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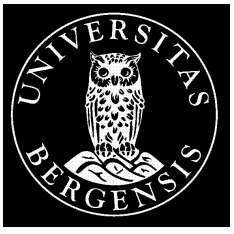
WORRIED SICK? WORKER RESPONSES TO ORGANIZATIONAL TURMOIL



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Worried Sick?

Worker Responses to Organizational Turmoil

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Abstract:
Sickness absence has risen over the past years in Norway. One explanation put forward is that a tougher labor market represents a health hazard, while a competing hypothesis predicts that loss of job security works as a disciplinary device. In this analysis we aim to trace a causal impact of organizational turmoil or job insecurity on sickness absence, applying a difference-in-difference approach. Utilizing a negative financial shock that hit specific employers and workplaces, we find that sickness absence decreased considerably in the following year. The decrease is substantially larger among male than among female employees, and stronger for days of sickness absence than for its incidence.

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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 financial loss led to massive negative coverage in national and even international mass 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 ashamed. The quote saying "The people in City Hall were naïve and they were manipulated," appears to be representative of public opinion at the time (*New York Times*, December 2, 2007). 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, three of the Chief Executive leaders had had to leave their jobs because of the losses, and two years later, none of them 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 a longer period, a maximum of ten instead of four years (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 a loss of 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. For the common public 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 organizational turmoil and economic uncertainty at the employer level. One aspect of such behavior is sickness absenteeism. Job-related shocks may influence sickness absence by affecting health but also by affecting incentives to report sick. Even though institutional arrangements vary, the sickness absence level has been a much-debated problem causing worries in several European countries.

High absence rates 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). A popular explanation is that the labor market has become tougher. There is a growing interest in the role of the workplace, with key words being 'downsizing', 'organizational change', 'employee turnover' and 'local social norms'. 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

organizational turmoil on sickness absence, using as a source of exogenous variation the financial shock in 2007–2008 to a specific group of local governments. We make no sharp distinction between organizational turmoil and job insecurity. As a job is defined by a bundle of attributes, income being only one of them, we use the term "job insecurity" in a broader context than simply fear of unemployment and income loss. 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.

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. There are also some 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. 2007; 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 absences lasting more than three days. For sickness 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 2011 the absence rate was 5.8%, still above the aim in 2001.

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.

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 in 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, such as we have, trends may be incorporated in the model. We estimate the DID effect from a regression model for individuals i in periods $t = 1,\ldots, T$. Let FS_{it} be an indicator for working in one of the municipalities that were exposed to the financial shock, $POST_t$ a dummy variable

which equals 1 in periods after the shock, and D_t, $t = 1,\ldots,T$ period dummies. The regression equation, estimated on quarterly data, 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 we have also included quarter dummies D_q to control for seasonal variation and a vector X_{it} of individual characteristics. 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 (1) 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 on basis of 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 attitudes are differenced out of the model. The same argument applies to differences in individual health endowment.

5. Data

The key data source is administrative registers from Statistics Norway which comprise the whole 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 and their employers by December 31, 2006, about a year prior to the financial shock. In order to purify the relation between employer and sickness absence, we exclude individuals who also held a job outside the municipal sector or who had employment in several municipalities of different treatment status, or in several treated municipalities. Furthermore, employees above the age of 66 are excluded.

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. We have information on all sickness absence episodes 1992–2008, but for the purpose of this paper, 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 employees within municipalities which, like treatment group employers, gain 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 a number of respects: 1–4 are situated in Southern or Western Norway, whereas 5–8 are located within the same county in Northern Norway, see figure 2.

[Figure 2 (map)]

Municipalities 2, 5, and 8 are middle-sized towns by Norwegian standards, whereas the rest are thinly settled. The one thing they have in common is that they invested heavily in complicated financial products. These investments were made possible by their expected income from hydroelectric energy production. Municipalities who have a share in this natural resource are typically affluent. Still, the financial loss 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 3 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 3]

We estimate 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 appears to be quite a 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.

[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.

The main impression this far is that sickness absence was reduced in the affected municipalities, but most clearly for men and more distinctly for absence days than for incidence. In the next section we investigate whether this impression holds in an econometric 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.

[Table 2 Effect on number of days of sickness absence]

The post-shock effect is statistically significant and larger for men than for women, as expected from 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.

