WORKING PAPERS IN ECONOMICS

No. 8/12

ESPEN BRATBERG AND KARIN MONSTAD

WORRIED SICK? WORKER RESPONSES TO A FINANCIAL SHOCK

Department of Economics
UNIVERSITY OF BERGEN

Worried Sick?

Worker Responses to a Financial Shock

Espen Bratberg
University of Bergen, Department of Economics
Karin Monstad
Uni Rokkan Centre

December 2013

Abstract:
Excessive sickness absence may hurt productivity and put a strain on public finances. One explanation put forward for increasing absence rates is that a tougher labour market represents a health hazard. A competing hypothesis is that loss of job security works as a disciplinary device. We use a financial shock that hit the public sector in Norway in 2007 in some, but not all, municipalities to identify the effect of reduced job security on sickness absence. Public sector workers in municipalities that were not affected are used as a control group in a difference-in-differences analysis. We find that sickness absence among public employees decreased considerably in the year after the shock in the affected municipalities. The results survive a number of robustness checks. The evidence is strongest for women, and consistent with a hypothesis that reduced job security has a disciplining effect.

Financial support from the Norwegian Research Council (Grant 187912) is gratefully acknowledged. We are also grateful for information from Ingvar Linde, Jan G. Myrvang, and chief executives of the eight municipalities impacted by the financial shock.

1. Introduction

When the United States housing bubble of the early 2000s burst in 2007, its worldwide repercussions eventually triggered the financial crisis of 2008–2009. Investors with exposure in credit default swaps tied to US subprime loans faced heavy losses, amplified by financial products with high leverage. A somewhat surprising example of such investors was a group of municipalities in energy-rich Norway, who turned out to have invested expected future earnings from hydroelectric power plants in high-risk financial products. The central government at first rejected any bailouts, and the affected municipalities had to cut running expenses at short notice. In the aftermath, the competence of small local governments to operate in financial markets has been questioned. The "Terra crisis", named after the brokerage house that sold the financial products, soon led to fears of job losses, and activated public employee unions. The unexpected nature of the financial shock makes a good case for a natural experiment, and in this paper we exploit this feature of the events to investigate how employees respond to reduced job security.

The financial loss led to massive negative coverage in national and even international media such as the *Financial Times*, *New York Times* and *Wall Street Journal* (see Figure 1). Several sources state that the general reputation of the municipalities involved was severely harmed and that their inhabitants felt embarrassed. The following quote from *New York Times* (December 2, 2007) appears to be representative of public opinion at the time: "The people in City Hall were naïve and they were manipulated." The municipalities involved had not adhered to laws and regulations, concluded experts from the County Governor's office in January 2008. Internal investigations were launched. By February 2008, the Chief Executive leaders in three of the eight affected municipalities had left their jobs because of the losses, and two years later, none were left in their original positions (www.kommunal-rapport.no).

[Figure 1: Media coverage]

When the news about the loss broke, it was clear that it would be of considerable magnitude. The complexity of the financial product added to the uncertainty. Budget cuts soon became a subject in meetings between employee representatives and administrative leaders; and articles in union member magazines expressed concern for job security and working conditions. To protect municipal welfare provision, the Government proposed a change in the law, implemented in June 2008, allowing municipalities to cover losses over periods up to ten years instead of four as the previous rule said (Ot.prp. nr. 53 (2007–2008)). This option was a response to the worry some of the municipalities expressed, but utilizing it was unattractive because it implied less municipal autonomy in economic matters, and more detailed state governance.

Until revealed by a financial newspaper in late October 2007, the approaching problems were not acknowledged at the mayors' offices, and even less in the general population. For the common municipal employee, the crisis came as a shock in the true sense of the word (Hofstad 2008). Thus, the Terra crisis is well-suited for a case-study of how worker behavior is affected by economic uncertainty at the employer level. In this paper we focus on sickness absenteeism, a much-debated problem causing worries in several European countries. Job-related shocks may influence sickness absence by affecting health, but also by affecting incentives to report sick. These two channels are clearly not mutually exclusive but we can shed light on which channel dominates.

High absence rates may cause large production losses and strain public finances. Different explanations have been put forward; for a recent summary of the Norwegian case, see Markussen et al. (2011). There is a growing focus at the role of the workplace: working conditions in general, but also downsizing and organizational changes. However, causal

relationships are difficult to trace because of selection into education, occupations, employment and workplaces. In this analysis we aim to trace a causal impact of a negative shock at the employer level on sickness absence, using the financial shock in 2007–2008 to a specific group of local governments as the source of exogenous variation. We argue that the financial problems may have led to a reduction in municipal workers' perceived job security that affected sick absence behaviour, even though there were no consequences for sickness compensation schemes.

We now proceed with a short review of related literature in the next section. Section 3 gives a short account of relevant institutional facts; Section 4 details our empirical strategy, and Section 5 describes the data. Section 6 presents the econometric results, and Section 7 concludes. We find evidence that average absence days decreased after the shock by about 10% for women employed by the affected municipalities. For men we find even larger effects, but we cannot rule out that they are affected by changes in the composition of male employees.

2. Related literature

Obviously there is a strong health component in sickness absence, but absence is also affected by the opportunity cost of reporting sick. There is an empirical economic literature that relates absence to economic incentives, such as Allen (1981); Dunn and Youngblood (1986); Kenyon and Dawkins (1989); Barmby et al. (1991); Johansson and Palme (2005); Puhani and Soderlof (2010). Johansson and Palme (1996) find that a Swedish reform which made absence more costly for workers reduced sickness absence. Notably, for all practical purposes Norwegian sickness insurance entails 100% income replacement. There are also studies which relate sickness absence to the unemployment rate (e.g. Leigh 1985), where the proposed mechanism

is that an increased risk of job loss works as a disciplining device which reduces the sickness propensity. An alternative explanation to the observed countercyclical variation in sickness absence in some countries is that labor force composition varies over the cycle as labor demand increases or decreases; and that less healthy workers are pushed out of the labor market in downturns. However, it is hard to find evidence that composition explains cyclical absence variation (Arai and Thoursie 2005; Askildsen et al. 2005). Recently, a growing body of research connects sickness leave and other social insurance plans to social norms and attitudes (e.g., Lindbeck et al. 1999; Bamberger and Biron 2007; Rege et al. 2012; Ichino and Maggi 2000). A strand of contributions aims to identify social interaction effects (Bradley et al. 2006; Hesselius et al. 2009; Lindbeck et al. 2009). One such interaction is 'learning' in the sense that workers in the same firm have similar absence behavior. Another is reciprocity between employer and employee: if the employer treats the workers well, they may respond by having less absence. Vice versa: worsened conditions for workers may induce increased absence to 'get back at' the employer (Fehr and Gächter 2000).

A negative shock to the employer has similar effects as a rise in the local unemployment rate – jobs are perceived as less secure, and adding to the threat of losing the job is firm reorganizations that may affect workers. The literature presents two competing hypotheses for analyzing the financial trouble of affected Norwegian municipalities in 2007–2008. Both are relevant in a situation where employees are worried about the future, whether they think that there is a (greater) risk of job loss, or worry about an unfavorable change in their job content. The first hypothesis claims that less secure jobs will encourage workers to avoid absenteeism. This is supported by Arai and Thoursie (2005); Ichino and Riphahn (2005); Lindbeck, Palme and Persson (2006); but not supported by Markussen et al. (2011). On the other hand, insecurity and worry caused by reorganization may in itself be a health hazard, as indicated in the well-known Whitehall studies (Ferrie et al. 1995, 1998a, 1998b). Using register data,

Røed and Fevang (2007) find that sickness absence grew among Norwegian nurses and auxiliary nurses employed by municipalities who experienced downsizing or large staff reshuffling at their unit. The present study differs by exploiting an external shock and not focusing on a particular group of workers.

3. Institutional background

Norwegian sickness insurance is mandatory and regulated by law, covering all employees who have been with the same employer for at least two weeks. Once this requirement is met, coverage is 100% from the first day. A medical certificate is necessary for absence spells lasting more than three days. For spells lasting more than eight weeks, the physician is obliged to provide a more detailed certificate to the Social Insurance authorities, stating diagnosis and a prognosis assessment. The first 16 days are paid by the employer (the employer period), whereas the remaining period is paid by social insurance, organized under the National Insurance Administration (NAV). The maximum period of benefits is one year, including the employer period. NAV expenses are covered jointly by wage earners' income taxes and employers' payroll taxes. The compensation scheme stands out as very generous, and compared to most other countries, absence rates are high. During the past ten years, certified sickness absence has been fluctuating around 6–7%, peaking in 2003 at almost 7.5%. Public expenditures for the program (not including the employer period) are substantial, about 2.5% of GDP. Measures to reduce sickness absence have been on the agenda for several years, but suggestions to reduce the replacement ratio or to increase the employer period have proved highly controversial. In 2001, the so-called "Including working life" agreement was introduced. This agreement, including the government, employers' and workers' organizations, aimed to reduce sickness absence by 20% from the 2001 level. The agreement did not involve any changes in replacement rates but emphasized improving working conditions and better follow-up of sick-listed workers. In the last quarter of 2012 the absence

rate was 5.6%. This is down from 2001, but the reduction can be traced to a tightening of doctors' certification rules in 2003 – not to the agreement.

