Downsizing and sickness absence

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Abstract

It is generally assumed that organizational downsizing has considerable negative consequences, not only for workers that are laid off, but also for those who remain employed. The empirical evidence with regard to effects on sickness absence is, however, inconsistent. This study employs register data covering a major part of the total workforce in Norway over the period 2000 to 2003. Number of sickness absence episodes and number of sickness absence days are both analyzed by means of Poisson regression. To control for both observed and unobserved stable individual characteristics, conditional (fixed effects) estimation is employed. The analyses provide some weak indications that downsizing may lead to slightly *less* sickness absence, but the overall impression is that downsizing has few if any effects on the sickness absence of the remaining employees.

Key words: Downsizing, upsizing, sickness absence, fixed effects, conditional Poisson regression

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Introduction

Organizational downsizing and restructuring is generally assumed to have considerable negative health effects, not only for those who lose their jobs, but also for those left in the organization (Ferrie, Westerlund, Virtanen, Vahtera, Kivimäki, 2008; Quinlan, Mayhew & Bohle, 2001; Sverke, Hellgren & Näswall, 2002). Out of 41 studies in the review by Quinlan et al., for instance, 36 reported negative associations of downsizing or other restructuring with some health outcome. The evidence is nevertheless limited in several ways: Many studies rely on workers' self-reports of both downsizing and health outcomes, most evidence comes from the public sector, and studies are often limited to a single organization. With regard to downsizing and sickness absence in particular, the 2001 review by Quinlan et al. included only six studies (Beale & Nethercott, 1988; Szubert, Sobala, & Zycinska, 1997; Kivimäki et al., 1997; Kivimäki, Vahtera, Koskenvuo, Uutela, & Pentti, 1998; Kivimäki, Vahtera, Pentti, & Ferrie, 2000; Vahtera, Kivimäki, & Pentti, 1997), and the latter four of these seem to use basically the same data source.

Subsequent studies also provide only limited support for a substantial effect of downsizing on remaining workers' sickness absence. Most notably, the fairly large general population study by Westerlund et al. (2004) in Sweden finds no increase in long term sickness absence associated with large downsizing (>18% reduction in work force), while there is a small increase (OR=1.07) associated with moderate (8%-18%) downsizing. A smaller population study of the Stockholm area by Theorell et al. (2003) found downsizing to be associated with *lower* sickness absence in women, while a zero association was found for men. Two other large studies find evidence of downsizing effects (Røed & Fevang, 2007; Vahtera et al., 2004), but at least in the Røed & Fevang study the estimated effects are not very large: Among Norwegian nurses a 20% workforce reduction led to 6% increase in the sickness absence hazard rate and to a 13% decrease in the return to work rate. The estimated downsizing effects are somewhat larger in Vahtera et al.'s study of Finnish municipal employees, with major downsizing (18% or more) being associated with about 22% increase in the frequency of absence spells among permanent employees.[1]

There are numerous reasons why findings may differ between studies. In the first place, there may be national differences. Secondly, Westerlund et al. (2004) and Theorell et al. (2003) are the only studies known to us that use general population samples, so otherwise there could also be differences between occupations or industries. Third, there are considerable differences in absence measures, with Westerlund et al. including only very long absences (90 days or more), Theorell et al. (2003) and Røed and Fevang (2007) even medium length spells (more than 14 or 16 days, respectively), and Vahtera et al. (2004) all medically certified sickness absence. In one of the very few studies comparing short and long absence episodes, Vahtera et al. (1997) found that downsizing was associated with a decline in short and an increase in long absence episodes.

Since the empirical evidence on the downsizing and sickness absence is quite limited, there is an obvious need for more studies, and particularly of data not limited to specific organizations or industries. In this study we use a very large data set encompassing the entire population of employees in Norway (although excluding very small organizations). The data are based on record linkage of information from several public registers and cover the years 2000 to 2003.

Since we have panel data over four years, an important improvement in comparison with most previous research in this field can be obtained by using conditional (fixed effects) regression methods. This method effectively removes the impact of all unmeasured individual heterogeneity that is stable over time, e.g. a potential tendency for individuals with long-lasting health problems to be either more or less likely to be laid off in downsizing processes (cf. Cameron & Trivedi, 2005).[2] Although our major focus is on downsizing, we also include separate categories for upsizing. Since the consequences of downsizing are arguably greater in the private than in the public sector (where redundant employees, at least in Norway, are generally transferred to other units rather than laid off), separate analyses are carried out for each sector (Røed & Fevang, 2007).

Conceptual and theoretical issues

Downsizing processes

By its very nature, downsizing is a selection process, of which the most obvious aspect is that some workers exit while others, i.e. the downsizing 'survivors', remain in the organization. Neither management nor workers are likely to have full control over this process. It is therefore not obvious if or how downsizing on average will affect the health distribution of remaining workers.

