.23)	7.26 (17.25)
Age	43.44 (9.85)	42.49 (10.08)	43.63 (9.73)	43.66 (9.40)	43.47 (9.80)	43.35 (9.26)
Education (years)	3.59 (2.57)	2.67 (2.13)	3.82 (2.61)	3.58 (2.77)	2.64 (2.42)	3.73 (2.77)
Experience (years)	23.89 (10.27)	23.82 (10.46)	23.8 (10.23)	24.07 (10.36)	24.84 (10.59)	23.62 (10.38)
Seniority(years)	9.94 (7.45)	9.76 (7.31)	9.98 (7.50)	9.20 (6.85)	9.29 (7.07)	9.11 (6.80)
Union member	0.74 (0.44)	0.74 (0.44)	0.74 (0.43)	0.71 (0.45)	0.71 (0.45)	0.70 (0.46)
Workplace age	27.00	28.04 (12.93)	26.47 (12.83)	27.91 (12.12)	27.79 (12.83)	27.68 (12.25)
Workforce size	2309(10079)	363.1 (314.5)	3041 (11729)	1868 (8265)	342.5 (329.9)	2516.2 (9754)
N	88358	21656	64114	28299	6432	20013

Note: Population: Workers 2004 not absent 2003 employed by workplaces with more than 10 employees earning more than 3G (G=National Social Service baseline figure, which is adjusted May 1st each year. In 2004 it equaled on average 58139 NOK or 4711£/8625\$). Public sick pay is limited to 6G. All workers earning less than 6G are eligible for public sick pay. Column heading denotes workplace and worker populations. Average bonus (%) expresses how large proportion bonuses typically constitute of total earnings. Years of education measures the years of education in excess of compulsory schooling.

Table 3 Sickness absence days, sick pay and pay regimes.

	Men						Women					
	Public sick pay only			Public and private sick pay			Public sick pay only			Public and private sick pay		
	All	PP	FP	All	PP	FP	All	PP	FP	All	PP	FP
Model:	1	2	3	4	5	6	7	8	9	10	11	12
RD-effect	-2.876*	-5.171**	0.462	-1.408	-2.000	0.117	2.758	0.388	-1.188	0.992	0.758	0.910
	(1.321)	(1.488)	(2.197)	(1.097)	(1.282)	(1.797)	(3.357)	(3.302)	(5.147)	(1.749)	(2.143)	(3.970)
Bandwidth	0.203	0.296	0.154	0.219	0.240	0.237	0.264	0.421	0.357	0.264	0.298	0.336
Robustness checks												
RD-50	-2.488	-4.202*	0.268	-1.034	-0.973	-1.221	0.221	3.068	-0.530	0.221	-0.209	-1.263
	(1.792)	(2.017)	(3.038)	(1.538)	(1.823)	(2.351)	(4.653)	(4.548)	(6.742)	(4.653)	(3.065)	(5.191)
RD-200	-2.942**	-3.815**	0.056	-2.071	-3.251	0.004	-0.462	1.564	-2.314	-0.462	1.035	1.655
	(1.010)	(1.194)	(1.661)	(0.819)	(0.961)	(1.391)	(2.556)	(2.733)	(4.191)	(2.556)	(1.635)	(3.246)
RD- <i>pseudo</i>	0.612	0.521	0.662	0.070	0.288	-0.253	-0.347	-0.560	0.649	0.347	0.509	-0.015
	(2.030)	(2.634)	(3.025)	(1.447)	(1.668)	(2.175)	(4.079)	(4.692)	(7.043)	(2.163)	(2.450)	(5.138)
N	12987	10958	3703	29447	23191	8472	3807	2454	1184	13570	10068	4055

Note: Population: Workers 2004 not absent 2003 employed by workplaces with more than 10 employees earning more than 3G (G=National Social Service baseline figure, which is adjusted May 1st each year. In 2004 it equaled on average 58139 NOK or 4711£/8625\$). N expresses the number of observations (workers) used in the main analyses (default bandwidth). Public sick pay is limited to 6G. Column heading denotes workplace and worker populations. PP and FP denote performance pay and fixed pay, respectively. The default bandwidth from Imbens and Kalyanaraman (2009) is designed to minimize MSE, or squared bias plus variance, in a sharp RD design. -50 and -200 denote half and double the default bandwidth, respectively. The row headed RD-*pseudo* reports the average estimates from RD-analyses based on 100 randomly assigned placebo cut-off points. Standard errors are presented in parentheses. ** and * denote 1 and 5 per cent level of significance, respectively.

Table 4 Male sick leave days, sick pay and performance pay systems under public sick pay only.