Norway has a large public sector, with public consumption at almost 30% of GDP. The number of public employees is also substantial. About 30% of the workforce is employed in the public sector, and more than two thirds of this share consists of municipal workers. Municipalities can produce services themselves, purchase services from the private sector, or produce them in co-operation with other municipalities. Worker protection is Norway is quite strong; in particular there are regulations against dismissing workers while on sick leave.

4. Empirical strategy

Our source of exogenous variation in job security is the financial shock that hit eight Norwegian municipalities in the late autumn of 2007. Employees of other municipalities were not affected by the shock and may be used as a control group in a natural experiment set-up. We apply a standard difference-in-differences (DID) approach. In what follows, we use the standard term "treatment" for exposure to the shock. In its simplest form, DID compares average sickness absence in the treated group to the average in the untreated group, before and after an event which is exogenous to group assignment. Let Y denote the outcome (sickness absence), and let subscripts 0 and 1 denote the pre- and post-treatment periods, respectively. The DID estimator, β, of the average treatment effect is then

$$(1)\ \beta = \left(\bar{Y}_{treat,1} - \bar{Y}_{treat,0}\right) - (\bar{Y}_{contr,1} - \bar{Y}_{contr,0}).$$

The idea is that the average change in outcome for the control group is the same as it would have been for the treatment group in absence of treatment, under the identifying assumption that there is no difference in pre-treatment trends between the groups. With multi-period data trends may be incorporated in the model. Using quarterly data 2006-2008, we estimate the DID effect from the following regression model for individuals i in periods $t = 1,\ldots, T$. Let FS_{it} be a dummy variable indicating that individual i worked in one of the affected municipalities in period t. Furthermore, let $POST_t$ denote a dummy variable which equals 1 in periods after the shock, and D_t, $t = 1,\ldots,T$ period dummies. The regression equation is

$$(2)\ Y_{it} = \alpha + \beta FS_{it}POST_t + \delta_{FS}FS_{it} + \sum_{t=2}^{T}\delta_t D_t + \sum_{t=2}^{T}\delta_{t,FS}D_t FS_{it} + \sum_{q=2}^{4}\delta_q D_q + \delta_X X_{it} + \varepsilon_{it},$$

where ε_{it} is a random error term and X_{it} is a vector of individual characteristics. We have also included quarter dummies D_q to control for seasonal variation in sickness absence. This model allows for different time trends and intercepts for treatments and controls, and the treatment effect, β, is modeled as the post-treatment shift in the treatment group trend. Equation (2) is estimated by ordinary least squares (OLS) and fixed effects (FE). The FE estimator allows for unobserved individual heterogeneity that is time-constant.

A potential pitfall of this approach is that even though the financial shock was unexpected, workers may have self-selected into the "Terra municipalities". We have good counter-arguments: first, the control, as well as the treatment, group consists of municipal employees. Thus, possible selection into public/private employment based on preferences for job security is not an issue. Second, the control group is selected from municipalities with similar

characteristics as the exposed municipalities; see the data section for details. Third, the FE estimator controls for time-invariant unobserved individual characteristics. For instance, if the affected municipalities were known to have particularly lax – or strict – practices regarding sickness absence that attracted workers with particular attitudes, such unobserved characteristics are differenced out of the model. The same argument applies to differences in individual preferences or health endowments. In addition, we estimate the model on workers in the private sector. It could be that the financial shock in the public sector affected the private sector through negative demand effects. However, it is less likely that such effects were large enough to affect sickness absence in the sector, and we do not expect to see any particular changes in sickness absence following the shock. Thus, estimating the model on the private sector serves as a robustness check.

5. Data

The key data source is administrative registers from Statistics Norway, which comprise the full population and enable us to link data on employer with data on sickness absence for the same individual. First we identify all individuals who held a job in the municipal sector in the "Terra municipalities" or the comparison group by December 31, 2006, about a year prior to the financial shock Our control group consists of employees in municipalities which, like treatment group employers, gain income from hydroelectric energy production. Employees above the age of 66 are excluded. Our main analysis is performed on this sample of public employees. As noted above, we use a sample of private employees in the same municipalities for comparison. The financial shock did not affect workers in the private sector directly, and we do not expect to find changes in their absence related to the shock.

This data set is merged to the data on sickness absence from The Norwegian Labor and Welfare Administration by means of the unique personal identification code. We include only

absence episodes caused by the employee's own sickness, i.e. absence due to illness among family members is ignored. In order to ease the construction of the data set we exclude individuals with a very high number of sickness absence spells. Sickness absence is measured during twelve 3-month periods, i.e. January 2006–December 2008. This procedure leaves us with a data set of 336,621 individuals, each with 12 observations. For details on sample selection, see table A1.

The treatment group counts 7,985 individuals. Our control group consists of close to 70000 employees in municipalities that are comparable in the sense they have income from hydroelectric energy production. These 167 municipalities are located in all regions of the country, within 16 of Norway's 20 counties. Data on the eight affected municipalities are found in table A2. The municipalities differ in some respects: 1–4 are situated in Southern or Western Norway, whereas 5–8 are located within the same county in Northern Norway. [1]

Municipalities 2, 5, and 8 are middle-sized towns by Norwegian standards, whereas the rest are thinly settled. They all invested heavily in complicated financial products. These investments were made possible by their expected income from hydroelectric energy production. Municipalities in this category are typically affluent. Still, the financial loss in 2007 and 2008 was of considerable magnitude to most of them, as can be seen from table A2 where it is expressed in per capita terms. The loss recorded in 2007 had to be covered by reduced expenditure in future budgets. Figure 2 shows that on average the growth in expenditure per capita was, although positive, lower in the municipalities affected than in control group municipalities in 2008 and 2009.

[Figure 2]

[1] The analogue sample of private employees includes 21516/134744 workers in the affected/non-affected municipalities, respectively.

In the analysis of sickness absence, we consider two outcomes: i) the number of days of certified sickness absence per quarter of a year and ii) a dummy variable indicating at least one absence spell in a given period (named 'incidence' in the tables). Certified sickness absence excludes the initial 16 days of each spell that is covered by the employer. Our results may therefore be interpreted as "lower bounds" of the full effect on the incidence of sickness absence and the number of sick days.

The pre- and post-shock periods are defined, respectively, as Q1, 2006 – Q4, 2007 and Q1–Q4, 2008. Media reports on the financial losses commenced in October–November 2007 but it seems reasonable that potential effects on absence level would be observed no sooner than the following quarter.

[Table 1a Descriptive statistics]

[Table 1b Descriptive statistics by gender]

Tables 1a and 1b show descriptive statistics for the treatment and control groups, before and after the financial shock. We note that the groups are similar with respect to age, education and family characteristics. Their distribution on employment sectors is also similar, see table A3. However, there a substantial difference in the change in sickness absence from before to after the financial shock: -0.442 days per quarter of year, i.e., 7–8 % of an average absence of about six days per quarter of year. The difference in incidence is of the same order. These are simple DID estimates according to equation (1). The gender-wise calculations in table 1b indicate that the absence level is notably higher for women, but the change is largest for men.

In the appendix, Table A4, we show the according distribution for workers in the private sector. There is a difference between workers in the two groups of municipalities but in the opposite direction.

[Figure 3 Average days of absence]

[Figure 4 Average incidence of sickness absence]

Figure 3 shows seasonally adjusted absence days in the observation period by gender and group. For both genders, absence decreased through 2008, but apparently more in the affected municipalities. Average incidence, displayed in figure 4, reveals a similar tendency for women. For men the picture is less clear: in the treatment group, incidence was reduced in the beginning of 2008 but then increased, while there is no clear trend in the control group.

[Figure 5: Average absence days, private sector]

[Figure 6: Average absence incidence, private sector]

Figures 5 and 6 display the according averages for the private sector. We do not find a similar pattern here.

Our main impression from exploring descriptive statistics/ is that sickness absence was reduced for public employees in the affected municipalities; most clearly for men and more distinctly for absence days than for incidence. In the next section we investigate whether this finding remains in a regression analysis with control variables.

