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Worried sick? Worker responses to a financial shock

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HIGHLIGHTS

- A financial shock to specific public sector employers implied loss of job security.
- In our diff-in-diff design, the control group is non-affected public sector workers.
- The results are robust to a number of robustness checks.
- Sickness absence among affected employees decreased considerably after the shock.
- Reduced job security appears to have a disciplining effect.

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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. In addition, trends in sickness absence of public and private sector employees are compared, in a triple 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.

1. Introduction

When the United States housing bubble of the early 2000s finally burst in 2007, its worldwide repercussions ultimately triggered the financial crisis of 2008–09. A somewhat surprising example of investors facing heavy losses at this time was a group of eight municipalities in energy-rich Norway, which turned out to have invested expected future earnings from hydro-electric power plants in high-risk financial products. These affected municipalities were obliged to cut running expenses at short notice, and the so-called 'Terra crisis' named after Terra Securities, the brokerage house that sold the financial products, soon led to fears of job losses, and activated public employee unions. The unexpected nature of this particular financial shock provides 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.

After the first disclosure in a financial daily newspaper on 31 October 2007, massive media coverage followed, as shown in Fig. 1. Typical statements were that this would harm service production in the municipalities involved, that this severely damaged their general reputation, and that their inhabitants felt embarrassed. An investigation followed, and in January 2008, the County Governor's office concluded that the investments had not been in accordance with the Norwegian Local Government Act.

When the news broke, it was clear that the losses would be of considerable magnitude, and the complexity of the financial products involved added to this uncertainty. This was especially so given that local governments employ about 20% of the Norwegian work-force, wages represent most municipal expenses, and thus jobs could be at risk in the affected communities. Pertinently, the Norwegian central government did not offer a bailout for the affected municipalities, but instead proposed an amendment in the Local Government Act allowing them to cover any losses over a period of up to 10 years instead of four, as previously stated. The Norwegian Parliament passed this amendment in June 2008, only two months after being proposed and without the customary hearings. However, the option provided was generally unattractive because it implied less municipal autonomy in economic matters.

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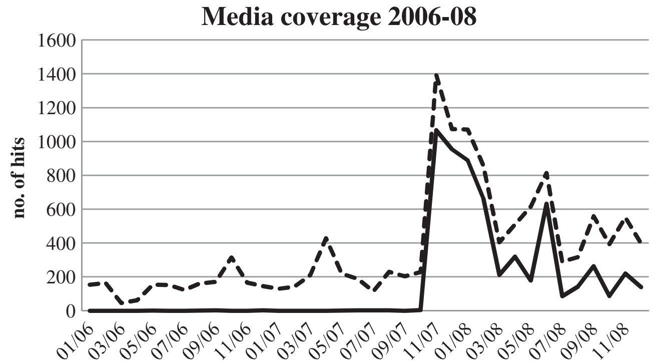

Fig. 1. Hits per month in Norwegian media database covering printed and web-based newspapers, periodicals, radio, and television. Dotted line: at least one of the affected municipalities mentioned. Solid line: dotted line requirement plus 'Terra'.
Source: http://ret-web05.int.retriever.no/services/.

For the typical municipal employee, the crisis came as a shock in the truest sense of the word (Hofstad, 2008). When the extent of the financial problems was revealed, union leaders representing most municipal workers soon expressed concern that the cuts in expenses would affect job security and working conditions. Thus, the Norwegian Terra crisis is well suited to a case study of how economic uncertainty at the employer level affects worker behaviour. In this paper, we focus on sickness absenteeism, a much-debated problem causing concerns in several European countries. In general, job-related shocks may influence sickness absence by affecting health, e.g., by creating emotional stress. However, shocks may also affect the incentives to report sick if the worker believes that there is a risk of down-sizing, and that absenteeism will then affect the worker's personal risk of losing a job or threaten career opportunities as the competition for attractive jobs becomes tougher. These two channels are clearly not mutually exclusive, but we can shed light on which channel dominates.

High absence rates may also account for large production losses and strain public finances, with a number of different explanations proposed: for a recent summary of the Norwegian case, see Markussen et al. (2011). In this analysis, we aim to trace the causal impact of job security on sickness absence exploiting the 2007–08 financial shock to a specific group of local governments as a source of exogenous variation. We argue that the financial problems experienced by these local governments may have led to a reduction in perceived job security by municipal workers, which in turn affected sick absence behaviour, even though there were no consequences for sickness compensation schemes. Overall, we find evidence that average absence days decreased after the shock by about 10% for female employees in the affected municipalities. For male employees, we identify effects of an even larger magnitude, but cannot discount that these may have resulted from changes in the composition of male employees at the time.

2. Related literature

Obviously, there is a strong health component in sickness absence, but the opportunity cost of reporting sick also affects sick absence. There is an extensive empirical literature that relates absence to economic incentives, including Allen (1981), Dunn and Youngblood (1986), Kenyon and Dawkins (1989), Barmby et al. (1991), Johansson and Palme (2005), and Puhani and Soderlof (2010). For instance, Johansson and Palme (1996) find that a Swedish reform that made absence more costly for workers served to reduce sickness absence. It is of note that, for all practical purposes, Norwegian sickness insurance involves 100% income replacement.

A number of existing studies also relate sickness absence to the unemployment rate. For example, using a standard labour supply model, Leigh (1985) shows that if an increase in the unemployment rate increases the perceived risk of job loss by workers, and a poor attendance record likewise increases the probability of being fired, then under reasonable assumptions, an increase in the unemployment rate reduces absenteeism. An analysis of US Panel Study of Income Dynamics data supports this hypothesis. An alternative explanation to the observed negative association between sickness absence and the unemployment rate in some countries is that labour force composition varies over the business cycle as labour demand increases and decreases, pushing less healthy workers out of the labour market in market down-turns. However, it is difficult to find compelling evidence that composition alone explains procyclical absence variation.