[Table 3 Effect on incidence]

As 'incidence' is a discrete outcome, the coefficients are interpreted as marginal effects on the probability of absence.[1] Here, the estimated effect is statistically significant only for men: -0.011 or a decrease of 1.1 percentage points with FE. Again, the effect is considerable, implying a 20% reduction from about 0.05. The point estimate for women is larger than the according number in Table 1B, but far from any acceptable level of statistical significance.

The evidence this far is quite clear that the length of absence spells was reduced by the shock, less clear that the probability of absence was affected. This is reasonable. 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

[1] 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.

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 different job characteristics, health differences or different attitudes.[2]

[Table 4 Placebo shock]

As a check for robustness, table 4 shows results for regressions where we have redefined the POST-dummy to equal one from Q1, 2007 onwards. This is almost one year before the crisis, and there were no other particular events that should 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 4 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 actual results in tables 2 and 3.

[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 loss and level of expenditure 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 in tables 2 and 3, the reduced samples produce quite similar results for absence days and

[2] 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.

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 loss recorded was smallest (in Kvinesdal, Haugesund and Narvik, as seen from table A2), the estimated effects generally increase in magnitude.

[Figure 5 Change in employment status]

In each period, we include in the estimated sample only workers who remain with the employer that they had one year prior to the shock. It could be the case that the negative effect of the shock is driven by high-absence workers shifting employer or leaving the labor force. However, figure 5 shows that the proportion of employees who left their employer temporarily or permanently is very similar in the treatment and control group; there are no sharp shifts in trends. A potential reaction to perceived increased job insecurity could be to look for other jobs, but figure 5 does not support that hypothesis. Furthermore, when estimating a stable-workers sample (consisting only of workers who stayed throughout the whole sample period) we get very similar results to those in tables 2 and 3 (not reported). Thus, the drop in absence is driven by workers who stay with the same employer and cannot be explained by changes in the composition of workers.

Our main conclusion is that the financial shock reduced sickness absence among employees in the municipalities that were affected, that the effect was stronger for men than for women, and clearer for the length than the incidence of absence. 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. However, that is not supported by the observed turnover rates. 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 10–20%. 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 must be termed 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.

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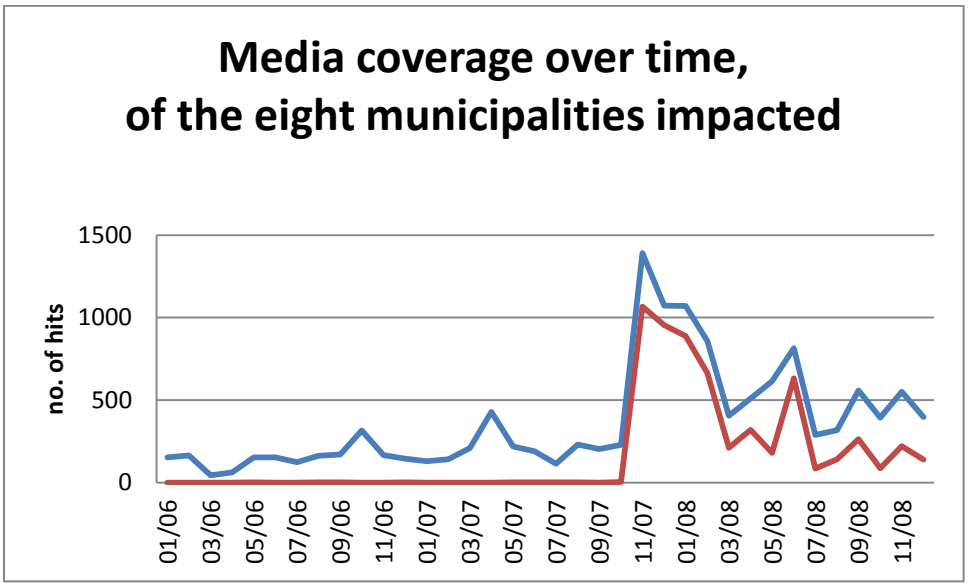

Figure 1. The X-axis shows the number of hits across Norwegian media; printed and/or web-based newspapers, periodicals, radio and television. Numbers are counted per month during the years 2006–2008. The blue line shows hits that include at least one of the names of the municipalities impacted, without 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/