	Piece rate	Individual bonus	Group bonus	Surplus sharing	Commission pay	Stocks	Stock options
RD-effect	-8.475* (3.690)	-2.714 (4.046)	-2.101 (2.398)	-1.857 (3.602)	-7.623 (6.084)	-2.937 (3.697)	-1.857 (3.602)
Bandwidth	0.210	0.248	0.202	0.253	0.329	0.149	0.253
RD-50	-6.790 (4.593)	-1.297 (4.872)	-0.959 (3.271)	-1.580 (4.655)	-0.996 (8.781)	-2.167 (5.545)	-1.580 (4.656)
RD-200	-7.140* (2.893)	-0.653 (3.152)	-2.795 (1.855)	1.722 (2.756)	-5.111 (4.813)	-6.743 (2.696)	-1.722 (2.757)
N	2104	4023	1749	1521	678	2267	1650

Note: Population: Male workers 2004 not absent 2003 employed by workplaces providing performance pay with more than 10 employees earning more than 3G. Column heading denotes workplace populations. N expresses the number of workers contingent on bandwidth. See also note to Table 3. Standard errors are presented in parentheses.** and * denote 1 and 5 per cent level of significance, respectively.

Table 5 The relationship between sick leaves and different sick pay regimes in 2004 for workers earning more than the income threshold for public sick pay.

	Men				Women			
	Poisson		IV-Poisson		Poisson		IV-Poisson	
	Fixed pay	Perf. Pay	Fixed pay	Perf. Pay	Fixed pay	Perf. Pay	Fixed pay	Perf. Pay
Private SP	0.279** (0.083)	0.163* (0.077)	-0.104 (0.251)	0.645* (0.274)	0.161 (0.179)	0.284* (0.112)	-0.038 (0.701)	1.390** (0.373)
Marg.effect	2.345** (0.699)	1.371** (0.595)	-0.994 (2.091)	4.909* (1.351)	2.732 (2.877)	3.934** (1.429)	-0.708 (11.530)	15.161** (2.648)
Additional controls: ln(W)-ln(G6), union member (dummy), years of education (and squared), years of experience (and squared), years of seniority (and squared), occupation (9 dummies), workforce size (and log), workplace age (and log), and industry (7 dummies)(note workplace variables only for men)								
On instruments								
Recruit/retain			1.302**	0.603**			1.303**	1.342**
Preference			1.131**	0.123**			1.454**	0.609**
Save pay-roll			-0.845**	1.798**			-0.787	0.123**
Price			-0.383**	-0.233**			-0.024	-0.362**
LR-test CHI:			1429.1**	590.9**			169.1**	383.7**
N	12125	46935	12125	46935	1560	9876	1560	9876

Note: Population: See note Table 3. The table reports the coefficients and standard errors (in parentheses) from Poisson and IV-Poisson regressions of the number sick days on a dummy for Employer provides additional private sick pay (denoted Private SP in the table), as well as the corresponding average marginal effects associated with the provision of private sick pay. The reported coefficients on the instruments are from separate Logit-regressions of the probability of providing additional private sick pay. The reported Likelihood-ratio tests are constructed from similar separate Logit-regressions. ** and * denote 1 and 5 per cent level of significance, respectively.