6. Estimation results.

Equation (2) was estimated by OLS and individual fixed effects (FE) for both outcomes (absence days and incidence). In the OLS regressions we control for age, education, marital status, and number of children, in addition to quarter of year and time period. In FE regressions most of the controls are excluded because they do not vary over time. Table 2 shows results for absence days and incidence. We only report the parameter of interest, β in equation (2), with robust standard errors clustered at municipality level.

[Table 2 Effect of the financial shock on sickness absence in the public sector]

For absence days, the post-shock effect is statistically significant and larger for men than for women, in accordance with the descriptive statistics. The FE estimates are -0.99 and -0.74 for men and women, respectively. This is more than the simple estimates in table 1b, in particular for women. For women, the FE estimate is also clearly larger than the OLS estimate. In general, we put more confidence in FE because it controls for unobserved heterogeneity. Sickness absence may be affected by, e.g., health and attitudes, and the case for the FE estimator seems particularly strong. The standard tests also favor FE. The relative changes are substantial: a decrease of 10% for women and 23% for men as compared to the average pre-shock levels.

As 'incidence' is a discrete outcome, the coefficients in the lower panel of Table 2 are interpreted as marginal effects on the probability of absence.[2] Here, the estimated effect is statistically significant only for men: -0.011 or a decrease of 1.1 percentage points with FE. The point estimate for women is larger than the according number in Table 1b, but statistically insignificant.

[Table 3 Effect of the financial shock on sickness absence in the private sector]

Panel A of Table 3 reports FE results from estimating equation (2) for workers in the private sector. As the financial shock affected the public sector, we do not expect to find any effect on private sector employees. Panel B shows coefficients on an interaction term between the impact dummy and a private sector dummy in a pooled regression of workers in the private and public sector, i.e., the coefficient gives the differential impact. The top panel shows no evidence of any post-shock effect on days of absence. This strengthens our interpretation that

[2] We do not apply non-linear probability models like logit or probit because it is easier to implement a fixed effects estimator in a linear model. The linear probability model has the disadvantage that it may predict outcomes outside the unit interval; however the focus here is on marginal effects.

the according reduction in the municipal sector is due to the financial shock. For incidence, we actually find a post shock decrease for women, which is significant at the 5% level but smaller in size than the decrease for men in the public sector. However, the private/public interaction terms are insignificant for incidence, giving evidence against an "effect" in the wrong sector. On the other hand, the interactions are significant and positive for absence days, indicating that public sector absence in the "Terra" municipalities decreased compared to private employees in the same municipalities (and also compared to the comparison group).

The evidence this far is quite clear regarding the length of absence spells: There is a significant reduction for both genders in the municipal but not in the private sector. For the probability of absence, the evidence is more mixed. Inspection of the graphs showed that the data are noisier. The regressions gave results for men that were consistent with the absence days results, but not for women. The reason may be that we analyze sickness spells that last two weeks or more, and reducing the duration is a smaller adjustment than skipping the sickness episode altogether. The gender differences are interesting. It is well known that women's sickness absence levels are higher than men's and they also appear less elastic to negative organizational shocks. However, we cannot infer whether this is due to differences in job characteristics, health or attitudes.[3]

We have performed several tests for robustness. First, Table 4 checks if there is any effect of a placebo shock.

[Table 4 Placebo shock]

We have redefined the treatment dummy to equal one from Q1, 2007 onwards. This is almost one year before the crisis, and there were no other particular events at the time that should

[3] Table A3 shows that men and women work in different sectors within the municipalities, and women are known to have a much higher frequency of part-time jobs.

have affected sickness absence systematically. Thus, if this placebo treatment turns out to have any effect, it leads us to suspect that the effects revealed in tables 2 and 3 are spurious. In the left panel of table 5 we include only observations for Q1, 2006–Q4, 2007. There is no effect of the placebo treatment on either outcome. In the right panel we also include 2008; the treatment dummy equals one in 2007 and 2008. All coefficients but one are insignificant in this case, too. One should note that the placebo in the right panel is different from the other in the sense that also observations from the true post-shock period are included. The overall impression from the placebo regressions is to increase our confidence in the results in Table 2.

[Table 5 Omitting municipalities]

As seen in table A2, the affected municipalities vary in size from Haugesund (32,302 inhabitants) to Hattfjelldal (1,482 inhabitants), and their recorded losses and expenditure levels vary as well. Thus, the results may be sensitive to inclusion/exclusion of some municipalities. To check this, we have re-estimated the models, omitting one affected municipality at a time. The results are depicted in Table 5. Compared to the main results, the reduced samples produce quite similar results for absence days and incidence as well, the exception being that excluding the second-largest municipality, Rana (column 8), makes the effect on incidence insignificant but still negative. Furthermore, the strength of the response is related to the size of the shock. Column 9 shows that when we omit the municipalities where the financial losses recorded were smallest (in Kvinesdal, Haugesund and Narvik, as seen from table A2), the estimated effects generally increase in magnitude.

Even though we do not find evidence at the aggregate level that the results are driven by particular municipalities, it could be the case that worker turnover differs between the treatment and comparison samples. If the most sick-prone workers change sector, move to another municipality, or move out of employment completely, it could affect our results. We

address these pitfalls by i) analyzing samples of stable workers, ii) comparing turnover in the treatment and comparison groups, and iii) looking at absence histories of workers who changed employer in the post-shock period.

[Table 6 Stable workers]

Table 6 shows fixed effects regressions of absence days and incidence for subgroups of stable workers. Columns 1-3 show results for workers who have stayed with the same employer as they had in Dec 2006 continuously from Jan 2006 till Dec 2008 (column 1), in 2007 and 2008 (column 2) and in 2008 (column 3).Comparing with the main results, repeated in the last column, we find that for days, they are very similar when men and women are estimated jointly. When estimating by gender, the point estimates are similar but significant only for women. The incidence estimates are almost unchanged, however. We conclude that our main results are supported, but with some uncertainty regarding a potential selection effect for men.

[Figure 7 Turnover]

Figure 7 shows turnover, defined as the proportion of individuals in each period that was not employed with the same employer as on Dec 31, 2006. The employer-employee relationship of that date is the basis for the indicator for working in an affected municipality in equation (2). Turnover is defined separately for the subsamples of treated and non-treated. Levels are somewhat higher in the treatment group but trends are very similar. However, we also estimated linear probability models for the probability of leaving a public sector job in the affected municipalities (not reported). We found that men actually had a larger probability of leaving in the periods after the shock, but not women.

[Figure 8 Days of sickness absence by employment stability, men]

[Figure 9 Days of sickness absence by employment stability, women]

The purpose of Figures 8 and 9 is to investigate absence trends before the shock for leavers and stayers. Absence days are plotted separately for stayers and leavers. We see that levels are somewhat higher for female stayers in the impacted municipalities than in the comparison group, but with no clear difference in trends. For both gender, there is an upward shift in 2007 for leavers, and for men this shift is larger in the impacted group. Thus there is some evidence that there was a selection out of employment (or to other employers) for men who were the most sick-prone before the financial shock, and this may explain the absence reduction for men estimated for the main sample. As we saw in Table 8, absence was reduced among stable workers, but the effect on absence length seems to be driven mostly by women. Incidence, on the other hand, decreased for men in the groups of stable workers.

Our main conclusion is that the financial shock reduced sickness absence among employees in the municipalities that were affected. After inspecting selection out of the sample, the evidence of an effect on stable workers is strongest for women. The placebo exercise gives evidence against the effect being a time trend, and the conclusion also is robust to omitting municipalities or workers who changed job.

7. Concluding remarks

The financial shock that hit some Norwegian municipalities in 2007–2008 might affect sickness absence of public employees through several channels. Previous research suggests some main hypotheses. First, the crisis could have a direct health effect. In that case, we would expect sickness absence to increase in line with the Whitehall studies. To the contrary, we find that sickness absence decreased. Second, the reciprocity hypothesis also implies increased absence: in response to the irresponsible behavior of employers (the "Terra municipalities") workers would feel less compelled to hold back on absence. Again, the fact

that absence was actually reduced falsifies hypotheses that imply increased absence. Third, one hypothesis is that reduced absence is brought about by changes in the composition of workers. There are some indications of selection out of employment among men. On the other hand, we also find a decrease in incidence among stable male workers. Fourth, the prospect of jobs becoming less secure could have a disciplining effect leading to less absence. Our results are consistent with this hypothesis and also agree with previous research concluding that less secure job environments reduce sickness absence, whether insecurity is brought about by rising unemployment rates (Arai and Thoursie 2005), probation (Ichino and Riphahn 2005) or softening of job security legislation (Lindbeck et al. 2006). Our confidence in this interpretation of the results is strengthened by the fact that the financial loss actually hampered economic activity in the municipalities affected during the period studied and that the response is stronger in municipalities with a high per capita loss.