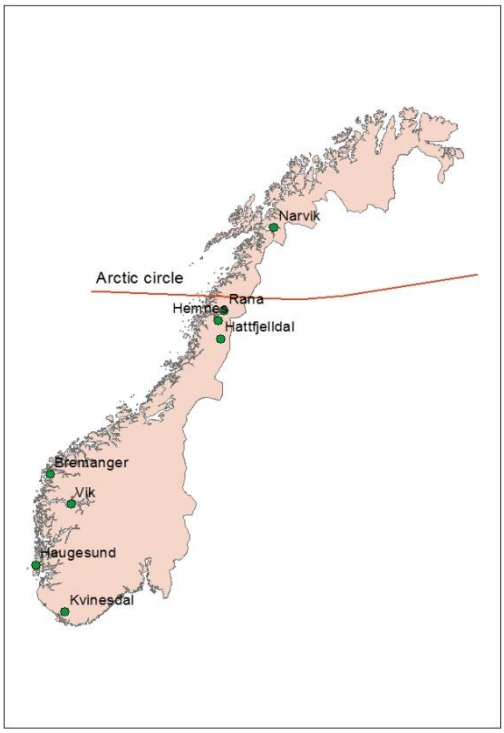

Figure 2. The location of the eight municipalities impacted.

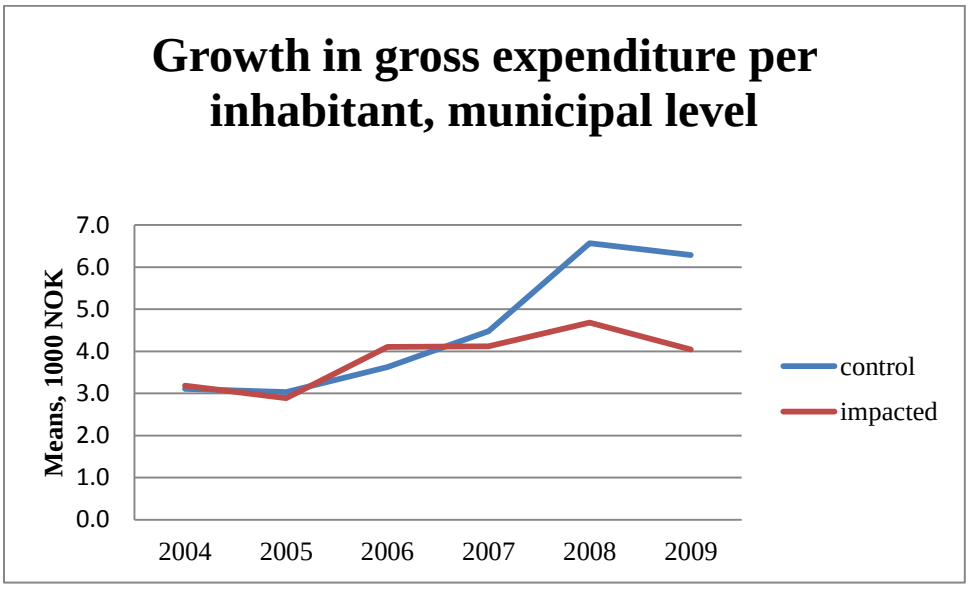

Figure 3. Growth in gross expenditure per inhabitant measured in 1000 Norwegian kroner, nominal terms. Means are taken across municipalities in the control group and the group of impacted municipalies, respectively.