For example, downsizing tends to produce a more senior work force (in terms of both age and length of service). This is partly because layoffs are often by seniority, even in non-unionized settings, but organizational growth or decline will also affect mobility prospects for individuals within the organization regardless of how it is achieved (Pfeffer, 1983). In downsized organizations, creation of new jobs is likely to be modest, and longer-tenured and older workers are also less likely to quit voluntarily regardless of the reason. New workers are therefore likely to be discouraged by the lack of advancement opportunity and leave, and those with less investment in the organization, with fewer commitments, and less experience will be more likely to turn over as prospects of their slow advancements become clear. A negative selection in terms of health may also occur if limited opportunities in downsizing firms cause those with relatively good labour market prospects to leave while those with few alternative opportunities remain.

Of course, employers will probably try to retain more motivated and productive workers, who presumably also have less absence, e.g. by offering promotions etc., and to lay off less productive workers. Such factors will likely contribute to a positive health selection, i.e. remaining workers have better health than laid off workers. There is also some evidence that unhealthy workers are more likely to be laid off (Henningsen & Hægeland, 2008; Kivimäki, Vahtera, Elovainio, Pentti and Virtanen, 2003; Mastekaasa, 1996), suggesting that productivity considerations often trump seniority considerations in layoff decisions. The study by Henningsen & Hægeland (2008) suggests, however, that overall the relationship between previous sickness absence and departure (voluntary or involuntary) is not stronger during periods of downsizing than at other times. We feel it is premature to conclude about the average characteristics of the process, given the scarce empirical evidence, but it seems clear that the process is moderated by individual and organizational characteristics, such as the negotiating power of unions, legal issues, and cultural norms, as well as outside employment opportunities.

Downsizing and sickness absence

The effect of downsizing on remaining workers sickness absence is most often explained as the result of a detrimental effect of downsizing on remaining workers' health. Kivimäki, Vahtera, Griffiths, Cox & Thompson (2000:89), for example, suggest that the association between downsizing and sickness absence can be viewed as a psychosocial process: 'downsizing may exacerbate the 'classic' equation for stressful working conditions […] – increased job demands, decreased job control, and decreased social support – and it is entirely possible that such an effect may partly mediate its other effects on absence and health'. The main argument can be perceived as a causal chain where downsizing → stress → ill health → increased absence. However, downsizing may also have other effects on remaining workers' sickness absence.

Brockner (1988) was among the first to recognize the need for systematic research on the impact of downsizing on the employees who are not laid off, i.e. the 'layoff survivors'. He laid out a simple conceptual model that suggested that layoffs can affect survivors' psychological states (e.g. feelings of job insecurity, positive inequity, anger, relief) which, in turn, can influence their work behaviours (e.g. performance level, motivation) and attitudes (e.g. satisfaction, commitment). Thus, Brockner and his colleagues were clear that layoff survival is the kind of event that can be experienced very differently and may even be associated with positive psychological states like relief. Mishra & Spreitzer (1998) also found, in their synthesis of the literature on survivor responses to downsizing, that survivors can have a wide variety of responses to the stress of organizational downsizing. These responses may be constructive or destructive, depending on the extent to which employees view significant threat or harm from the downsizing.

Evidence of a strong association between ill health and sickness absence, in particular for medically certified and long-term absences (e.g. Marmot, Feeney, Shipley, North, & Syme, 1995), does not mean we should downplay the importance of other factors, e.g. the motivation to go to work, as important causal determinants of sickness absence. According to Steers & Rhodes' (1978) 'process model' of sickness absence for example, presence at work (i.e., not being absent) is seen as a joint function of the motivation and the ability to be present, and both components are necessary for the employee to go to work. For example, sickness absence rates tend to be negatively associated with unemployment rates (e.g. Askildsen, Bratberg, & Nilsen, 2005) and have been found to be positively related to job security (e.g. Virtanen et al., 2003). This suggests that rising unemployment and perceived job insecurity may act as disciplinary devices, producing pressure or incentives for employees to go to work regardless of their health status, thus replacing sickness absence with 'sickness presence' (see e.g. Vingård, Alexanderson & Norlund, 2004). This phenomenon may be relatively prevalent among downsizing survivors, due to the increase in job insecurity and workplace demands, e.g. difficulties in staff replacement, time-pressure or insufficient resources, that are frequently associated with downsizing.

Methods

Sample and measures

Norwegian employers are required to report all employment relationships to the Norwegian Labour and Welfare Administration. This employment register makes it possible to track how individuals move between employers (or more specifically establishments, as an organization's activities may be spread on several sites). Our data set encompasses all individuals that were employed in a private or public sector organization in Norway per May 15, for each year 2000-2003. Unique identification numbers make it possible to link individuals and organizations and to follow both from year to year. These identification numbers also make it possible to obtain additional information on both individuals (e.g., sickness absence, demographic data) and organizations (e.g., sector) from other registers administered by the Norwegian Labour and Welfare Administration or Statistics Norway.