In our analysis, the data is at the individual level whereas the negative shock came at the employer level, and the mechanism is not quite clear. Even so, we find quite large effects – sickness absence was reduced by about 10% for women. The bad news became publicly known in October–November 2007. Media coverage was extensive, and a statement in November from the leader of the largest public employee union that cuts must not be at the cost of workers, indicates that there was a fear of such cuts. We find that sickness absence dropped from the first quarter of 2008, but we have no evidence that the number of jobs was reduced at that time. However, it seems probable that the possibility of less secure jobs may have had a disciplining effect that led to reduced sickness absence. Thus it is the expectation of future downsizing that may have induced less absence, not downsizing itself. It should also be noted that what we have found is a short run effect. The post-shock period is too short to test for long run effects; moreover it is most likely that the effect of an *expected* reduction in job security is temporary. Our results are not necessarily at odds with Røed and Fevang

(2007) who found that *actual* downsizing increased absence among Norwegian nurses. A possible mechanism is that the threat of future downscaling gives workers incentives to reduce absence in the short run, but that prolonged insecurity has negative health effects that dominate in the longer run.

References

Allen, S. G. (1981). "An Empirical Model of Work Attendance." *Review of Economics and Statistics* 71: 77–87.

Arai, M., and Thoursie, P. S. (2005). "Incentives and Selection in Cyclical Absenteeism." *Labour Economics* 12(2): 269–280.

Askildsen, J. E., Bratberg, E., and Nilsen, Ø. A. (2005). "A Panel Data Study of Physicians' Labor Supply: The Case of Norway." *Health Economics* 14 (10): 1035–1045.

Bamberger, P., and Biron, M. (2007). "Group Norms and Excessive Absenteeism: The Role of Peer Referent Others." *Organizational Behavior and Human Decision Processes* 103: 179–196.

Barmby, T. A., Orme, C. D., and Treble, J. G. (1991). "Worker Absenteeism: An Analysis Using Micro Data." *Economic Journal* 101: 214–229.

Bradley, S., Green, C. and Leeves, G. (2007). "Worker Absence and Shirking: Evidence from Matched Teacher-School Data." *Labour Economics* 14(3): 319.

Dunn, L. F., and Youngblood, S. A. (1986). "Absenteeism as a Mechanism for Approaching an Optimal Labor Market Equilibrium: An Empirical Study." *Review of Economics and Statistics* 68: 668 – 674.

Fehr, E. and Gächter, S. (2000). "Fairness and Retaliation: The Economics of Reciprocity." *Journal of Economic Perspectives* 14(3): 159–181.

Ferrie, J. E., Shipley, M., Marmot, M. G., Stansfeld, S. A., and Smith, G. D. (1995). "Health Effects of Anticipation of Job Change and Non-Employment: Longitudinal Data from the Whitehall II Study." *British Medical Journal* 311: 1264–1269.

Ferrie, J. E., Shipley, M., Marmot, M. G., Stansfeld, S. A., and Smith, G. D. (1998a). "The Health Effects of Major Organisational Change and Job Insecurity." *Social Science & Medicine* 46: 243–254.

Ferrie, J. E., Shipley, M., Marmot, M. G., Stansfeld, S. A., and Smith, G. D. (1998b). "An Uncertain Future: The Health Effects of Threats to Employment Security in White-Collar Men and Women". *American Journal of Public Health* 88: 1030–1036.

Hesselius, P., Johansson, P., and Nilsson, P. (2009). "Sick of Your Colleagues' Absence?" *Journal of the European Economic Association* 7(2–3): 583–594.

Hofstad., M. (2008). "Eventyret om Terra og de åtte små kommunene som dro til Wall Street for å bli rike." Hegnar Media AS, Oslo. [The Fairy Tale about Terra and the Eight Small Municipalities that Went to Wall Street to Get Rich]. In Norwegian.

Ichino, A., and Maggi, G. (2000). "Work Environment and Individual Background: Explaining Regional Shirking Differentials in a Large Italian Firm." *Quarterly Journal of Economics* 115(3): 1057–1090.

Ichino, A., and Riphahn, R. T. (2005). "The Effect of Employment Protection on Worker Effort: Absenteeism During and After Probation." *Journal of the European Economic Association* 3(1): 120–143.

Johansson, P., and Palme, M. (1996). "Do Economic Incentives Affect Worker Absence? Empirical Evidence Using Swedish Micro Data." *Journal of Public Economics* 59: 195–218.

Johansson, P., and Palme, M. (2005). "Moral Hazard and Sickness Insurance." *Journal of Public Economics* 89: 1879–1890.

Kenyon, P., and Dawkins, P. (1989). "A Time Series Analysis of Labour Absence in Australia." *Review of Economics and Statistics* 71: 232–39.

Leigh, J. P. (1985). "The Effects of Unemployment and the Business Cycle on Absenteeism." *Journal of Economics and Business* 37: 159–170.

Lindbeck, A., Nyberg, S., and Weibull, J. (1999). "Social Norms and Economic Incentives in the Welfare State." *Quarterly Journal of Economics* 114: 1–35

Lindbeck A., Palme, M., and Persson, M. (2006). "Job Security and Work Absence: Evidence from a Natural Experiment." *CESifo Working Paper Series* No. 1687.

Lindbeck A., Palme, M., and Persson, M. (2009). "Social Interaction and Sickness Absence." *CESifo Working Paper* No. 2215.

Markussen, S, Røed, K, Røgeberg, O. J., and Gaure, S. (2011). "The Anatomy of Absenteeism." *Journal of Health Economics* 30(2): 277–292.

Ot.prp. nr. 53 (2007–2008) Om lov om endringer i kommuneloven. [Proposition to the Odelsting No. 53 2007–2008 on Changes in the Local Government Act.]

Puhani, P. A., and Soderlof, K. (2010). "The Effects of a Sick Pay Reform on Absence and on Health-Related Outcomes." *Journal of Health Economics* 29: 285–302.

Rege, M., Telle, K., and Votruba, M. (2012). "Social Interaction Effects in Disability Pension Participation: Evidence from Plant Downsizing." *The Scandinavian Journal of Economics*, 114(4): 1208-1239.

Røed, K., and Fevang, E. (2007). "Organizational Change, Absenteeism, and Welfare Dependency." *Journal of Human Resources* 42(1): 156–193.

FIGURES

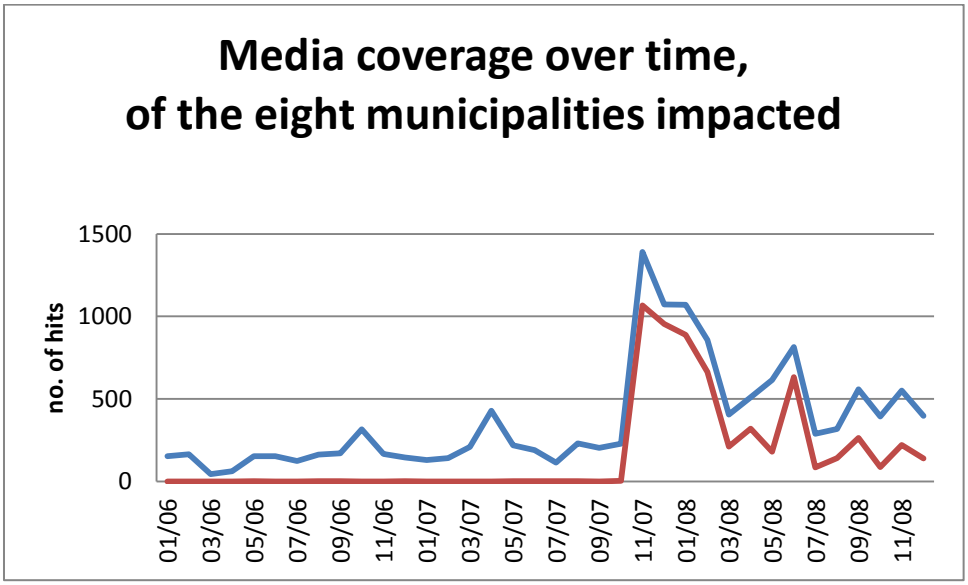

Figure 1. The X-axis shows the number of hits across Norwegian media; printed and/or web-based newspapers, periodicals, radio and television. Numbers are counted per month during the years 2006–2008. The blue line shows hits that include at least one of the names of the municipalities impacted, without further restrictions. The red line shows hits under the restriction that the term "Terra*" should be included along with the name of at least one municipality.Source: http://ret-web05.int.retriever.no/services/