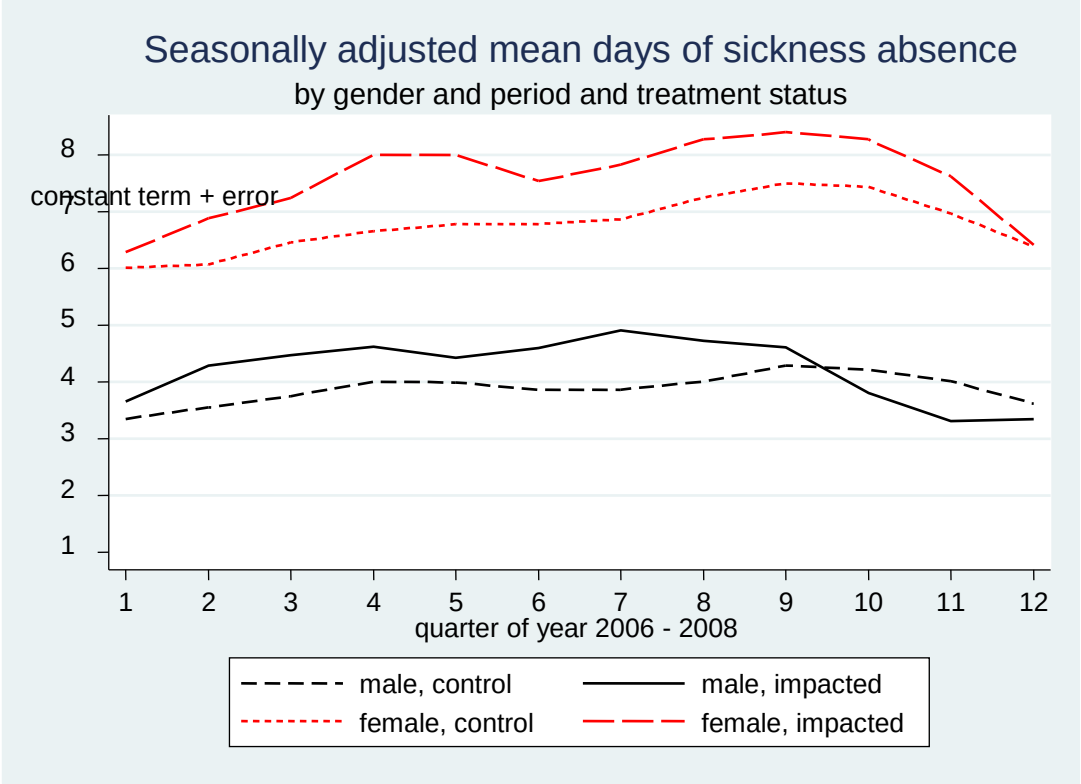

Figure 4 Sickness absence 2006–2008. Mean number of days

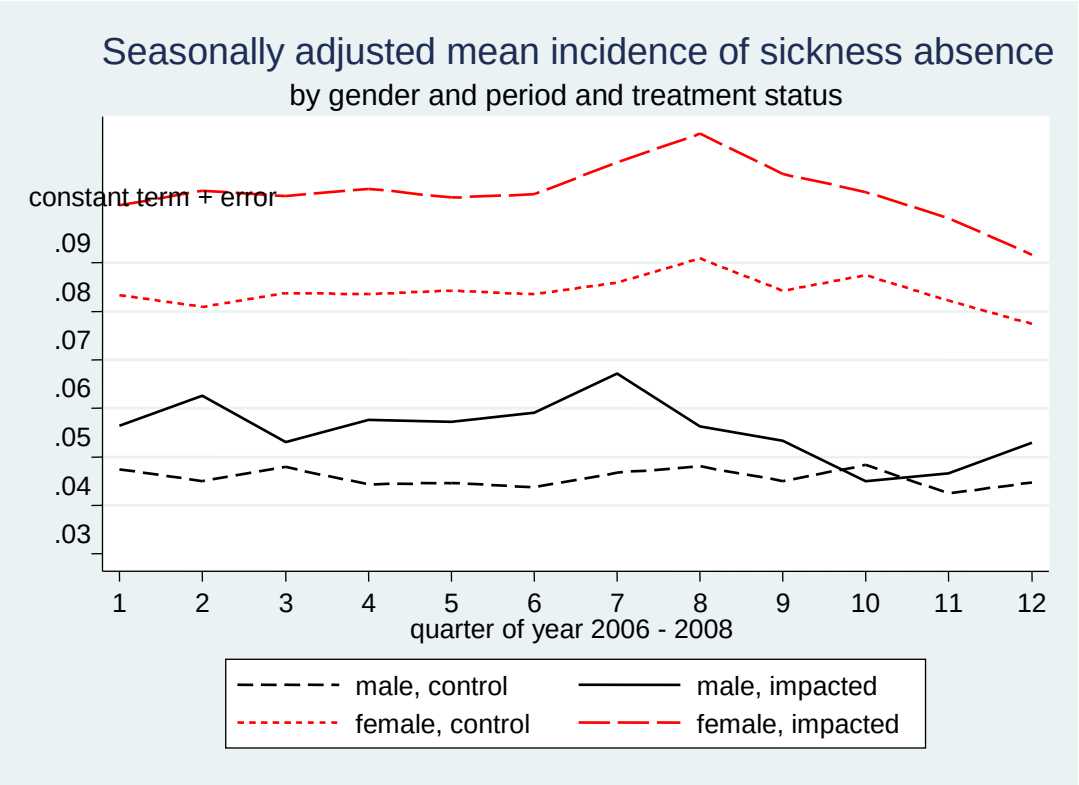

Figure 5 Sickness absence 2006–2008. Mean incidence

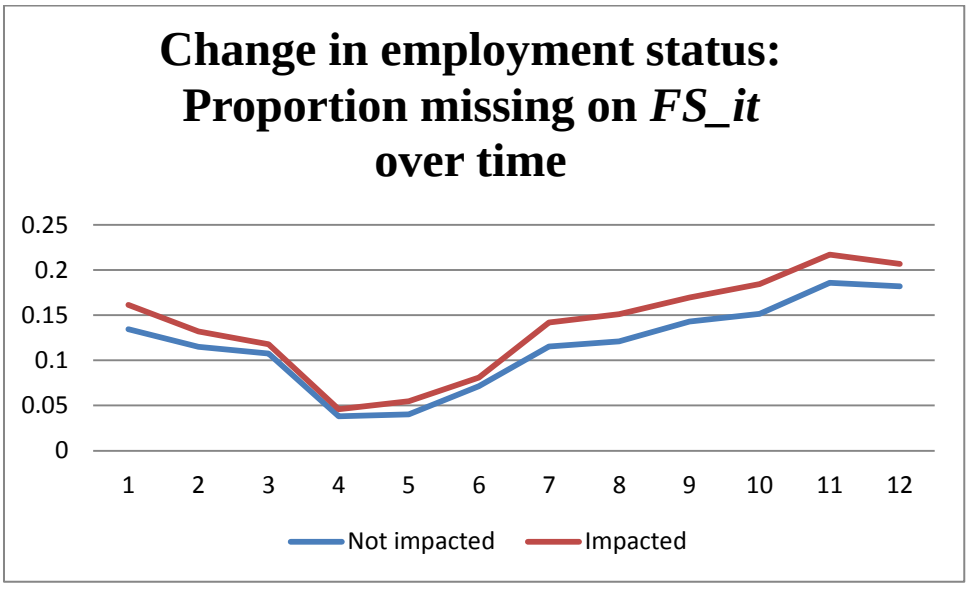

Figure 6. Change in employment status among employees in the estimated sample.

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 on days of sickness absence

	Men		Women	
	(1)	(2)	(1)	(2)
Impacted	-1.021**	-0.994***	-0.513**	-0.740***
	(-2.31)	(-2.80)	(-2.19)	(-3.37)
Impacted employer	0.706	.	0.715	.
	(0.81)	.	(0.96)	.
2. quarter of year	0.319***	0.351***	1.691***	0.279***
	(3.21)	(3.55)	(9.61)	(3.30)
3. quarter of year	0.021	1.054***	-0.968***	-0.904***
	(0.12)	(5.15)	(-8.73)	(-8.17)
4. quarter of year	0.353*	1.486***	1.181***	2.160***
	(1.68)	(7.19)	(7.33)	(13.23)
Background variables	Yes	Yes	Yes	Yes
Dummies for period	Yes	Yes	Yes	Yes
Extra trend for impacted employers	Yes	Yes	Yes	Yes
Individual fixed effect	No	Yes	No	Yes

t statistics 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. Standard errors are robust to heteroskedasticity and clustered at the employer level. "Impacted employer" equals 1 for an individual who holds a job in a municipality that was hit by the financial shock, and "impacted" equals 1 for such an individual in a post-shock period, otherwise 0. Background variables include education, a polynominal of age, marital status, and number of children.

Table 3 Effect on incidence of sickness absence

	Men		Women	
	(1)	(2)	(1)	(2)
Impacted	-0.012**	-0.011**	-0.006	-0.007
	(-2.20)	(-2.22)	(-0.71)	(-0.78)
Impacted employer	0.010	.	0.016***	.
	(1.45)	.	(3.02)	.
2. quarter of year	-0.006***	-0.006**	-0.007***	-0.013***
	(-2.70)	(-2.59)	(-4.17)	(-7.70)
3. quarter of year	-0.007***	-0.008***	-0.022***	-0.022***
	(-3.13)	(-3.15)	(-13.41)	(-13.44)
4. quarter of year	-0.003	0.003	0.008***	0.013***
	(-1.08)	(1.33)	(4.03)	(6.27)
Background variables	Yes	Yes	Yes	Yes
Dummies for period	Yes	Yes	Yes	Yes
Extra trend for impacted employers	Yes	Yes	Yes	Yes
Individual fixed effect	No	Yes	No	Yes

t statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
See notes to table 2

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 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:									
Treat, 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)
Treat, 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:									
Treat, 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)
Treat, 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:									
Treat, 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)
Treat, 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:									
Treat, 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)
Treat	-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 in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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. Employment 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*