Measurement of downsizing and upsizing. Since there is information on all active employment relationships per May 15 in each year, we measure change in the number of employees from May 15 in a given year to May 15 the next year. Following previous Finnish and Swedish studies, we distinguish between a net personnel reduction of more than 8% (decline), a net increase of more than 8% (growth), and less than 8% net change (stability). The decline category was further subdivided into moderate (8-18%) and strong (>18%) decline. Measures of down- and upsizing based on net changes in the number of personnel are highly unreliable in small organizations. If there are twelve employees or less, the departure of a single individual, for instance, is sufficient to put the organization in the moderate decline category, but such small fluctuations in the number of employees are of course very likely to happen by chance. We therefore restricted the sample to organizations with at least 25 employees in the year 2000.

Sickness absence. The sickness absence register, also kept by the Norwegian Labour and Welfare Administration, contains information on all sickness absence spells certified by a physician (and physicians are required to report these). Short absence spells not requiring such certification are not included. In Norway, all employees are allowed to have up to three short absence spells within a twelve month period without certification by a physician. The maximum length of these self-certified absence spells is generally three days, although it has been extended to eight days in many organizations. Since there is a limit on how many self-certified sickness spells that can be taken in a twelve month period, the sickness absence register will contain some sickness spells shorter than four days. The sickness absence measures used in our analyses are based on absence spells of at least four days duration, however.

We employ separate measures of the *number of sickness absence episodes*, and of the *number of days lost due to sickness absence*. Since most absence spells are short, the former measure will be relatively sensitive to short term absences. The number of days lost measure, in the other hand, is very sensitive to long term absences.

Statistical methods and analytical strategy

We start by regressing sickness absence in year t on downsizing and other variables as measured in the same year (t) and in the previous year (t-1). In this analysis all employment relationships that are active in two subsequent years during the 2000 to 2003 period are included. (Two subsequent years are necessary to estimate the relationship between downsizing in one year and sickness absence in the next.) Since our dependent variables are counts (of absence spells and absence days, respectively), Poisson regression is an appropriate method. The equation can be written as:

$$\ln(\lambda_{it}) = \alpha_i + \beta_1^{'} C_{it-1} + \beta_2^{'} C_t + \beta_3^{'} T_t + \beta_4^{'} Z_i + \beta_5^{'} Z_{it} \qquad (1)$$

λ_{it} is the expected number of sickness absence spells (alternatively days) for individual *i* in year *t*, C_{it} and C_{it-1} are vectors of dummies for upsizing and downsizing in year *t* and *t-1*, respectively, T_t is a vector of year dummies, and Z_i and Z_{it} are vectors of time invariant and time varying explanatory variables, respectively. α_i is a time invariant individual specific term and the β's are vectors of regression coefficients.

Panel data with two to four years of observation for each individual make it possible to estimate the equation using conditional (fixed-effects) Poisson regression, i.e. conditioning on the individual's total number of absence spells or absence days during the period of observation (see Cameron & Trivedi, 2005). The conditional estimator implies that all comparisons are made within individuals, i.e. we compare an individual's sickness absence in the year following exposure to downsizing or upsizing with the same individual's sickness absence in other years. All time invariant variables then drop out of the equation, implying that the α_i and β_4 parameters are not estimated.[3] The fact that time invariant variables drop out of the equation implies that the conditional regression model controls even for *unmeasured* confounders as long as these are stable over time (e.g. stable individual differences in health or personality). This can be proven rigorously (see e.g. Hausman, Hall & Griliches, 1984), but it also makes intuitive sense, as a characteristic which is itself stable cannot easily explain variation over time in another characteristic. The great advantage of the conditional estimator, then, is that it provides consistent estimates of causal effects even if selection/attrition determines the sampled data, provided that this selection/attrition is on time-invariant factors (cf. Cameron & Trivedi, 2005).

The Poisson model makes the often unrealistic assumption that the variance is equal to the mean. If this assumption does not hold, estimates of the regression coefficients are still consistent, but estimates of the standard errors are not. We therefore report robust standard errors. Since employees are clustered within organizations, the robust standard errors corrected are also corrected for clustering.[4]

A problem with the design described above is that many organizations have repeated down- or upsizings. Thus, organizations that did not downsize or upsize in year t or t-1 may have done so in year t-2 (or in earlier years), and these earlier personnel changes may have an effect on sickness absence in year t. The likely implication of this is that downsizing and upsizing effects will be underestimated. On the other hand, organizations that down- or upsized in year t or t-1 may also have done so in earlier years, which may serve to inflate the estimated effects. We try to solve these problems by including a second set of analyses, using only a subset of the data and concentrating on estimating the effects of downsizing in 2002 on sickness absence in 2003. In these analyses, only employment relationships that were active in the whole period 2000–2003 are included. (Organizations that did not exist in 2000 or that ceased to exist in the 2000–2003 period are thus excluded, as are individuals who were not employed in the same organization in all years.) We also include only organizations that did not downsize or upsize in the two previous years (2000 and 2001).