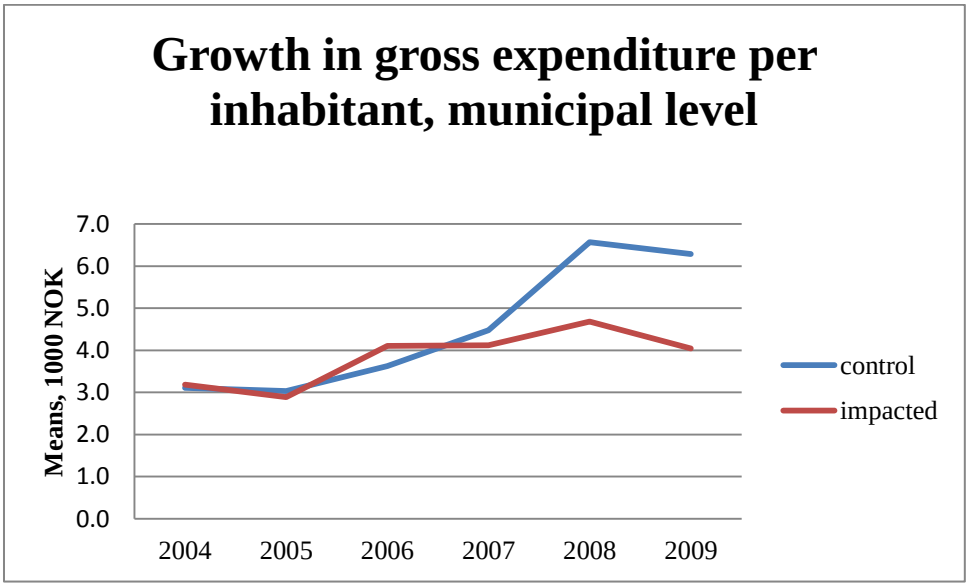

Figure 2. Growth in gross expenditure per inhabitant measured in 1000 Norwegian kroner, nominal terms. Means are taken across municipalities in the control group and the group of impacted municipalies, respectively.

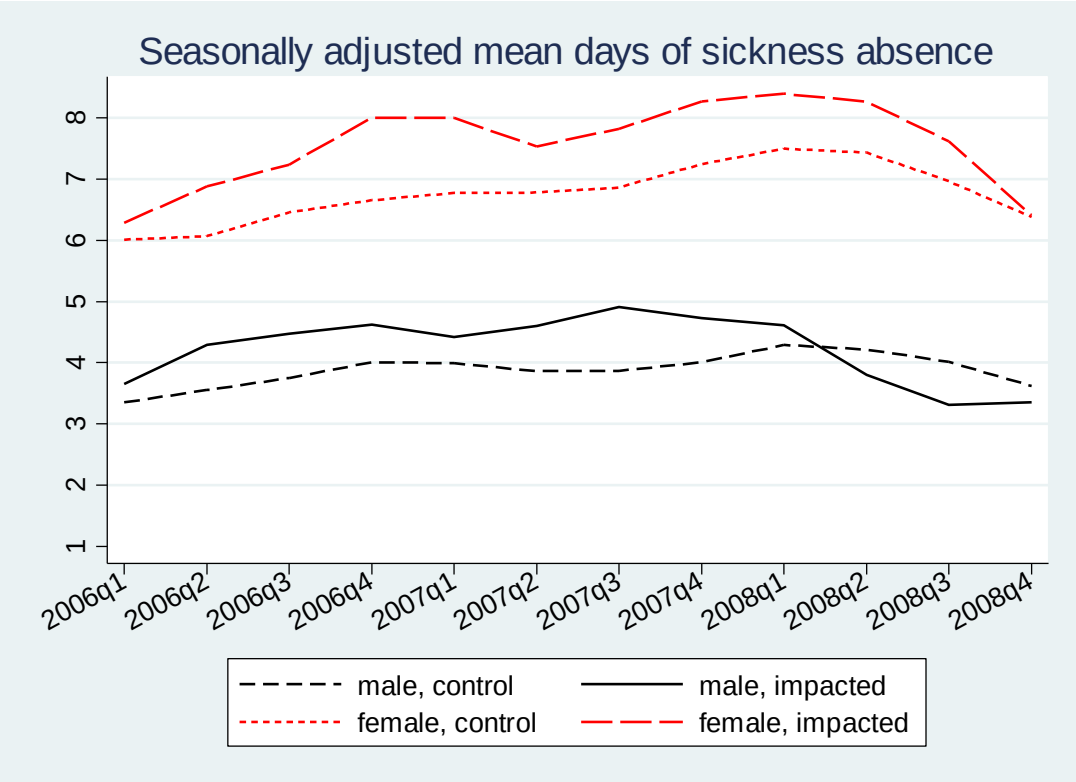

Figure 3. Mean number of days 2006-2008, by gender, period, and treatment status.

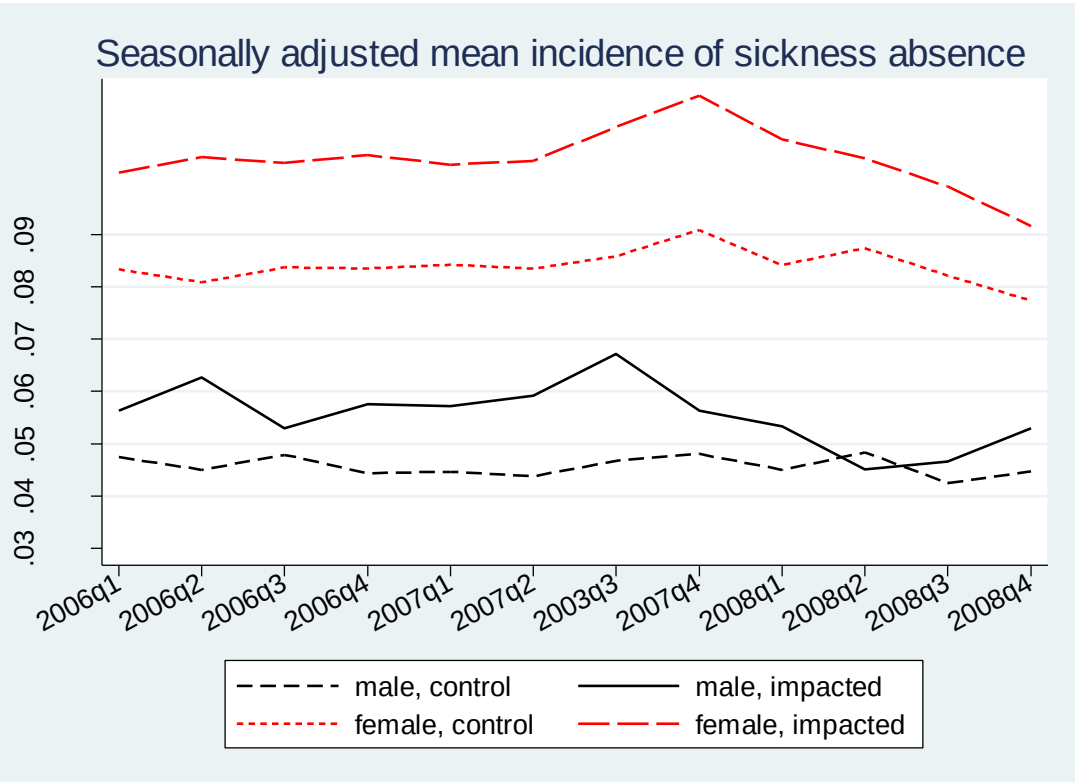

Figure 4. Mean incidence 2006-2008, by gender, period, and treatment status.