For instance, Arai and Thoursie (2005) examine Swedish industry-regional panel data and identify a negative relation between the share of temporary contracts and the sick rate. They interpret this as evidence that workers on temporary contracts have weaker incentives to report sick, and thus the behaviour of marginal workers cannot explain fluctuations in the rate of absence. Askildsen et al. (2005) also find a negative effect of the local unemployment rate on sickness absence in a panel study of Norwegian data, including in a subsample of stable workers. More 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; Ichino and Maggi, 2000; Bamberger and Biron, 2007; Rege et al., 2012). A strand of contributions in this body of work aims to identify social interaction effects (Bradley et al., 2007; Hesselius et al., 2009; Lindbeck et al., 2009). One such interaction is 'learning', in the sense that workers in the same firm display similar absence behaviour. Another is the reciprocity between employer and employee: if the employer treats the workers well, they may respond by having fewer absences, whereas worsened conditions for workers may induce their increased absence to retaliate or 'get back at' the employer (Fehr and Gächter, 2000).

The proposed behavioural link between the unemployment rate and sickness absence is that a rise in unemployment affects the worker's risk of job loss and thus 'disciplines' the worker to have fewer absences. If so, we may argue, at least from the worker's perspective, that a negative shock to the employer may have a similar effect if the worker believes the shock implies the threat of organizational down-sizing. Adding to the threat of losing a job are firm reorganizations that may also affect workers. The literature therefore presents two competing hypotheses for analysing the financial troubles of the affected Norwegian municipalities in 2007–08. Both are relevant in a situation where employees are concerned about their future, whether they believe that there is a (greater) risk of job loss, or worry about an unfavourable change in their working requirements. The first hypothesis asserts that less secure jobs will encourage workers to avoid absenteeism for the reasons suggested, as supported by the aforementioned study by Arai and Thoursie (2005). Elsewhere, Ichino and Riphahn (2005) examine absence around a probationary period without restrictions on firing workers, and find that when employment protection increases after the probation period, absenteeism also increases. Lindbeck et al. (2006) obtain similar results in the analysis of a change in Swedish legislation that reduced job security. However, in a large-scale Norwegian study by Markussen et al. (2011), there was no consistent evidence that short-tenured workers have fewer certified absences than do more secure workers.

The second competing hypothesis is that the insecurity and worry associated with reorganization may themselves be a health hazard, as indicated in the well-known Whitehall II studies (Ferrie et al., 1995, 1998a, 1998b). These longitudinal cohort studies examine the health effects of the work environment among British white-collar civil servants. The results indicate that employees threatened with or experiencing early privatization or reorganization suffered deteriorating health when compared with a control group, without significant changes in health behaviour (e.g., smoking, alcohol use, etc.). In other work, Røed and Fevang (2007) use register data and conclude that

Table 1
Background characteristics.

Variables	Work-place in treated municipality		Work-place in non-treated municipality	
	Municipal sector	Private sector	Municipal sector	Private sector
1 if female; otherwise 0	0.792	0.367	0.799	0.362
Year of birth; otherwise 0	1962.3	1966.7	1961.3	1966.3
1 if information on education missing; otherwise 0	0.020	0.040	0.022	0.042
1 if 10 years of schooling or less; otherwise 0	0.108	0.196	0.106	0.228
1 if 11–13 years of schooling; otherwise 0	0.436	0.587	0.437	0.575
1 if 14–16 years of schooling; otherwise 0	0.218	0.128	0.217	0.113
1 if 17 years of education or more; otherwise 0	0.218	0.048	0.218	0.042
Number of children less than 15 years of age; otherwise 0	0.734	0.677	0.715	0.662
1 if never married by period t; otherwise 0	0.288	0.428	0.269	0.445
1 if married by period t; otherwise 0	0.561	0.465	0.591	0.447
1 if separated, divorced or widowed by period t; otherwise 0	0.152	0.107	0.140	0.108
Number of individuals	7,919	21,472	69,653	134,464

Notes: Pre-shock levels.

Table 2
Average sickness absence by sector.

	Men				Women			
	Non-treated municipality		Treated municipality		Non-treated municipality		Treated municipality	
	Pre	Post	Pre	Post	Pre	Post	Pre	Post
A. Municipal sector								
Days of sickness absence	3.695	3.935	4.353	3.678	6.421	6.891	7.234	7.403
Incidence	0.043	0.042	0.056	0.047	0.076	0.075	0.094	0.089
Difference post-/pre-shock, days		*0.240*		*−0.675*		*0.470*		*0.169*
Difference-in-differences, days				*−0.915*				*−0.301*
				(*0.281*)				(*0.184*)
Difference post-/pre-shock, incidence		*−0.001*		*−0.009*		*−0.002*		*−0.005*
Difference-in-differences, incidence				*−0.008*				*−0.004*
				(*0.004*)				(*0.002*)
B. Private sector								
Days of sickness absence	3.512	3.568	3.132	3.561	5.229	5.429	5.140	5.440
Incidence	0.039	0.038	0.040	0.040	0.054	0.053	0.058	0.057
Individuals, municipal sector	13,989	12,219	1651	1421	55,664	49,934	6268	5544
Observations, municipal sector	102,780	46,321	11,995	5277	404,867	187,170	44,813	20,451
Individuals, private sector	85,760	72,853	13,597	11,850	48,704	40,715	7875	6588
Observations, private sector	629,696	277,476	99,318	45,061	355,535	153,882	56,980	24,825

Notes: All averages are per quarter. The computation of difference-in-differences is shown in italics, confer Eq. (1). Standard errors of difference-in-differences, estimated in a separate regression, are in parentheses. "Pre" is periods within the years 2006 and 2007; "Post" is 2008. The columns labelled "Treated municipality" report means for employees whose work-place is located in a municipality impacted by the financial shock.

sickness absence grew among Norwegian nurses and auxiliary nurses employed by municipalities implementing down-sizing or significant staff reshuffling in their unit during an eight-year period. The present study differs from Røed and Fevang (2007) in that we exploit the information entailed in an external shock and not just in a particular occupation.