The idea here is to approximate a quasi-experimental situation. Our analytical strategy is similar to what economists call 'difference-in-differences' (DID) estimation, which effectively removes all bias that would otherwise arise from trends in outcomes (Meyer, 1995). The reason for this is that it controls for such trends by including a comparison group who is never exposed to the hypothesized causal event, but (are assumed to) experience the same trends. Within this framework it is standard to define a 'treatment' group indicator (distinguishing workers in downsizing organizations from the 'comparison group' of workers in stable organizations) and a time indicator (which identifies the 'before' and 'after' period), and enter them along with an interaction term (the treatment group indicator multiplied by the time trend indicator) in the regression model:

$$\ln(\lambda_{it}) = \alpha_i + \beta_1^{'} D_i + \beta_2^{'} T_t + \beta_3^{'} (D \times T)_{it} + \beta_4^{'} Z_i + \beta_5^{'} Z_{it} \qquad (2)$$

In equation (2), D_i is a vector of treatment group indicators (dummies that distinguish between treatment and comparison group workers), and $(DxT)_{it}$ is a vector of interaction terms of D_i with T_t. The other variables are as defined in connection with equation (1). As before, time invariant terms drop out of the equation. Also as before, the β's are vectors of coefficients for the various variables. More specifically, β_1 measures the pre-treatment differences between the treatment groups and the comparison group, and the coefficients for the interaction terms (β_3) measure the extent to which the pre-treatment differences have declined or increased after treatment, and are the parameters of primary interest.[5] Since we use Poisson regression, exp(β_3) can be interpreted as showing by what factor the pre-treatment rate ratios (RR's) for the treatment versus the comparison groups have changed.[6]

We use the year 2000 as the baseline year or 'before' period. The reason for this is that 'anticipation effects' may otherwise result in biased estimates (2002 downsizing may for instance be announced in 2001, and impact workers' absence or quit behaviour before any actual downsizing has taken place).

Results

Table 1 provides some descriptive statistics. For both sectors and for both men and women, establishments with less than 8% change in the number of employees make up about half of the observations. In the private sector, downsizing (the two downsizing categories combined) and upsizing seem to occur about equally often, whereas upsizing is more common than downsizing in the public sector. Part time work is much more common among women than among men, and particularly among women in the public sector.

The average number of sickness absence spells as well as sickness absence days does not vary strongly between the sectors. On both measures the averages for women are, however, much higher than the averages for men. The gender differences are also somewhat larger in the public than in the private sector.

Men outnumber women in the private sector, whereas the opposite is the case in the public sector. This is also reflected in the average number of male and female employees per

Table 1. Descriptive statistics. Men and women in private and public sector establishments with more than 25 employees.

	Private sector		Public sector	
	Men	Women	Men	Women
Year (percent)				
2000	25.0	24.8	24.5	23.5
2001	23.9	24.1	24.6	24.0
2002	25.6	25.5	25.1	25.5
2003	25.6	25.6	25.8	27.1
Personnel change in year t (percent)				
Less than 8% change	53.2	52.4	61.2	53.8
8-18% reduction in t	15.1	14.6	10.9	12.0
>18% reduction in t	9.2	9.1	5.6	5.4
>8% growth in t	22.5	23.9	22.4	28.8
Personnel change in year t-1 (percent)				
Less than 8% change	46.2	44.8	49.5	43.3
8-18% reduction in t	12.9	13.0	9.7	10.3
>18% reduction in t	13.8	13.9	11.2	9.1
>8% growth in t	27.1	28.3	29.6	37.2
Working hours (percent)				
4-19 hours per week	5.2	15.7	5.9	20.8
20-29 hours per week	1.5	12.1	3.7	20.6
30 hours per week	93.3	72.2	90.3	58.6
No. of sickness absence spells				
Mean	0.422	0.557	0.383	0.607
St. dev.	0.798	0.891	0.757	0.912
No. of sickness absence days				
Mean	12.917	19.980	12.215	22.855
St. dev.	38.904	48.405	38.525	52.173
No. of person-years	1172449	572958	617499	1064824
No. of individuals	465215	242260	239407	412092
No. of establishments	10470	10229	8202	8457
Mean no. person-years per ind.	2.52	2.37	2.58	2.58
Mean no. of ind. per establishm.	44.43	23.68	29.19	48.73

establishment. The average number of employees (males plus females) is, however, about the same in both sectors.