Table A1 Descriptive statistics. Optimal bandwidth. 2004

	Men				Women			
	Public sick pay only		Public and private sick pay		Public sick pay only		Public and private sick pay	
	Bw below	Bw above	Bw below	Bw above	Bw below	Bw above	Bw below	Bw above
Sick leave days	17.60 (43.64)	10.00 (30.22)	19.39 (49.03)	12.57 (38.12)	23.90 (54.29)	16.27 (41.88)	24.81 (56.96)	19.88 (50.85)
Ln(W)-ln(6G)	-0.10 (0.06)	0.09 (0.06)	-0.10 (0.06)	0.11 (0.06)	-0.14 (0.07)	0.10 (0.07)	-0.13 (0.07)	0.12 (0.08)
Fixed pay	0.42 (0.49)	0.32 (0.47)	0.32 (0.47)	0.25 (0.43)	0.33 (0.47)	0.23 (0.42)	0.25 (0.44)	0.16 (0.36)
Piece-rate	0.16 (0.37)	0.23 (0.42)	0.07 (0.25)	0.09 (0.28)	0.12 (0.33)	0.17 (0.38)	0.06 (0.24)	0.04 (0.19)
Individual bonus	0.10 (0.30)	0.10 (0.30)	0.13 (0.33)	0.18 (0.39)	0.12 (0.33)	0.15 (0.36)	0.24 (0.42)	0.33 (0.47)
Group bonus	0.27 (0.45)	0.35 (0.48)	0.25 (0.43)	0.26 (0.44)	0.22 (0.42)	0.31 (0.46)	0.26 (0.44)	0.34 (0.47)
Surplus sharing	0.10 (0.30)	0.09 (0.29)	0.12 (0.32)	0.13 (0.33)	0.13 (0.34)	0.20 (0.40)	0.11 (0.31)	0.15 (0.36)
Commissions	0.03 (0.18)	0.03 (0.18)	0.05 (0.21)	0.05 (0.22)	0.09 (0.29)	0.13 (0.34)	0.07 (0.26)	0.07 (0.25)
Stocks	0.18 (0.38)	0.26 (0.44)	0.31 (0.46)	0.36 (0.48)	0.19 (0.39)	0.23 (0.42)	0.27 (0.44)	0.41 (0.49)
Stock options	0.05 (0.25)	0.08 (0.27)	0.04 (0.26)	0.07 (0.28)	0.10 (0.31)	0.20 (0.40)	0.07 (0.25)	0.12 (0.33)
Bonus relative to total pay	5.30 (13.58)	6.57 (13.12)	4.46 (13.27)	4.64 (12.10)	7.29 (16.44)	11.30 (19.51)	5.85 (15.68)	8.24 (18.74)
Age (years)	42.00 (10.35)	42.09 (9.80)	42.27 (10.05)	42.27 (10.05)	43.01 (9.82)	43.93 (9.47)	43.32 (9.93)	42.33 (9.23)
Education (years)	2.23 (1.83)	2.72 (1.94)	3.23 (2.19)	3.23 (2.19)	2.67 (2.30)	3.72 (2.53)	2.90 (2.35)	4.20 (2.65)
Experience (years)	23.72 (10.79)	23.36 (10.30)	23.05 (10.74)	23.05 (10.74)	24.35 (10.71)	24.21 (10.29)	24.42 (10.89)	22.12 (10.44)
Seniority (years)	9.93 (7.45)	10.36 (7.42)	10.08 (7.78)	10.08 (7.78)	9.24 (7.05)	9.79 (7.37)	9.22 (6.94)	9.01 (6.87)
Union member	0.79 (0.41)	0.77 (0.42)	0.83 (0.38)	0.80 (0.40)	0.73 (0.44)	0.67 (0.47)	0.74 (0.44)	0.68 (0.47)
N	6269	5914	11971	15014	2936	1076	7024	5449

Note: Population: Workers 2004 employed by workplaces with more than 10 employees 2003 earning more than 3G (G=National Social Service baseline figure, which is adjusted May 1st each year. In 2004 it equaled on average

58139 NOK or 4711£/8625\$). Public sick pay is limited to 6G. $\ln E = \ln W - \ln G6$ and expresses log yearly earnings less log 6G. Education is measured in years in excess of compulsory schooling. Bw below and Bw above denote bandwidth below threshold and bandwidth above threshold, respectively. This bandwidth expresses a so-called optimal bandwidth, and is calculated using the method of Imbens and Kalyanaraman (2009), see note to Table 3 and text for details.

Table A2 Test of discontinuities around cut-off in predetermined variables. 2004

	Men						Women	
	Public sick pay only			Public and private sick pay			Public sick pay only	Public and private sick pay
	All	PP	FP	All	PP	FP	All	All
Fixed pay	-0.006	-	-	0.020	-	-	-0.004	-0.014
Piece-rate	-0.012	-0.018	-	0.005	0.004	-	-	-
Individual bonus	-0.003	-0.010	-	0.016	0.028	-	-	-
Group bonus	0.024	0.033	-	-0.027	-0.024	-	-	-
Surplus sharing	-0.037**	-0.056**	-	-0.005	-0.015	-	-	-
Commissions	-0.010	-0.019	-	-0.024	-0.007	-	-	-
Stocks	0.035	0.037	-	-0.023	-0.018	-	-	-
Stock options	-0.003	-0.008	-	0.100	0.016	-	-	-
Age (years)	-0.209	-0.332	-0.453	0.193	0.068	-0.573	1.281	-0.133
Education(years)	0.018	0.191*	-0.183*	-0.002	0.055	0.028	-0.200	-0.225
Experience(years)	-0.220	-0.451	-0.059	-0.186	-0.045	-0.604	1.499	0.112
Seniority(years)	0.117	0.511	-0.623	-0.043	0.013	-0.140	-0.375	0.602
Union member	0.007	0.021	-0.012	0.002	0.008	-0.004	-0.023	0.021
Workplace age	0.409	-0.176	1.532	-0.512	-0.892	0.349	-0.460	0.210
Ln(workforce size)	-0.014	0.073	-0.092	-0.023	-0.027	0.050	0.029	-0.053

Note: Population: See note to Tables 3 and 4. Each cell reports the treatment effect of the discontinuity at 6G for the variable denoted in column 1. Based on default bandwidth calculated using the method of Imbens and Kalyanaraman (2009). ** and * denote 1 and 5 per cent level of significance, respectively.