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, with sickness coverage of 100% from the first day. A medical certificate is required for spells of absence of more than three days. The first 16 days of absence are paid by the employer (the employer period), whereas the remaining period is paid by social insurance, organized under the National Labour and Welfare Administration (NLWA). The maximum period of benefits is one year, including the employer period. Wage-earners' income taxes and employers' payroll taxes jointly bear the cost of the NLWA. Clearly, the compensation scheme as it stands is very generous, and compared with most other countries, absence rates are high. During the past 10 years, certified sickness absence has fluctuated around 6–7%, peaking in 2003 at almost 7.5%. As expected, public expenditures for the programme (not including the employer period) are also substantial, comprising about 2.5% of GDP.

Measures to reduce sickness absence have been on the policy agenda for several years, but suggestions to reduce the replacement ratio or to increase the employer period have proved highly controversial. In 2001, the introduction of the so-called Including Working Life agreement, signed by the government and employer and worker organizations aimed to reduce sickness absence by 20% from its 2001 level. The agreement did not involve any changes in replacement rates, but did emphasize improving working conditions and better follow-up of sick-listed workers. In the last quarter of 2012, the absence rate was only 5.6%. However, while this was down from its 2001 level, the reduction resulted not from the agreement but to a tightening of doctor certification rules in 2003 (Markussen, 2009). Other aspects of worker protection in Norway are also quite strong. In particular, there are restrictions on dismissing workers on sick-leave.

Norway has a large public sector, with public consumption representing almost 30% of GDP. The municipalities provide the main public services,[1] either producing services themselves, purchasing services from the private sector, or producing services in co-operation with other municipalities. Municipalities also employ about 20% of the

[1] Hospitals are the most important exception, being enterprises owned by the state.

Table 3
Effect of the financial shock on sickness absence in the public sector. Difference-in-differences.

	Men		Women	
	OLS	FE	OLS	FE
Outcome				
Days	−1.024**	−0.997***	−0.512**	−0.738***
	(−2.33)	(−2.81)	(−2.20)	(−3.37)
Incidence	−0.012**	−0.011**	−0.006*	−0.006
	(−2.19)	(−2.22)	(−0.71)	(−0.77)
Control variables				
Background variables	Yes	Yes	Yes	Yes
Trend, dummies for quarter	Yes	Yes	Yes	Yes
Extra trend for impacted employers	Yes	Yes	Yes	Yes
Individual FE	No	Yes	No	Yes
Observations	166,373	166,373	657,301	657,301
Individuals	15,645	15,645	61,987	61,987

Notes: t-statistics clustered at the municipality level in parentheses.
Columns show results from the OLS and the fixed effects (FE) estimator, respectively. Control variables include education, polynomial of age, marital status, and number of children.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

Norwegian work-force. As noted, the affected municipalities had speculated in future revenues from hydro-electric power production. Norway is rich in water-power, and specific legislation regulates profit sharing between the producer, the state, and local municipalities hosting power plants, so that the latter are secured a part of financial benefits. The municipalities impacted by the Terra crisis are spread throughout the country, although it could be the case that workers employed by municipalities with power production revenues differ from the typical municipal worker. To deal with this possibility, our control group is not the full pool of municipal workers (Norway has 430 individual municipalities) but rather workers in municipalities that have revenues from hydro-electric energy production. The number of control municipalities is much larger than the number of affected municipalities. In the aftermath of the crisis, it became clear that the financial products had been very actively promoted by two brokers in Terra Securities and that the competence for assessing risky financial products was low in the eight affected municipalities. However, we cannot determine why the leadership in some municipalities chose to invest in these risky assets while others did not (Hofstad, 2008).

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 energy-rich municipalities were unaffected by the shock and serve as a control group in this natural experiment. We take advantage of this feature by employing a standard difference-in-differences (DID) approach. Furthermore, private sector workers in these eight municipalities were also unaffected by the shock, a fact that may be exploited in a triple difference (DDD) set-up. In what follows, we use the standard term ‘treatment’ for exposure to the shock. We first consider DID. This estimator compares the average outcome in the treated group to the average in the untreated group, before and after an event exogenous to the group assignment. Let Y denote the outcome (sickness absence), T and C the treatment and control groups, respectively, and let subscripts 0 and 1 denote the pre- and post-treatment periods. The DID estimator of the average treatment effect is then:

$$\hat{\delta}_{DD} = \left(\overline{Y}_1^T - \overline{Y}_0^T\right) - \left(\overline{Y}_1^C - \overline{Y}_0^C\right). \tag{1}$$

In our analysis, we consider municipal sector workers only. The identifying assumption is that the expected change in outcomes for the control group is the same as it would have been for the treatment group in the absence of treatment. Our choice of comparison group relies on assuming that employees in the non-treated municipalities did not expect exposure to similar shocks. We believe this assumption is reasonable in that it became quite clear from the extensive media coverage which particular municipalities had exposed themselves to high-risk financial products. However, if there was a ‘spillover of fear’ we would expect it to diminish the difference in behavioural responses between workers in the affected and non-affected municipalities, and our effect estimate would exhibit a downwards bias. Technically, there could have been indirect effects for *all* municipalities with a full bailout by the central government. However, it seems unlikely that even this would affect the behaviour of the ordinary municipal worker (assuming that the losses in the eight affected municipalities were averaged across all 430 municipalities); however, even if it did, the bias would be negative.

With multiperiod data, we can readily incorporate trends in the model. Using quarterly data 2006–08, we estimate the DID effect from the following regression model for individual i in period $t = 1,\ldots, 12$:

$$Y_{it} = \alpha_0 + \alpha_1 D_{it} + \delta_0 POST_t + \delta_1 POST_t D_{it} + \gamma_0 t + \gamma_1 D_{it} t + \sum_{j=2}^{4} \theta_j Q_j + \beta X_{it} + \varepsilon_{it}, \tag{2}$$

where D_{it} is a dummy variable indicating that individual i was employed in one of the affected municipalities in period t, $POST_t$ is a dummy variable that equals one for periods after the shock and otherwise zero, Q_j is quarter j (to control for seasonal variation in sickness absence), X is a vector of individual characteristics, and ε_{it} is the random error term. This model allows for different time trends and intercepts for the treatment and control groups, and the treatment effect, δ_1, is modelled as the post-treatment shift in the treatment group mean. We estimate Eq. (2) using ordinary least squares (OLS) and fixed effects (FE) estimators. The FE estimator allows for unobserved time-invariant individual heterogeneity.