In Table 2 the number of sickness absence spells is regressed on personnel changes in the same and in the previous year, along with control for working hours. The main impression is that there is very little evidence of any effect of personnel changes. For men and women in the public sector and for women in the private sector none of the coefficients is significantly different from zero. For men in the private sector, there are two significant coefficients: Moderate personnel reduction in the present year and large reduction in the previous year are both associated with a lower frequency of sickness absence. The estimated effects are extremely weak, however.

In addition to the findings with regard to personnel changes, one may note that part time work is associated with fewer sickness episodes, but the associations are weak.

The overall impression of near zero relationships between personnel changes and the frequency of sickness absence is replicated for the number of days lost in Table 3. Three coefficients are significantly different from zero. The largest of these (in numerical value) is a negative coefficient of -0.063 for moderate downsizing in the same year among women in the private sector. There is no effect of lagged downsizing for this group, however. Large downsizing is positively associated with sickness absence for men in the private sector, but again this holds only for sickness absence in the downsizing year and not the following year. Personnel *increases* are very weakly associated with increased sickness absence for women in the private sector and with reduced sickness absence for men in the public sector.

There is a strong relationship between working hours and number of days absent due to sickness. In both sectors and for both men and women, employees have more than 30 percent fewer absence days when working less than ten hours a week than they have in full time jobs.

The conclusion to be drawn from the analyses presented so far is clearly that personnel changes have very little if any impact on both the number of sickness spells and on the number sickness absence days. As noted above, however, the true effects of these changes may have been underestimated with the design used so far, e.g. because the reference category of stable establishments will include establishments that have undergone personnel changes in previous years. We therefore now turn to see whether personnel changes have any impact when we exclude establishments with more than one year of personnel changes 2000–2003 and also employees that are not stably employed in one establishment during this period. The expectation is that these sample restrictions will maximize our possibilities for finding downsizing or upsizing effects.

Table 4 shows the estimated effects of personnel changes in 2002 on the frequency of sickness absence in the restricted sample. For men in the private sector, moderate downsizing is associated with a small 7 percent decrease in the number of sickness absence spells in the post-downsizing year (2003). The coefficient for major downsizing is also negative, but slightly smaller and far from being statistically significant. For women in the private sector, none of the coefficients for the change in the number of employees by year terms is significantly different from zero.

Table 2. Conditional Poisson regression of the frequency of sickness absence on personnel changes. Establishments with more than 25 employees. All individuals employed in at least two consecutive years 2000-2003.

	Private sector		Public sector	
	Men	Women	Men	Women
	b	s.e.	b	s.e.
Year				
2001	0.049***	0.034***	0.046***	0.035***
	(0.006)	(0.006)	(0.007)	(0.005)
2002	0.061***	0.022**	0.056***	0.033***
	(0.007)	(0.007)	(0.007)	(0.005)
2003	0.067***	0.050***	0.069***	0.027***
	(0.007)	(0.008)	(0.009)	(0.006)
Change in no. of employees (<8% change omitted)				
8-18% reduction in t	-0.017*	-0.009	0.010	0.001
	(0.009)	(0.010)	(0.010)	(0.007)
8-18% reduction in t-1	-0.015	0.004	0.011	0.007
	(0.008)	(0.009)	(0.010)	(0.006)
>18% reduction in t	-0.021	-0.018	-0.017	-0.005
	(0.012)	(0.012)	(0.014)	(0.009)
>18% reduction in t-1	-0.024*	-0.018	-0.012	-0.002
	(0.012)	(0.014)	(0.014)	(0.012)
>8% growth in t	0.005	0.004	-0.004	0.010
	(0.006)	(0.007)	(0.008)	(0.004)
>8% growth in t-1	-0.009	-0.006	-0.002	0.002
	(0.006)	(0.007)	(0.008)	(0.005)
Working hours per week (>30 h. omitted)				
<10 hours	-0.127***	-0.080***	-0.076**	-0.087***
	(0.028)	(0.018)	(0.025)	(0.009)
10-30 hours	-0.056	-0.039**	-0.060*	-0.037***
	(0.031)	(0.015)	(0.024)	(0.008)
Log likelihood	-286400	-168885	-139334	-354132
N (person-years)	492612	274468	248123	567261
N (persons)	150399	86295	76358	176074

Note: Robust standard errors corrected for clustering. Significance probabilities are denoted as follows: *** for p<.001, ** for p<.01, and * for p<.05.

Table 3. Conditional Poisson regression of the number of sickness absence days on personnel changes. Establishments with more than 25 employees. All individuals employed in at least two consecutive years 2000-2003.