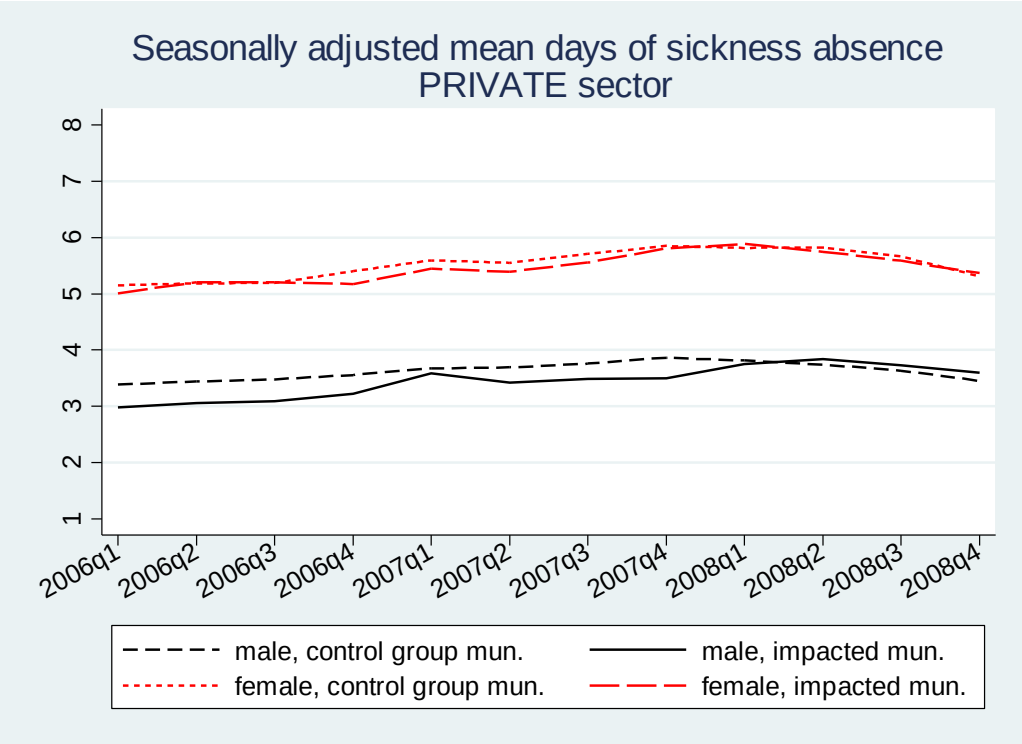

Figure 5. Mean number of days 2006-2008, by gender, period, and treatment status, within private sector.

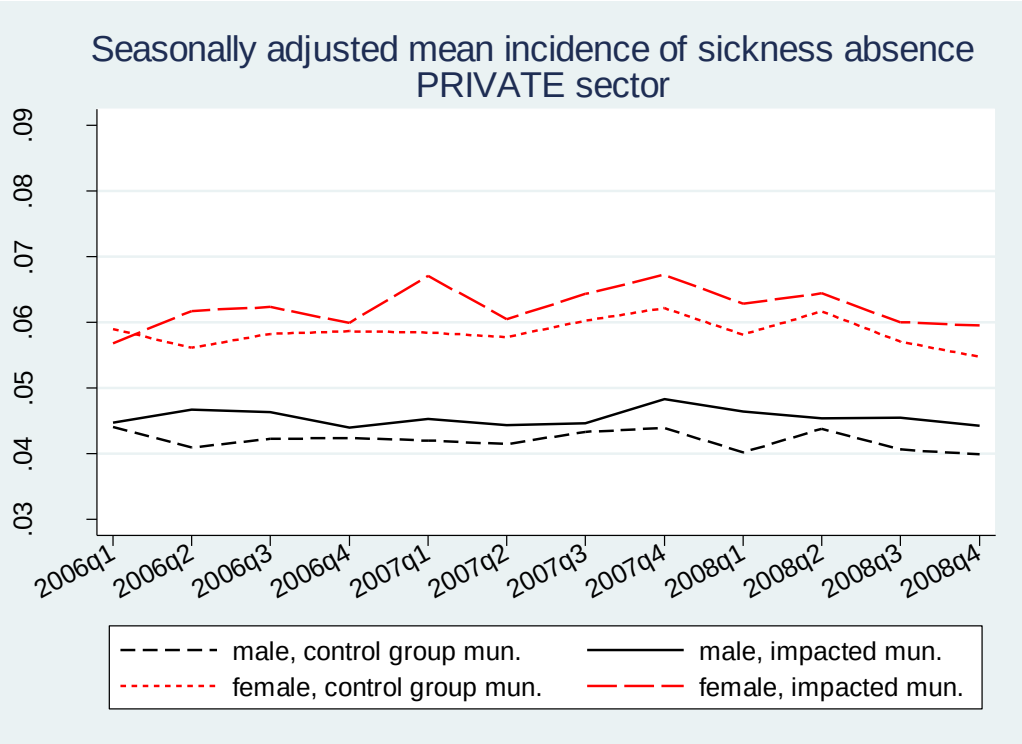

Figure 6. Mean incidence 2006-2008, by gender, period, and treatment status, within private sector.

.

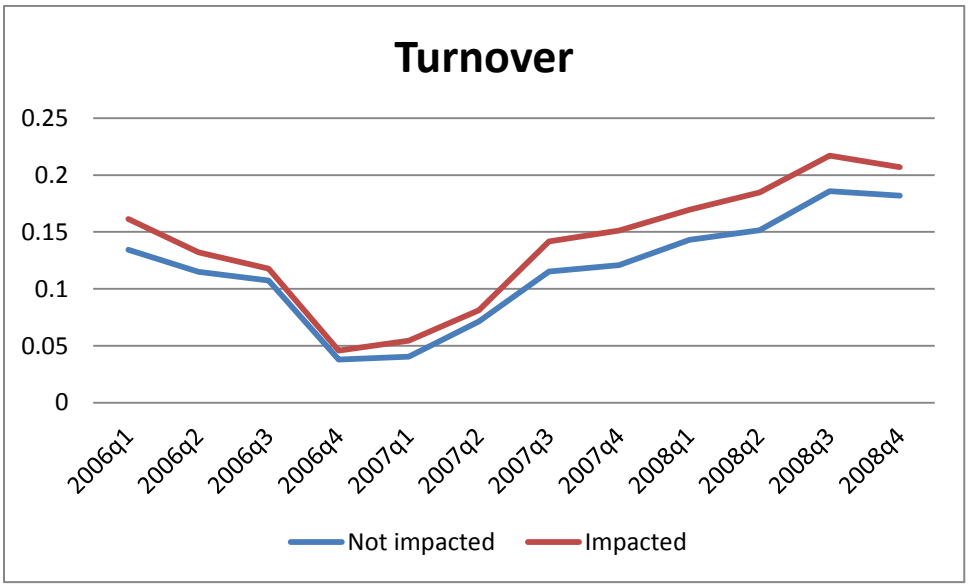

Figure 7. Turnover, by treatment status. The figure shows the proportion of each subsample that did not have the same employer throughout each quarter of year as they had one year prior to the shock.

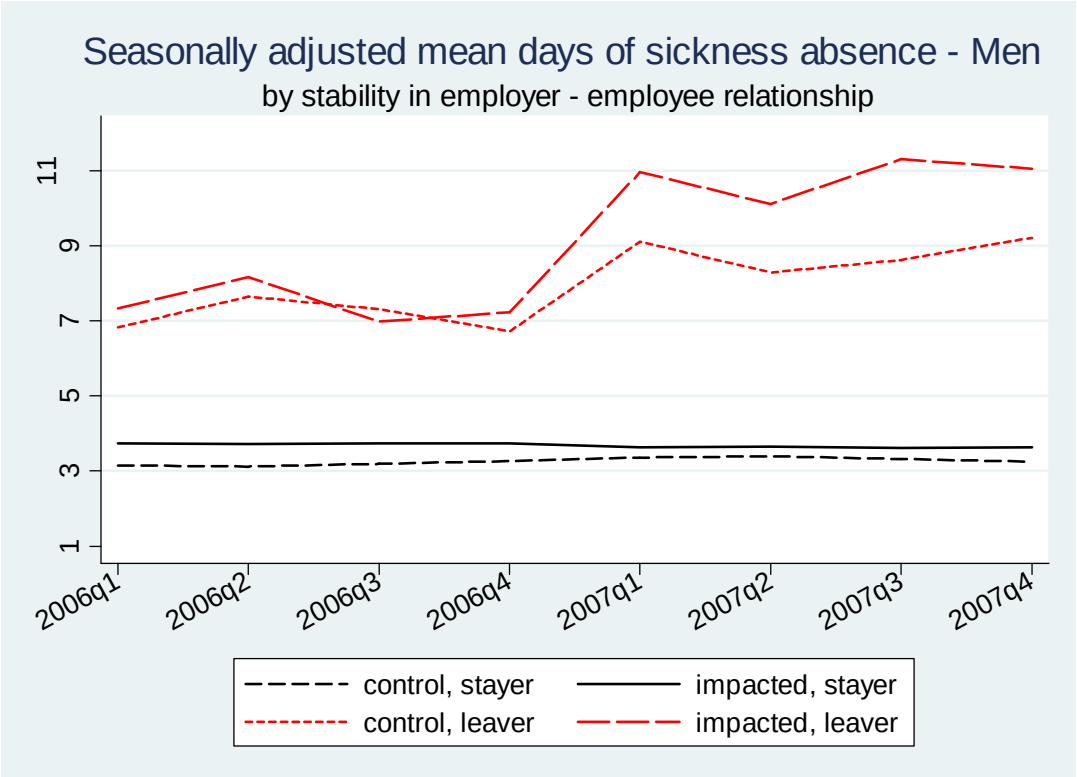

Figure 8. Pre-shock level of sickness absence among men, by stability in employer-employee relation, period, and treatment status.