A potential pitfall of this approach is that even though the financial shock was unexpected, workers may have already self-selected into the Terra municipalities. We address this objection in several ways. First, the comparison group is selected from municipalities with similar characteristics as the exposed (see Section 5 for details). Second, we have tried to check whether employees in impacted municipalities had reasons to feel greater job security than employees of control municipalities, pre-shock. When comparing mean level and growth of gross expenditure pre-shock as well as the net growth in number of jobs, we find no such indications. Furthermore, the FE estimator

Table 4
Effect of the financial shock on sickness absence in the public sector. Triple difference-in-differences (municipal vs. private sector).

	Men		Women	
	OLS	FE	OLS	FE
Outcome				
Days	−1.017**	−1.245***	−0.609**	−1.038***
	(−2.26)	(−3.51)	(−2.16)	(−4.01)
Incidence	−0.009	−0.010*	−0.005	−0.005
	(−1.61)	(−1.81)	(−0.60)	(−0.63)
Control variables				
Background variables	Yes	Yes	Yes	Yes
Trend, dummies for quarter	Yes	Yes	Yes	Yes
Extra trend for impacted employers	Yes	Yes	Yes	Yes
Individual FE	No	Yes	No	Yes
Observations	1,217,924	1,217,924	1,248,523	1,248,523
Individuals	115,027	115,027	118,582	118,582

Notes: See notes accompanying Table 3.

Table 5
Placebo shock. Difference-in-differences.

	Post-shock periods set to periods 5–8				Post-shock periods set to periods 5–12			
	Men		Women		Men		Women	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE
a) Days of absence								
Placebo effect	−0.115	−0.106	−0.405	−0.318				
	(−0.23)	(−0.22)	(−1.23)	(−1.18)				
Placebo effect					0.707	0.678	0.329	0.581**
					(0.87)	(0.99)	(1.28)	(2.15)
b) Incidence								
Placebo effect	0.004	0.002	−0.006	−0.006				
	(0.40)	(0.17)	(−0.88)	(−0.90)				
Placebo effect					0.008	0.006	0.002	0.003
					(0.81)	(0.65)	(0.64)	(0.79)
Observations	114,775	114,775	449,680	449,680	166,373	166,373	657,301	657,301
Individuals	15,640	15,640	61,932	61,932	15,645	15,645	61,987	61,987

Notes: See notes accompanying Table 3.

controls for time-invariant unobserved individual characteristics. For instance, if the affected municipalities had particularly lax (or strict) practices regarding sickness absence that attracted workers with particular attitudes, we difference such unobserved characteristics out of the model. The same argument applies to any differences in individual preferences or health endowments.

A drawback of the DID set-up is that only public sector workers are included in the analysis. By applying a DDD estimator, private sector workers may also be included. Letting T and C denote the same municipalities as before and using $\Delta = \overline{Y}_1 - \overline{Y}_0$, we may write the estimator as:

$$\hat{\delta}_{DDD} = \left(\Delta^{T,Pub} - \Delta^{T,\,Priv}\right) - \left(\Delta^{C,Pub} - \Delta^{C,\,Priv}\right). \qquad (3)$$

This estimator compares the DID between the public and the private sector in the treated municipalities to the corresponding DID in the control municipalities. The identifying assumption now is that the expected DID between the public and the private sector is the same for the control group as it would have been for the treatment group if untreated. This assumption may be problematic if there are spillover effects between sectors, e.g., if absence is contagious in neighbourhoods similarly to Rege et al.'s (2012) findings for disability retirement. However, if private sector absence actually fluctuated in the same direction as public sector absence after the shock, we would expect the estimated effect to be downwards biased. That is less worrying than a case where we suspect that the effect is over-estimated.

In a regression framework, let the dummy variable P indicate a worker in the public (municipal) sector, such that $P = 0$ represents a

Table 6
Omitting municipalities. Difference-in-differences.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Days of absence									
Men									
Treatment effect, 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)
Treatment effect, 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									
Treatment effect, 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)
Treatment effect, 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)
Incidence									
Men									
Treatment effect, 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)
Treatment effect, 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									
Treatment effect, 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)
Treatment effect, 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
Observations, men	165,420	161,937	165,732	165,645	162,461	165,910	165,184	161,423	157,072
Individuals, men	15,555	15,227	15,584	15,576	15,263	15,596	15,532	15,176	14,755
Observations, women	652,326	639,166	654,466	653,343	643,876	655,818	652,630	641,519	620,766
Individuals, women	61,525	60,215	61,719	61,624	60,689	61,849	61,540	60,461	58,455

Notes: See notes accompanying Table 3.

Table 7
Stable workers. Difference-in-differences estimates. Fixed-effects estimator.

	(1)	(2)	(3)	Main results
Days of absence				
Men	−0.609	−0.535	−0.57*	−0.994***
Women	−0.658***	−0.698**	−0.858**	−0.740***
Incidence				
Men	−0.010**	−0.011**	−0.011***	−0.011**
Women	0.000	−0.003	−0.004	−0.007
Observations, men	126,468	137,928	142,001	166,373
Individuals, men	10,539	11,712	12,126	15,645
Observations, women	484,416	536,692	558,456	657,301
Individuals, women	40,368	45,666	47,887	61,987

Notes: t-statistics clustered at the municipality level in parentheses. Subsamples:

Column 1: Employed 2006q1–2008q4 in the same municipality as at 31 December 2006.
Column 2: Employed 2007q1–2008q4 in the same municipality as at 31 December 2006.
Column 3: Employed 2008q1–2008q4 in the same municipality as at 31 December 2006.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

private sector worker. The estimated equation is:

$$Y_{it} = \alpha_0 P_{it} + \alpha_1 P_{it} D_{it} + \alpha_2 + \alpha_3 D_{it} + \delta_0 P_{it} POST_t + \delta_1 P_{it} POST_t D_{it} \\ + \delta_2 POST_t + \delta_3 POST_t D_{it} + \Gamma_P + \Gamma_{-P} + \beta X_{it} + \varepsilon_{it}, \tag{4}$$

where Γ_P and Γ_{-P} contain the trend and quarter terms for the public and the private sector, respectively. The treatment effect δ_1 now measures the effect of the shock on sickness absence by public sector workers in the affected municipalities, net of any general effects that could also have affected private sector workers.