	Private sector		Public sector	
	Men	Women	Men	Women
	b	s.e.	b	s.e.
Year				
2001	0.221***	0.155***	0.255***	0.186***
	(0.009)	(0.011)	(0.012)	(0.008)
2002	0.348***	0.258***	0.383***	0.282***
	(0.010)	(0.012)	(0.013)	(0.008)
2003	0.484***	0.354***	0.505***	0.376***
	(0.012)	(0.013)	(0.016)	(0.010)
Change in no. of employees (<8% change omitted)				
8-18% reduction in t	-0.020	-0.063***	-0.033	0.002
	(0.012)	(0.016)	(0.019)	(0.011)
8-18% reduction in t-1	0.000	-0.007	-0.013	0.006
	(0.012)	(0.014)	(0.017)	(0.010)
>18% reduction in t	0.046*	-0.016	-0.039	0.012
	(0.018)	(0.020)	(0.028)	(0.017)
>18% reduction in t-1	-0.008	-0.005	0.001	-0.006
	(0.017)	(0.021)	(0.024)	(0.016)
>8% growth in t	-0.017	0.033**	-0.019	0.006
	(0.010)	(0.012)	(0.013)	(0.008)
>8% growth in t-1	-0.020	-0.006	-0.030*	-0.012
	(0.011)	(0.012)	(0.015)	(0.008)
Working hours per week (>30 h. omitted)				
<10 hours	-0.383***	-0.320***	-0.398***	-0.412***
	(0.048)	(0.032)	(0.043)	(0.015)
10-30 hours	-0.180**	-0.139***	-0.228***	-0.193***
	(0.056)	(0.026)	(0.045)	(0.013)
Log likelihood	-7554373	-5276175	-3840773	-1.2E+07
N (person-years)	492612	274468	248123	567261
N (persons)	150399	86295	76358	176074

Note: See note to Table 2.

Table 4. Conditional Poisson regression of frequency of sickness absence on personnel changes and control variables for men and women in the private and the public sector. Establishments and employees observed all years 2000-2003.

	Private sector		Public sector	
	Men	Women	Men	Women
	b	s.e.	b	s.e.
Year				
2001	0.025*	0.048**	0.029	0.031**
	(0.011)	(0.014)	(0.016)	(0.010)
2002	0.036**	0.017	0.064***	0.039**
	(0.012)	(0.015)	(0.015)	(0.012)
2003	0.056***	0.056***	0.080***	0.045***
	(0.012)	(0.015)	(0.018)	(0.012)
Change in no. of employees (<8% change omitted)				
Y2001 x 8-18% reduction	-0.010	-0.017	0.048	-0.041
	(0.027)	(0.035)	(0.061)	(0.030)
Y2002 x 8-18% reduction	-0.018	-0.023	0.032	-0.043
	(0.029)	(0.039)	(0.052)	(0.033)
Y2003 x 8-18% reduction	-0.076*	-0.076	0.118	-0.008
	(0.032)	(0.040)	(0.060)	(0.031)
Y2001 x >18% reduction	0.006	-0.011	0.067	-0.005
	(0.044)	(0.042)	(0.087)	(0.043)
Y2002 x >18% reduction	0.046	0.058	0.096	-0.042
	(0.039)	(0.043)	(0.074)	(0.040)
Y2003 x >18% reduction	-0.051	0.012	0.146*	0.033
	(0.061)	(0.049)	(0.074)	(0.040)
Y2001 x >8% growth	-0.024	-0.014	-0.003	0.000
	(0.027)	(0.030)	(0.036)	(0.023)
Y2002 x >8% growth	-0.017	0.027	-0.011	0.012
	(0.027)	(0.033)	(0.040)	(0.026)
Y2003 x >8% growth	-0.036	-0.001	-0.023	-0.011
	(0.029)	(0.032)	(0.034)	(0.027)
Working hours per week (>30 h. omitted)				
<10 hours	-0.203**	-0.150***	-0.134**	-0.117***
	(0.059)	(0.037)	(0.051)	(0.021)
10-30 hours	-0.107	-0.072*	-0.116*	-0.074***
	(0.065)	(0.030)	(0.047)	(0.017)
Log likelihood	-92550.4	-50001.1	-46867	-80921.1
N (person-years)	151448	76804	80216	124460

Note: See note to Table 2.

Table 5. Conditional Poisson regression of the number of sickness absence days on personnel changes and control variables for men and women in the private and the public sector. Establishments and employees observed in all years 2000-2003.