Figure 1 Sick leave days and sick pay regimes for male workers

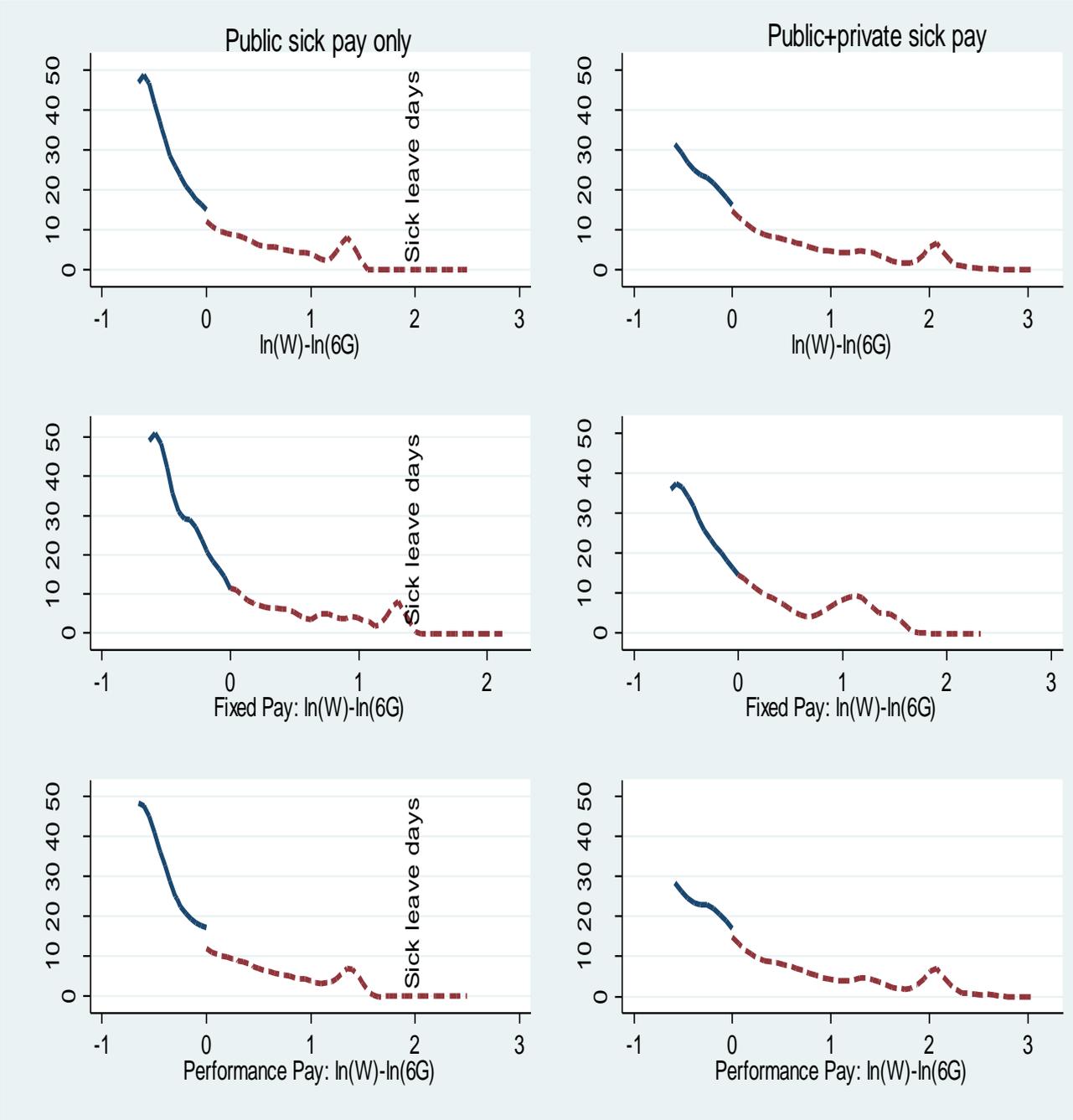


Figure 2 Sick leave days and sick pay regimes for female workers