5. Data

The key data source is the administrative registers from Statistics Norway, which comprise the full population and enable us to link data on employers with data on sickness absence (more than 16 days) for the same individual. We identify all individuals who held a job in the treated or control municipalities by 31 December 2006 (almost one year prior to the financial shock). As noted in Section 3, we limit the controls to workers in municipalities that gain income from hydro-electric power production, comprising 167 municipalities in addition to the eight affected municipalities. We exclude employees older than 66 years from the sample. In this sample, we identify municipal and private sector workers. Municipal workers are used in the DID analysis, and the full sample is used for the DDD.

This dataset and the individual-level data on sickness absence from the NLWA can be merged by means of the personal identifier. The absence data draw on sickness insurance payments from the NLWA, which as discussed in Section 3 are from the third week of absence. We include only absence episodes caused by the employee's own sickness, i.e., we ignore absence resulting from the illness of family members. For details on sample selection, see Table A1. We measure sickness absence during 12 three-month periods, i.e., January 2006–December 2008. This procedure leaves us with a dataset of 233,609 individuals for the analysis, 77,632 employed in the municipal sector and 155,977 in the private sector. While employment status on 31 December 2006 determines inclusion in the sample, periods out of employment are not included; thus, the panel is unbalanced.

The treatment group comprises 7925 individuals. Our control group includes 69,707 employees of 167 municipalities that are comparable in the sense that they receive income from hydro-electric energy production. These municipalities are located across all regions of Norway, while four of the affected municipalities are located in southern or western Norway, and the remainder in the same large county in northern Norway. With the DDD approach, we also include 21,480 private sector workers in the affected (treatment) municipalities and 134,497 in the control municipalities.

Table A2 details the data on the eight affected municipalities. Municipalities 2, 5, and 8 are medium-sized towns by Norwegian standards, with the rest relatively sparsely populated. The financial losses in 2007 and 2008 were of considerable magnitude for most of these municipalities, as shown in Table A2. Reduced expenditure in future budgets was required to cover the loss recorded in 2007. Fig. 2 shows that the average expenditure growth per capita was lower in the affected municipalities than in the control group in 2008 and 2009.

We consider two outcomes: i) the number of days of certified sickness absence per quarter, and ii) a dummy variable indicating that an absence spell starts in a given quarter (incidence). As noted, the absence data do not include the initial 16 days of each spell of absence covered by the employer, as is also the case in previous research using Norwegian data, such as Askildsen et al. (2005) and Røed and Fevang (2007). It may seem problematic that we do not have information on the first two absence weeks. However, it appears reasonable that the negative effect on absence (disciplining) would also show up in shorter absences, so if we identify a negative effect in long-term absences, we would also expect the effect on total absence to be negative. Conversely, if we identify a positive (or zero) effect on long-term absences, we may have missed a potential disciplining effect for short-term absences. We return to this issue later. From the worker's point of view, there is no change in the sick pay scheme, and thus no change in incentives, at the 16th day of absence.

We define the respective pre- and post-shock periods as Q1 2006–Q4 2007 and Q1–Q4 2008. Media reports on the financial losses commenced in November 2007, but it seems reasonable that we would observe any potential effects on the level of absence no sooner than the following quarter.

Table 1 details background characteristics by sector and municipality category. We note that within each sector, the groups are similar with respect to age, education, and family characteristics. However, in the municipal sector, the proportion of female employees is higher, along with the average age and the level of education. Table A3 shows the occupational distribution within the municipal sector. Here we note a gender difference, with stronger male representation in administration, whereas women are concentrated in health care and day-care centres. However, the distributions are quite similar in the control and treatment groups.

Table 2 details the average absence levels before and after the shock by gender, sector, and category of municipality. As shown, the level of absence is generally higher for women than for men. In the municipal sector, we note that the pre-shock absence levels are higher in the impacted municipalities for both genders. Moreover, among men, days absent decrease after the shock in the treatment group, but increase in the control group. Among women, days absent increase for both groups

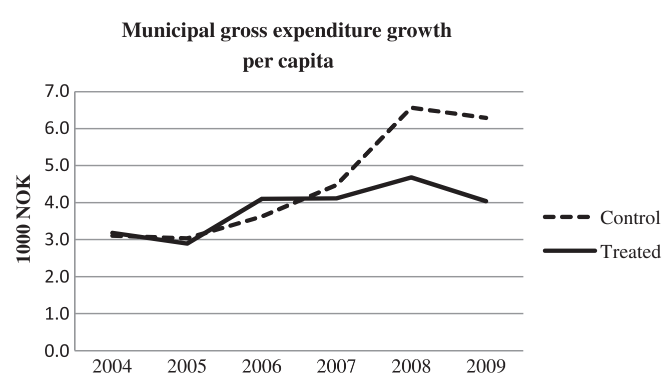

Fig. 2. Municipal gross expenditure growth per capita.

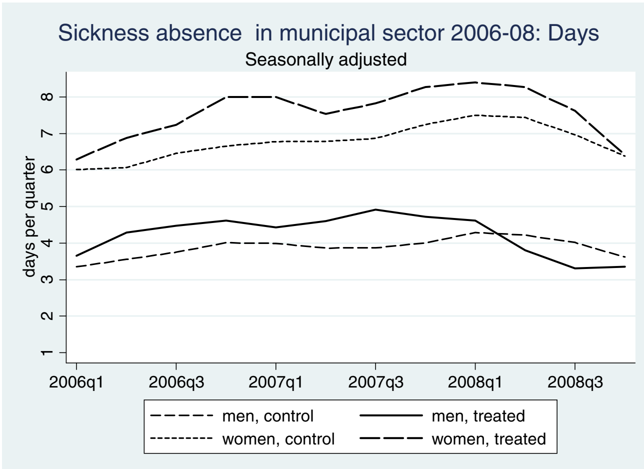

Fig. 3. Mean number of days of absence 2006–08, by gender, period, and treatment status.

after the shock, but less in the treated municipalities. Incidence decreases in all groups, but the most among the treated. Accordingly, when we compute simple DID estimates based on these averages they are negative, suggesting negative effects of the shock. The effect is particularly large for men (−0.9 days from a pre-shock level of 4.4) and is statistically significant. In the private sector, absence levels are more similar across the various municipality categories.