	Private sector		Public sector	
	Men	Women	Men	Women
	b	s.e.	b	s.e.
Year				
2001	0.101***	0.073**	0.134***	0.103***
	(0.019)	(0.024)	(0.027)	(0.019)
2002	0.177***	0.124***	0.223***	0.171***
	(0.021)	(0.027)	(0.024)	(0.019)
2003	0.318***	0.262***	0.411***	0.324***
	(0.021)	(0.027)	(0.030)	(0.018)
Change in no. of employees (<8% change omitted)				
Y2001 x 8-18% reduction	-0.022	0.027	0.096	0.031
	(0.053)	(0.058)	(0.084)	(0.052)
Y2001 x >18% reduction	-0.007	0.037	0.163	-0.068
	(0.089)	(0.073)	(0.108)	(0.086)
Y2001 x >8% growth	0.016	0.017	-0.042	0.029
	(0.048)	(0.057)	(0.065)	(0.040)
Y2002 x 8-18% reduction	0.047	-0.038	0.149	-0.039
	(0.054)	(0.070)	(0.092)	(0.057)
Y2002 x >18% reduction	0.076	-0.034	-0.090	-0.064
	(0.071)	(0.077)	(0.147)	(0.074)
Y2002 x >8% growth	-0.035	0.087	-0.009	0.028
	(0.050)	(0.056)	(0.071)	(0.041)
Y2003 x 8-18% reduction	0.066	0.003	0.180**	0.005
	(0.049)	(0.056)	(0.077)	(0.050)
Y2003 x >18% reduction	0.011	0.060	0.126	-0.069
	(0.072)	(0.099)	(0.139)	(0.068)
Y2003 x >8% growth	-0.032	0.057	-0.047	0.012
	(0.052)	(0.058)	(0.068)	(0.039)
Working hours per week (>30 h. omitted)				
<10 hours	-0.617***	-0.510***	-0.776***	-0.566***
	(0.094)	(0.058)	(0.082)	(0.032)
10-30 hours	-0.338**	-0.218***	-0.409***	-0.259***
	(0.112)	(0.048)	(0.088)	(0.028)
Log likelihood	-2243926	-1476059	-1221599	-2553676
N (person-years)	151448	76804	80216	124460

Note: See note to Table 2.

Even in the public sector, there are no significant relationships with absence frequency for women. For men, there is a significant and positive coefficient, implying that major downsizing is associated with a 15 percent increase in the number of sickness absence episodes. Inspection of the coefficients for the major downsizing by year interactions for earlier years suggests that the difference between this group and the comparison group of stable organizations has grown gradually over the period of observation: The coefficient is 0.07 for 2001, 0.10 for 2002 and 0.15 for 2003. Thus, there is no clear change in or after the downsizing year, as one might expect under the hypothesis of a true downsizing effect (although anticipation effects could be an explanation). On the other hand, we may note that the coefficient for the moderate personnel reductions group in 2003 is also negative, but not significant. There is some evidence, therefore, that downsizing may have different effects for men in the two sectors, but at any rate the relationships are weak.

Table 5 provides results for number of days lost due to sickness. Again very few significant coefficients are found. Moderate personnel reductions are, however, positively related to the number of sickness absence days in 2003 for men in the public sector, with a coefficient of 0.18. Even in this case, the difference between the treatment group (in this case the moderate personnel reduction group) and the comparison group seems to develop gradually, with a coefficient of 0.10 in 2001, 0.15 in 2002 and 0.18 in 2003. The coefficient for the large personnel reduction group in 2003 is also positive (0.13), but far from reaching statistical significance.

With the possible exception of men in the public sector it seems reasonable to conclude that the analyses of the restricted sample largely confirm the overall picture of near zero relationships between downsizing and upsizing on the one hand and sickness absence on the other.

Discussion

As noted in the Introduction, the available evidence on the relationship between downsizing and sickness absence is limited and inconclusive. Much of the most solid evidence of a positive relationship between these variables is based on studies of Finnish municipal employees (e.g., Vahtera et al., 2004), and the degree to which these results can be generalized to other populations is not obvious. The results presented in this paper are broadly consistent with two Swedish general population studies in finding little evidence that downsizing leads to increased sickness absence (Theorell et al., 2003; Westerlund et al., 2004).

We applied two different approaches to the estimation of downsizing and upsizing effects, one based on including all available observations, and the other one defining cleaner and more distinct treatment and comparison groups. The findings are consistent in showing generally very weak, relationships between downsizing and sickness absence.

The only exception from the overall picture of (numerically) very small coefficients are two results for men in the public sector. In the analyses of the restricted sample, there were indications of increased sickness absence in either the major downsizing group (for the number of absence episodes) or the moderate downsizing group (for the number of absence days). This might be taken to indicate that downsizing has different effects in the private and the public sectors. This can not be ruled out, but the evidence is at best very weak. The standard errors for the coefficients in question are quite large, and no trace of a similar pattern is found in the analyses of the full sample (Tables 2 and 3). Also, in both cases there was a trend toward increased absence throughout the years 2000–2003, and no clear change associated with the downsizing as such. It is also noteworthy that no evidence of similar effects are found for women. It is certainly possible that men and women are affected differently in downsizing processes, but it is not obvious why men should be more affected than women specifically in the public sector.