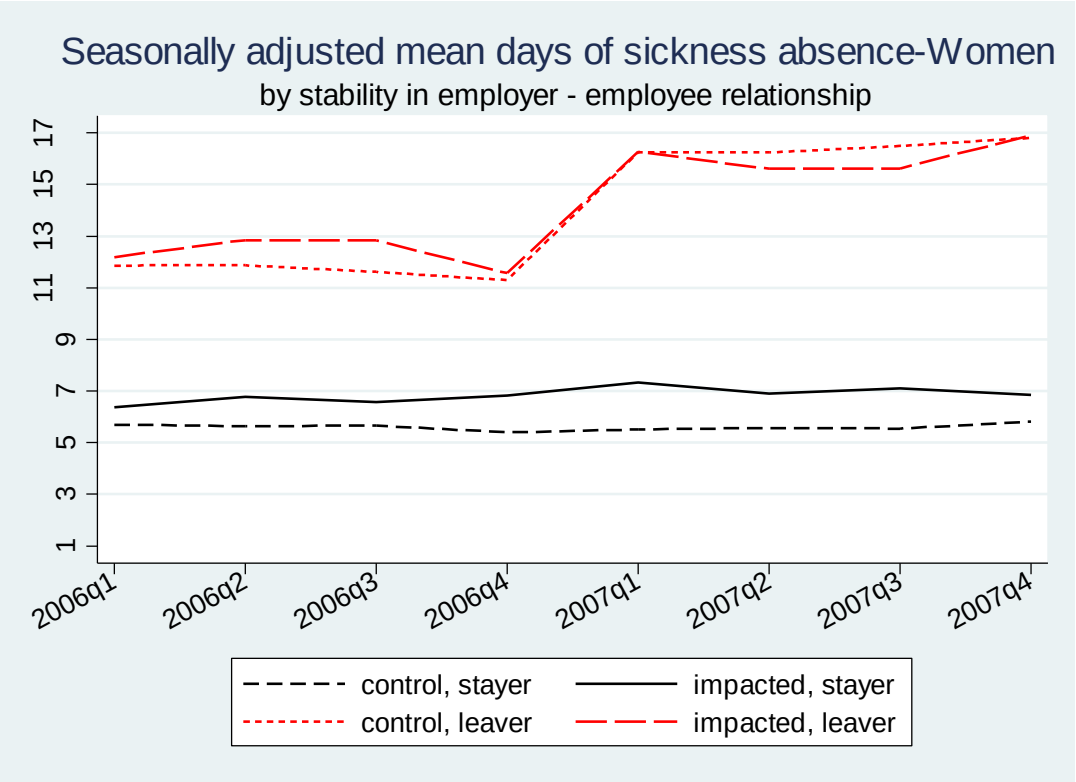

Figure 9. Pre-shock level of sickness absence among women, by stability in employer-employee relation, period, and treatment status.

Table 1a Descriptive statistics

	Control group		Impacted by shock	
	pre shock	post shock	pre shock	post shock
Days of sickness absence	5.869	6.305	6.625	6.639
Incidence of sickness absence	0.070	0.068	0.086	0.080
1 if same employer 1 Jan 2006–31 Dec 2008	0.724	0.787	0.699	0.771
1 if female	0.798	0.802	0.789	0.795
Year of birth	1960.6	1960.6	1961.4	1961.4
1 if information on education is missing	0.020	0.017	0.018	0.015
1 if 10 years of schooling or less	0.103	0.103	0.103	0.102
1 if 11-13 years of schooling	0.443	0.448	0.441	0.443
1 if 14-16 years of schooling	0.214	0.212	0.215	0.215
1 if 17 years of education or more	0.221	0.220	0.223	0.225
Number of children less than 15 years of age by period t	0.701	0.689	0.720	0.719
1 if never married by period t	0.251	0.236	0.268	0.251
1 if married by period t	0.607	0.615	0.579	0.589
1 if separated, divorced or widowed by period t	0.143	0.149	0.153	0.160
1 if no sickness absence spells 1992–2008	0.191	0.193	0.165	0.163
No. of sickness absence spells 1992–2008	3.648	3.613	4.202	4.173
N	507647	233491	56808	25728
Change in days post/pre shock		*0.435*		*0.013*
Difference in change in days, impacted vs control group				***-0.422***
Change of incidence post/pre shock		*-0.001*		*-0.006*
Difference in change of incidence, impacted vs control group				***-0.004***

Notes: "Pre shock" is periods within the years 2006 and 2007, while "post shock" is 2008.

Table 1b Descriptive statistics by gender

	Men				Women			
	control group		impacted		control group		impacted	
	pre	post	pre	post	pre	post	pre	post
Days of sickness absence	3.695	3.935	4.353	3.678	6.421	6.891	7.234	7.403
Incidence of sickness absence	0.043	0.042	0.056	0.047	0.076	0.075	0.094	0.089
1 if same employer 1 Jan 2006–31 Dec 2008	0.737	0.818	0.712	0.810	0.721	0.779	0.695	0.762
1 if female								
Year of birth	1958.8	1958.9	1960.2	1960.1	1961.0	1961.0	1961.8	1961.7
1 if information on education is missing	0.022	0.017	0.018	0.015	0.019	0.017	0.018	0.016
1 if 10 years of schooling or less	0.085	0.086	0.086	0.083	0.108	0.107	0.108	0.107
1 if 11-13 years of schooling	0.336	0.339	0.361	0.357	0.470	0.475	0.462	0.465
1 if 14-16 years of schooling	0.210	0.207	0.196	0.202	0.215	0.213	0.220	0.218
1 if 17 years of education or more	0.348	0.350	0.339	0.343	0.188	0.187	0.192	0.194
Number of children less than 15 years of age by period t	0.658	0.656	0.684	0.683	0.712	0.697	0.729	0.729
1 if never married by period t	0.263	0.245	0.307	0.280	0.248	0.233	0.257	0.244
1 if married by period t	0.626	0.637	0.568	0.589	0.602	0.610	0.582	0.588
1 if separated, divorced or widowed by period t	0.111	0.118	0.125	0.131	0.151	0.157	0.161	0.168
1 if no sickness absence spells 1992–2008	0.333	0.335	0.303	0.298	0.155	0.158	0.128	0.128
No. of sickness absence spells 1992–2008	2.275	2.259	2.711	2.689	3.996	3.947	4.601	4.555
N	102780	46321	11995	5277	404867	187170	44813	20451
Change in days post/pre shock		*0.240*		*-0.675*		*0.470*		*0.169*
Difference in change in days, impacted vs control group				***-0.915***				***-0.301***
Change of incidence post/pre shock		*-0.001*		*-0.009*		*-0.002*		*-0.005*
Difference in change of incidence, impacted vs control group				***-0.008***				***-0.004***

Table 2 Effect of the financial shock on sickness absence in the public sector. OLS and fixed effects

	Men		Women	
	OLS	FE	OLS	FE
Days	-1.021**	-0.994***	-0.513**	-0.740***
	(-2.31)	(-2.80)	(-2.19)	(-3.37)
Incidence	-0.012**	-0.011**	-0.006	-0.007
	(-2.20)	(-2.22)	(-0.71)	(-0.78)
N	166373	166373	657301	657301
Background variables	Yes	Yes	Yes	Yes
Dummies for period and quarter	Yes	Yes	Yes	Yes
Extra trend for impacted employers	Yes	Yes	Yes	Yes
Individual fixed effect	No	Yes	No	Yes

t statistics clustered at the municipality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Columns labeled (1) and (2) show results from OLS and the fixed-effects estimator, respectively. Background variables include education, a polynominal of age, marital status, and number of children.

Table 3. Effect of the financial shock on sickness absence in the private sector. Fixed effects.

	Days of absence		Incidence	
	Men	Women	Men	Women
A. Effect	0.085	0.063	-0.002	-0.004**
	(0.69)	(0.27)	(-0.94)	(-2.58)
B. Private vs public sector	1.079***	0.803***	0.009	0.002
	(2.78)	(2.61)	(1.56)	(0.26)
N	1051551	591222	1051551	591222

t statistics clustered at the municipality level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Controls as in Table 2.
Estimated on two different samples: A. Private sector employees only, B. A pooled sample of municipal and private sector employees. In the latter case, the coefficient of the interaction term private*treatment is reported.