Figs. 3–6 depict the mean absenteeism by gender in the public and private sectors during the observation period, adjusted for seasonal variation. Fig. 3 shows absence days in the public sector. For both genders, absence decreased in 2008, but apparently more so in the affected municipalities. Average incidence, displayed in Fig. 4, reveals a similar tendency for women. For men, the picture is less clear: in the treatment group, incidence fell at the beginning of 2008 but then increased, while there is no clear trend in the control group. Figs. 5 and 6 display the corresponding averages for the private sector. Notably, the patterns are much more similar in the treated and non-treated municipalities for private than for public sector workers. This observation is well in accordance with our assumption that the shock only affected employees in the public sector.

Our main impression from the descriptive statistics is that sickness absence reduced for public employees in the affected municipalities, most clearly for men and more distinctly for days absent than for incidence. In the next section, we investigate whether this finding also appears in a regression-based approach including control variables.

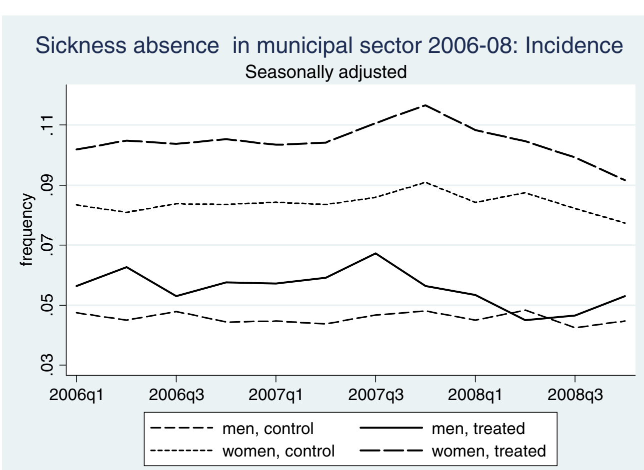

Fig. 4. Mean incidence 2006–08, by gender, period, and treatment status.

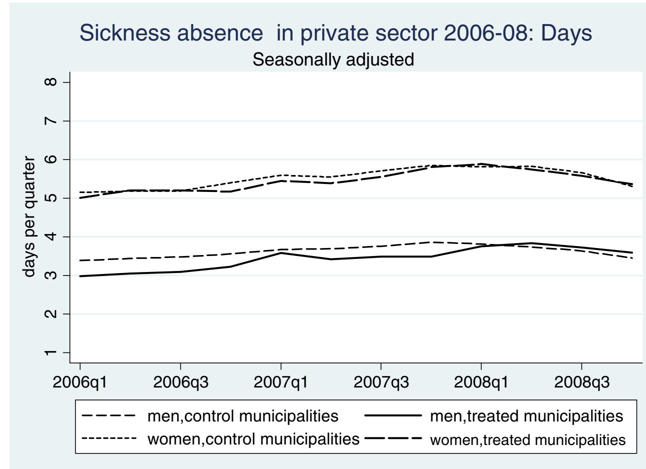

Fig. 5. Mean number of days of absence 2006–08 within private sector, by gender, period, and treatment status of work-place municipality.

6. Estimation results

In this section, we report gender-wise regression results for DID (Eq. 2) and DDD (Eq. 4). We estimate both models using OLS and individual FE for both outcomes (absence days and incidence). In the OLS regressions, we control for age, education, marital status, and the number of children, in addition to the quarter and time period. In the FE regressions, we exclude most of the controls because they do not vary over time.

Table 3 provides DID estimates for absence days and incidence. We only report the estimate for the parameter of interest, δ_1 in Eq. (2) and the t-statistics, estimated with robust standard errors clustered at the municipality level. For absence days, the post-shock effect is statistically significant and larger for men than for women, as with the descriptive statistics. The FE estimates are −0.997 and −0.738 for men and women, respectively. These are larger in magnitude than the simple estimates in Table 1, particularly for women. For women, the FE estimate is also clearly larger than the OLS estimate. In general, we have more confidence in the FE estimate because it controls for unobserved heterogeneity. A number of factors may affect sickness absence, e.g., health and attitudes, and as such, the case for using the FE estimator appears particularly strong. The relative changes are substantial: a decrease of 10% for women and 23% for men when compared with the average pre-shock levels.

As ‘incidence’ is a discrete outcome, we interpret the coefficients as marginal effects on the probability of commencing an absence spell.[2] Here, the estimated effect is statistically significant only for men: a decrease of 1.1 percentage points with FE. As shown, the point estimate for women is larger than the corresponding figure in Table 2, but is statistically insignificant.

Table 4 reports the DDD results from estimating Eq. (4). As the financial shock affected the public sector, we expect a difference in the public–private outcomes for the Terra municipalities. The DDD estimator compares this difference to the corresponding difference in the control municipalities (where the public sector was unaffected by the shock). As it turns out, the DDD point estimates are quite similar to those in the previous table, larger for men than for women, but significant only for absence days. The FE estimates are also somewhat larger than the corresponding DID results, −1.3 for men and −1.0 for women.

[2] We do not apply non-linear probability models such as 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.