Methodological strengths of the present study are large sample sizes (particularly with regard to the purely register based analyses) and the use of conditional regression methods, which eliminates any confounding due to time-invariant omitted variables. There are still important methodological limitations, however. For one thing, data over four years is not sufficient to completely rule out different long term trends in sickness absence in the downsizing, upsizing and stable categories. Secondly, only short term effects of downsizing and upsizing have been examined. A third issue is that the possible cumulative effects of repeated rounds of downsizing have not been considered (cf. Westerlund et al., 2004). A fourth potential problem is selection effects. To some extent, selection effects are inherent in any study of sickness absence, since individuals with poor health or low job motivation are probably more likely than others to leave employment, and thus the population studied. In a study of downsizing 'survivors', it could be argued that this problem is exacerbated, since not only those employees who leave the labour force are selected out the population under study, but even those who leave for other organizations. Previous Norwegian evidence indicate, however, that exit probabilities are not more strongly related to previous sickness absence in periods of downsizing than in non-downsizing periods (Henningsen & Hægeland, 2008).

Although methodological factors cannot be ruled out, we think that the overall pattern of weak or non-existent links between downsizing or upsizing and increased sickness absence is better explained by referring to substantial phenomena. We may distinguish between two main explanations. One possibility is that downsizing and reorganization/upsizing do not in general cause serious health problems for remaining workers. As noted above, reviews of the literature have concluded that there are important negative health effects, but there are also important diverging findings. In particular, a recent large Finnish study finds absolutely no evidence of an effect of downsizing on either all-cause or cause specific mortality (Martikainen, Mäki and Jäntti, 2008).

Another possible explanation is that downsizing (e.g. through its effect on work demands and job insecurity) simultaneously causes deteriorating health and attendance pressure for remaining workers. If so, then these two mechanisms may in sum cancel each other out, and deteriorating health among remaining workers will consequently not be reflected in increased sickness absence. If this were the case, however, one might expect more evidence of a positive relationship between downsizing and sickness absence in the public than in the private sector, since public sector employees have a higher level of job security. For men there were a couple of indications of such sector differences, but no consistent pattern, and for women no evidence of similar sector differences was found.

The present study is consistent with Swedish studies that also indicate that downsizing does not lead to strong increases in sickness absence. The degree to which these findings can be generalized to other countries is not obvious, however. Both Sweden and Norway are characterized by relatively low unemployment, strong unions and quite extensive worker protection legislation. It is not unlikely that downsizing may have stronger effects in countries lacking these institutional characteristics.

Conclusion

Although it is generally assumed that downsizing leads to increased sickness absence, a more detailed look at the available evidence indicates that there is no strong support for this. The present study provides a quite consistent picture of very weak if any effects of downsizing on sickness absence. Moreover, the same is found to hold for upsizing. Although the lack of downsizing effects is perhaps more striking in the present study, these Norwegian findings are quite similar to what has been found in previous studies in neighbouring Sweden.

The possibility that these findings are driven by methodological factors can not be completely ruled out, and neither can the possibility that adverse health effects are counterbalanced by increased attendance pressures. The interpretation that seems most reasonable to us, however, is that downsizing does not in fact have strong effects on sickness absence. Even if this interpretation is accepted, however, it is not clear that this can be generalized to countries with less extensive welfare and worker protection arrangements.

Notes

[1] The estimated effect measured in number of spells (18) divided by average number of spells for the same employees before downsizing (83).

[2] Some studies attempt to control for such heterogeneity by adjusting for pre-downsizing sickness absence in a regression model. This involves very unlikely assumptions about the underlying causal processes, however (Allison, 1990). In addition, earlier sickness absence will be a very imperfect measure of health, and the adjustment will only be partial.

[3] The fact that stable predictors (like e.g. gender) drop out of the regression may of course also be a disadvantage if such stable characteristics are not only potential confounders but variables of substantive interest. This is not the case in the present paper, however, since we focus on the impact of personnell changes.

[4] The *xtpqml* program written by Timothy Simcoe and available as an addition to Stata is used.

[5] Thus, it identifies the 'average treatment effect on the treated' (ATT) under the assumption that the two groups would have experienced the same trends in the absence of treatment.

[6] Coefficients in Poisson models are often generally referred to as rate ratios. This is not correct with regard to coefficients for interaction terms, however, and we do not use this term in the tables. Coefficients for two-way interactions (like β_3) are rather to be interpreted as ratios of rate ratios.

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