Table 4 Placebo shock

	Post-shock periods are periods 5–8				Post-shock periods are periods 5–12			
	Men		Women		Men		Women	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE
***a) Days of absence**:*								
Impacted	-0.113	-0.106	-0.405	-0.318				
	(-0.22)	(-0.22)	(-1.23)	(-1.18)				
Impacted					0.712	0.678	0.335	0.581**
					(0.87)	(0.99)	(1.32)	(2.15)
b) Incidence:								
Impacted	0.004	0.002	-0.006	-0.006				
	(0.40)	(0.17)	(-0.88)	(-0.90)				
Impacted					0.008	0.006	0.002	0.003
					(0.81)	(0.65)	(0.66)	(0.79)

Shock attributed to period 4, post shock periods are either periods 5–8 or 5–12.
t statistics clustered at the municipality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5 Omitting municipalities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
a) Days of absence:									
Men:									
Impacted, OLS	-1.154***	-1.193**	-1.092**	-1.057**	-1.175**	-1.011**	-0.762*	-0.711	-1.817***
	(-2.61)	(-2.28)	(-2.43)	(-2.32)	(-2.30)	(-2.24)	(-1.88)	(-1.55)	(-3.53)
Impacted, FE	-1.105***	-1.127***	-1.049***	-1.047***	-1.029**	-0.994***	-0.748**	-0.846**	-1.491***
	(-3.13)	(-2.71)	(-2.90)	(-2.89)	(-2.40)	(-2.74)	(-2.52)	(-1.98)	(-2.88)
Women:									
Impacted, OLS	-0.490**	-0.687***	-0.483**	-0.509**	-0.615**	-0.539**	-0.382**	-0.460*	-0.904***
	(-2.00)	(-2.76)	(-2.04)	(-2.08)	(-2.37)	(-2.25)	(-2.00)	(-1.71)	(-3.16)
Impacted, FE	-0.726***	-0.921***	-0.721***	-0.752***	-0.769***	-0.752***	-0.629***	-0.685***	-1.063***
	(-3.16)	(-4.51)	(-3.23)	(-3.27)	(-2.96)	(-3.36)	(-3.32)	(-2.75)	(-4.25)
b) Incidence:									
Men:									
Impacted, OLS	-0.014***	-0.011	-0.013**	-0.012**	-0.015***	-0.012**	-0.011*	-0.007	-0.019***
	(-2.74)	(-1.57)	(-2.48)	(-2.11)	(-2.71)	(-2.16)	(-1.84)	(-1.34)	(-3.68)
Impacted, FE	-0.013***	-0.009	-0.012**	-0.011**	-0.013***	-0.011**	-0.010*	-0.007	-0.016***
	(-2.91)	(-1.46)	(-2.53)	(-2.21)	(-2.71)	(-2.17)	(-1.85)	(-1.30)	(-3.38)
Women:									
Impacted, OLS	-0.008	-0.014**	-0.007	-0.005	-0.002	-0.007	-0.006	0.000	-0.015**
	(-0.90)	(-2.15)	(-0.75)	(-0.61)	(-0.20)	(-0.78)	(-0.65)	(0.04)	(-2.20)
Impacted, FE	-0.009	-0.014**	-0.007	-0.006	-0.002	-0.007	-0.007	-0.000	-0.015**
	(-1.00)	(-2.06)	(-0.81)	(-0.70)	(-0.23)	(-0.83)	(-0.74)	(-0.00)	(-2.09)
Municipality excluded:	Kvinesdal	Haugesund	Vik	Bremanger	Narvik	Hattfjelldal	Hemnes	Rana	Kvinesdal, Haugesund, and Narvik

t statistics clustered at the municipality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6 Effect on subsamples of stable workers

	(1)	(2)	(3)	Main results
Days of absence				
Impacted, both genders	-0.648***	-0.658***	-0.794**	-0.780***
Impacted, men	-0.609	-0.535	-0.57	-0.994***
Impacted, women	-0.658***	-0.697**	-0.857**	-0.740***
Incidence				
Impacted, both genders	-0.002	-0.004	-0.005	-0.007
Impacted, men	-0.010**	-0.011**	-0.011***	-0.011**
Impacted, women	0.000	-0.003	-0.004	-0.007

1) Employed 2006q1-2008q4 in same municipality as Dec 31, 2006
2) Employed 2007q1-2008q4 in same municipality as Dec 31, 2006
3) Employed 2008q1-2008q4 in same municipality as Dec 31, 2006
t statistics clustered at the municipality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

APPENDIX

Table A1 Sample selection

	No. of individuals
Employed in municipal sector by Dec 31 2006	370834
Dropped because employed in several municipalities with different treatment status	-2787
Dropped because employed in several treated municipalities	-3
Dropped because of age >66 in 2006	-744
Dropped because reason for sickness absence registered is something else than employee's own sickness	-5427
Dropped because is outlier, > 20 sickness absence episodes 1992–31.12.2008	-1919
Dropped because employed both within and outside of municipal sector	-22889
Dropped for other reasons	-444
Data set for analysis	336621
From this data set we extract:	
Treatment group: employees of municipalities impacted by shock	7985
Control group: employees of other municipalities that own hydroelectric power	69951

Table A2 Municipalities affected by the financial shock

	Municipality	No. of inhabitants, Dec 2006	**Gross expenditure per capita, 2006, NOK 1000**	**Recorded loss per capita, municipal accounts, NOK 1000**		
				2007	2008	2009
1	Kvinesdal	5595	59	2.8	1.3	0.0
2	Haugesund	32303	45	3.0	1.1	-0.7
3	Vik	2835	64	23.8	12.5	0.0
4	Bremanger	3930	63	46.2	14.9	0.0
5	Narvik	18301	54	6.4	3.9	0.0
6	Hattfjelldal	1482	71	86.8	-27.0	0.0
7	Hemnes	4510	66	22.4	-7.0	0.0
8	Rana	25190	47	10.1	-1.3	0.0

Note: Data on recorded loss has been given by municipal administrations. The per capita loss is computed using the number of inhabitants in 2006.

Table A3. Occupational sector by gender, percent

	Men		Women	
	Not impacted	Impacted	Not impacted	Impacted
Technical personnel	8	8	0	0
Administration	28	24	11	10
Fire brigade	3	7	0	0
Teaching (compulsory school)	28	26	20	20
Health care (primary care and nursing homes)	8	10	29	32
Home care services, kindergartens	14	15	37	34
Other services	11	11	3	4
In total	100	100	100	100
N	*14042*	*1657*	*55909*	*6328*

Table A4. Private sector employees, descriptive statistics by gender. Means

	Men				Women			
	control group		impacted		control group		impacted	
	pre	post	pre	post	pre	post	pre	post
Days of sickness absence	3.512	3.568	3.132	3.561	5.229	5.429	5.140	5.440
Incidence of sickness absence	0.039	0.038	0.040	0.040	0.054	0.053	0.058	0.057
1 if same employer 1 Jan 2006-31 Dec 2008, otherwise 0	0.707	0.802	0.695	0.766	0.694	0.802	0.674	0.773
Year of birth	1965.4	1965.2	1965.5	1965.5	1965.9	1965.3	1966.7	1966.2
1 if information on education is missing	0.039	0.036	0.039	0.037	0.040	0.037	0.035	0.033
1 if 10 years of schooling or less	0.215	0.214	0.169	0.171	0.237	0.236	0.226	0.229
1 if 11-13 years of schooling	0.608	0.614	0.618	0.621	0.542	0.548	0.560	0.563
1 if 14-16 years of schooling	0.098	0.096	0.123	0.121	0.138	0.136	0.136	0.134
1 if 17 years of education or more	0.041	0.040	0.051	0.050	0.043	0.042	0.043	0.040
Number of children less than 15 years of age by period t	0.670	0.689	0.686	0.711	0.669	0.683	0.684	0.696
1 if never married by period t, otherwise 0	0.452	0.427	0.429	0.405	0.385	0.355	0.376	0.345
1 if married by period t, otherwise 0	0.451	0.470	0.475	0.494	0.483	0.506	0.492	0.515
1 if separated, divorced or widowed by period t, otherwise 0	0.097	0.103	0.096	0.101	0.131	0.139	0.132	0.140
1 if no sickness absence spells 1992-2008	0.371	0.374	0.375	0.377	0.276	0.273	0.279	0.277
No. of sickness absence spells 1992-2008	2.068	2.036	2.087	2.068	2.700	2.691	2.760	2.762
N	629696	277476	99318	45061	355535	153882	56980	24825
Change in days post/pre shock		*0.056*		*0.429*		*0.200*		*0.300*
Difference in change in days, impacted vs control group				*0.373*				*0.100*
Change of incidence post/pre shock		*-0.001*		*0.000*		*-0.001*		*-0.001*
Difference in change of incidence, impacted vs control group				*0.001*				*0.000*

Department of Economics

University of Bergen
Fosswinckels gate 14
N-5007 Bergen, Norway
Phone: +47 55 58 92 00
Telefax: +47 55 58 92 10
http://www.svf.uib.no/econ