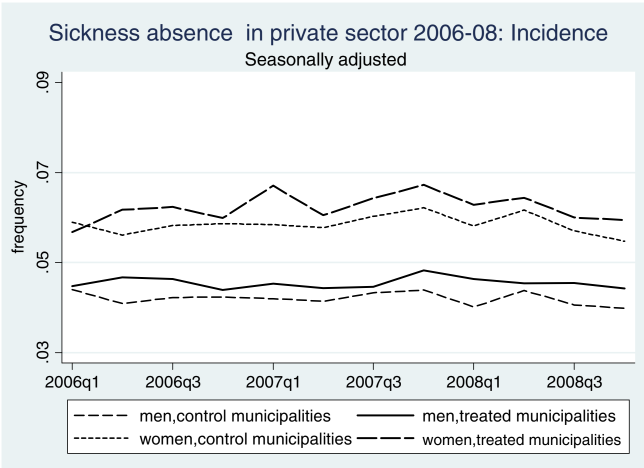

Fig. 6. Mean incidence 2006–08 within private sector, by gender, period, and treatment status of work-place municipality.

The evidence presented thus far is quite clear regarding the length of absence spells: that is, there is a significant reduction for both genders in the municipal sector in the treatment group. For incidence, the evidence is less clear. Inspection of the graphs shows that the data are noisier. The DID regressions suggested a negative effect, but this is statistically significant only for men, and the DDD results were insignificant (only one coefficient attained the 10% level of significance). The reason may be that we analyse sickness spells that last two weeks or more, and reducing the duration is a smaller adjustment than skipping the sickness episode altogether. Unfortunately, we do not have information on absences of less than two weeks, so there may have been an effect on the incidence of shorter spells of sickness that we cannot observe. The gender differences are interesting. It is well known that women's sickness absence levels are higher than men's (see, e.g., Vistnes, 1997; Mastekaasa and Olsen, 1998; Markussen et al., 2011) and they also appear less responsive to negative organizational shocks. However, we cannot infer whether this is because of differences in job characteristics, health, or attitudes.[3]

We performed several tests for robustness. First, Table 5 checks if there is any effect of a placebo shock in the DID set-up. In this, we redefined the treatment dummy to equal one from Q1, 2007 onwards. This is almost one year before the crisis, and we know of no other particular events at the time that should have affected sickness absence systematically. Thus, if this placebo treatment results in any effect, it leads us to suspect that the effects revealed in Tables 2–4 are spurious. In the left-hand 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-hand panel, we also include 2008, such that the treatment dummy equals one in both 2007 and 2008. All coefficients but one are also insignificant in this case. However, we should note that the placebo in the right-hand panel in Table 5 is different in the sense that observations from the true post-shock period are included. Nonetheless, the overall impression from the placebo regressions is to increase our confidence in the main results.

As seen in Table A2, the affected municipalities range in size from Hattfjelldal (1482 inhabitants) to Haugesund (32,303 inhabitants). Their recorded losses and expenditure levels also vary. Thus, the results may be sensitive to the inclusion/exclusion of some municipalities. To check this, we re-estimated the models, omitting one affected municipality at a time. Table 6 provides the results. Compared with the main results, the reduced samples produce quite similar results for absence days and incidence, the exception being that excluding the second-largest municipality, Rana (column 8), makes the effect on incidence insignificant, but still negative. Furthermore, it appears that the strength of the response relates to the size of the shock. Column 9 shows that when we omit the municipalities where the recorded financial losses were smallest (in Kvinesdal, Haugesund and Narvik, as shown in Table A2), the estimated effects generally increase in magnitude.

Even though we do not find evidence at the aggregate level that particular municipalities drive the results, it could be the case that worker turn-over 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) analysing subsamples of stable workers, ii) comparing turn-over in the treatment and comparison groups, and iii) looking at the absence histories of workers who changed employer in the post-shock period.

Table 7 shows FE regressions for absence days and incidence for the subgroup of stable workers. Columns 1–3 detail the results for workers who stayed with the same employer that they had in December 2006 continuously from January 2006 until December 2008 (column 1), in 2007 and 2008 (column 2), and in 2008 (column 3). Comparing these results with the main results (reproduced in the final column), we find that for days, the point estimates are quite similar, but significant only for women. However, the incidence estimates are almost unchanged. We conclude that our main results are supported, but with some uncertainty regarding a potential selection effect for men.

Fig. 7 depicts turn-over, defined as the proportion of individuals in each period not employed with the same employer as at 31 December 2006. The employer–employee relationship of that date is the basis for the indicator for working in an affected municipality in Eq. (2). We define turn-over separately for the subsamples of treated and non-treated municipalities. As shown, the levels are somewhat higher in the treatment group, but the trends are very similar. However, we also estimated linear probability models for the probability of leaving a public sector job in the affected municipalities (results not shown) and found that men, but not women, actually had a higher probability of leaving employment in the period after the shock.

Figs. 8 and 9 indicate absence trends before the shock for employment leavers and stayers. We plot absence days separately for stayers and leavers, where we define leavers as workers employed by the same municipality on 31 December 2006 but not on 1 January 2008. We can see that the levels are somewhat higher for female stayers in the affected municipalities than in the comparison group, but with no clear difference in trends. For both genders, there is an upward shift in 2007 for leavers, and for men this shift is larger in the affected group of municipalities. 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 estimated absence reduction for men in the main sample. As we

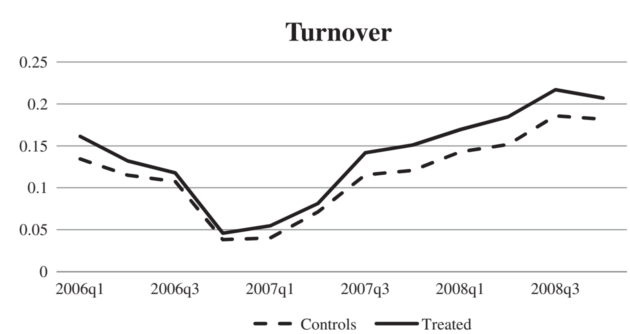

Fig. 7. Turn-over by treatment status and period. Proportion of each subsample not employed with the same employer as at 31 December 2006.

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

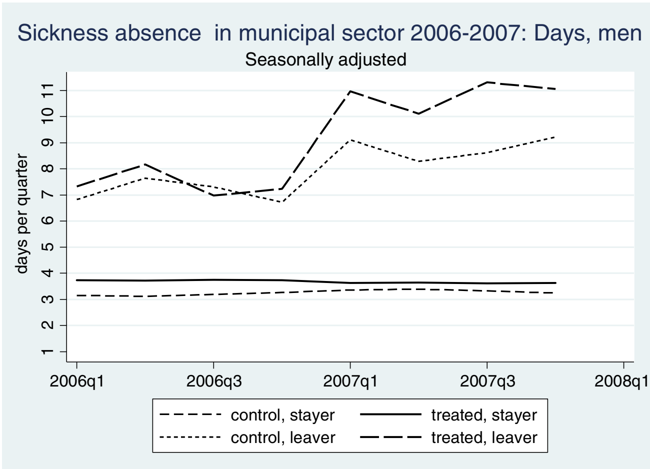

Fig. 8. Pre-shock levels of sickness absence among men, by stability in employer–employee relation, period, and treatment status.

saw in Table 7, absence fell among stable workers, but the effect on absence length appears mostly driven 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 affected municipalities. After inspecting selection out of the sample, the evidence of an effect on stable workers is strongest for women. The placebo exercise provides evidence against the effect being a time trend, and the conclusion is robust to omitting municipalities or workers who changed employment.

7. Concluding remarks

The financial shock that hit some Norwegian municipalities in 2007–08 may have affected sickness absences of public employees through several channels. Previous research suggests several primary hypotheses. First, the crisis could have had a direct negative health effect. Second, impacted workers may have felt less compelled to restrain themselves from absence in response to the apparently irresponsible financial behaviour of their employers (the reciprocity hypothesis). However, the fact that absence actually fell rejects these hypotheses. Third, another hypothesis is that reduced absence resulted from changes in the composition of workers. There are some indications of selection out of employment among men. Conversely, we also find a decrease in incidence among stable male workers.

Fourth, the prospect of jobs becoming less secure could have had a disciplining effect leading to less absence. Our results are consistent with this hypothesis and also agree with existing 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 the 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 higher per capita loss. Moreover, the fixed effects results are purged of unobserved heterogeneity at the individual or municipal level.

In our analysis, the data are at the individual level whereas the negative shock is at the employer level, and the mechanism connecting the two is not quite clear. Even so, we observe quite large effects, for example, sickness absence fell as a result by about 10% for women. The bad news became public in October–November 2007. The resultant 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 cuts. We find that sickness absence dropped from the first quarter of 2008, but have no evidence that the number of positions fell 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 down-sizing that may have induced less absence, not the down-sizing per se.

We should also note that what we have found is a short-run effect. Unfortunately, the post-shock period for which data are available is still too short to test for long-run effects. Moreover, it is most likely that the effect of an *expected* reduction in job security is only temporary. Overall, our results are not necessarily at odds with Røed and Fevang (2007) who found that *actual* down-sizing increased absence among Norwegian nurses. A possible mechanism is that the threat of future downscaling provides workers with an incentive to reduce absence in the short run, but that prolonged insecurity involves negative health effects that dominate in the longer run.

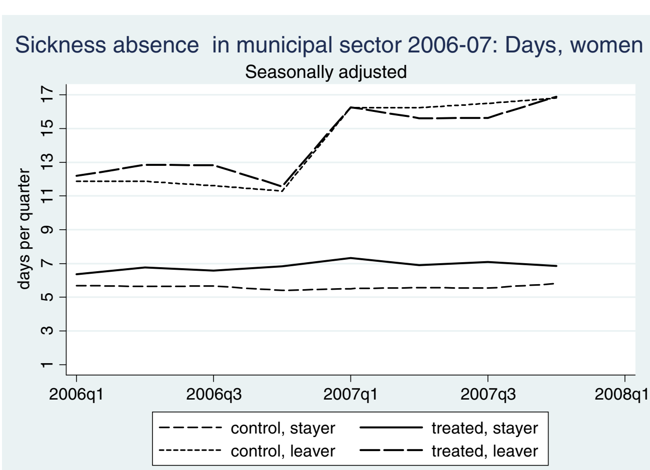

Fig. 9. Pre-shock levels of sickness absence among women, by stability in employer–employee relation, period, and treatment status.

Appendix A

Table A1
Sample selection.

	Number of employees, by sector	
	Municipal	Private
Employed in the sector at 31 December 2006	370,834	1,190,549
Removed from sample because:		
Employed in several municipalities with different treatment status	−2,787	−659
Employed in several treated municipalities	−3	−3
Aged 67 years or more in 2006	−744	−9,963
Other reason for sickness absence than own sickness	−5,427	−23,438
Outlier, >20 sickness absence episodes 1992–2008	−1,919	−1,937
Employed both within and outside given sector	−22,889	−30,772
Other reasons	−444	−18,137
Total	336,621	1,105,640
From this dataset we extract		
Treatment group: working in municipalities impacted by shock	7,925	21,480
Control group: working in other municipalities that receive income from hydro-electric power	69,707	134,497
Total	77,632	155,977
Dataset for analysis: 233,609 employees		

Table A2
Municipalities affected by the financial shock.

	Municipality	No. of inhabitants, December 2006	Gross expenditure per capita, 2006, NOK 1000	Recorded loss per capita, municipal accounts, NOK 1000		
				2007	2008	2009
1	Kvinesdal	5,595	59	2.8	1.3	0.0
2	Haugesund	32,303	45	3.0	1.1	−0.7
3	Vik	2,835	64	23.8	12.5	0.0
4	Bremanger	3,930	63	46.2	14.9	0.0
5	Narvik	18,301	54	6.4	3.9	0.0
6	Hattfjelldal	1,482	71	86.8	−27.0	0.0
7	Hemnes	4,510	66	22.4	−7.0	0.0
8	Rana	25,190	47	10.1	−1.3	0.0

Notes: Data on recorded loss provided by municipal administrations. Per capita loss calculated using the number of inhabitants in 2006.

Table A3
Occupational distribution 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
Total	100	100	100	100
Number of individuals	13,994	1,651	55,713	6,274

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