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Workplace size and sickness absence transitions

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Workplace size and sickness absence transitions[*]

by

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Abstract

This study examines how workplace size relates to transitions in- and out-of sickness absence. Overall, the study finds important differences in the long-term sickness absence behavior of individuals working in small and large workplaces. In particular, the results show that the sickness spells are of higher incidence, but somewhat shorter duration in large workplaces. However, the results also show that the strength of these relationships varies across different labor market groups. The analysis is based on rich administrative data from Sweden over the period 1994–2008.

Keywords: Sickness absence, workplace size, hazard model.
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1 Introduction

The social and economic costs of sickness absence loom large in most Western countries. The OECD put the average cost of sickness benefits alone at 0.8 percent of GDP in 2005 across its member states. This makes sickness spending comparable to unemployment benefit spending for this group of countries (OECD 2009, p. 36). If we were to add the costs incurred by employers and individual workers the sickness absence bill would multiply. It should therefore come as no surprise that bringing down excessive sickness rates has surfaced as a top priority for many national governments in recent years.

The increased political salience of the issue together with better data availability has also spurred increased scholarly attention to the problem of sickness absence. Over the last decades, social scientists of various strands have studied the importance of social insurance institutions (Johansson and Palme 1996; 2005), demographics (Barmby et al. 2002; Markussen et al. 2011), socio-economic position (Bäckman and Palme 1998; Hansen and Ingebrigtsen 2008), and life style factors (Bush and Wooden 1995) in explaining levels and trends in sickness absence rates within as well as across countries.

The factor receiving the most attention in previous research on the topic, however, is that of working conditions (Allebeck and Mastekaasa 2004, p. 50). One workplace characteristic that has frequently been reported to be associated with overall sickness absence rates is the size of the workplace. Findings of a positive relationship between establishment (or firm) size and individual sickness absence have been reported for a large number of countries (e.g., Allen 1981; Barmby and Stephan 2000; Dionne and Dostie 2007; Ose 2005).

Available empirical evidence thus suggests the existence of a robust positive correlation between workplace size and sickness absence rates. Nevertheless, there is still considerable uncertainty both on the mechanisms underlying this empirical regularity and on whether it could be given a causal interpretation. The present study primarily deals with the first of these issues by addressing the question of whether the positive correlation between workplace size and sickness absence rates is due to a higher incidence or longer duration of sickness absence spells in large workplaces.

To my knowledge there are no previous systematic studies that have examined how the size of the workplace relates to inflows and outflows into sickness absence.[1] This means that we do not know whether the size-absence correlation reported in previous studies is driven by the fact that workers in large workplaces are more likely to *become* sick or by the fact that they are more likely to *stay* sick (once they have become sick).

It seems important to try to disentangle these two possibilities, both from a theoretical and a policy perspective. Theoretically, better knowledge on this issue could help us decide between various competing explanations for the existence of a positive correlation between workplace size and sickness absence rates. With regard to policy-making, it seems likely that the relative effectiveness of different policy-measures aimed at reducing sickness absence rates in large workplaces could depend on whether the size-absence relationship is due to a higher frequency or longer duration of sickness absence spells in large workplaces.

The aim of this study is therefore to study how the size of the workplace relates to transitions in and out of sickness absence. For this purpose, the empirical analysis utilizes an extensive data set covering all sickness spells that include at least some payments from the Swedish Social Insurance Agency over the period 1994–2008.[2] The richness of these data enables me to contribute to previous research on the topic in, at least, three ways.

First, and most importantly, because I have access to spell data I can describe how workplace size relates to both the incidence of duration of long-term sickness absence spells. Second, due to the large sample size it is possible to examine to what extent the nature of the size-absence relationship is uniform or whether it varies across different labor market groups. Third, and finally, because the data is of a multi-spell nature and contain detailed information on various worker and establishment characteristics it provides unusually good opportunities to address the problem of non-random sorting of workers across workplaces, which constitutes the main threat to causal interpretation in this setting. In particular, the occurrence of multiple sickness spells will be used to take unobserved heterogeneity at the individual and workplace level into account. Although

[1] Markussen et al. (2011) is a partial exception in this regard, since they include a dummy variable for firms with less than 20 employees among their controls when studying sickness absence transitions in Norway. The authors, however, do not comment on the importance of this variable in their article.

[2] For most years this means that data contain infromation on all sickness spells lasting for at least two weeks, but for a few years the required minimum length was extended to three or four weeks (more about this later).

this approach fall considerably short of a randomized controlled trial it nonetheless constitutes an important improvement on most previous research on the issue. The study thus takes us at least one step closer to answering the question of whether the size-absence correlation could be given a causal interpretation.

Nevertheless, the main contribution of the study lies with the description of how the incidence and duration of sickness absence spells differ across workplaces of different size. Overall, the results reveal important differences in the sickness absence histories of workers in small and large workplaces that remain also when accounting for unobserved heterogeneity at the the individual and workplace level. More precisely, the results show that the sickness spells are of higher incidence, but somewhat shorter duration in large workplaces. To the extent that these findings could be thought to generalize to countries outside Sweden, they suggest that the positive relationship between workplace size and sickness absence rates is mainly driven by a higher incidence of sickness absence in large workplaces. At least as long as we restrict attention to sickness spells of similar duration to those studied here.

Having said that, the empirical analysis also indicates that substantial heterogeneity exists in the strength of the size-absence relationship. Most importantly, the relationship between long-term sickness absence behavior and workplace size appears to be more pronounced in the private than in the public sector. Explaining the reasons for these differences, however, is left for future research.

The paper is organized as follows. Section 2 provides an overview of previous research on the topic. Section 3 describes the data and the institutional setting. Section 4 discusses the empirical specification. Section 5 presents the empirical results. Section 6, finally, concludes.

2 Theoretical background and previous research

The literature on the relationship between workplace size and worker absence has a long history. As early as 1948 Kossoris expressed his surprise of finding a strong positive relationship between establishment size and illness absenteeism in the US manufacturing industry (Kossoris 1948, p. 266). The reason for the surprise was that the finding of a positive correlation between size and absence was seen as contradicting the perceived wisdom that the conditions of workers tend to improve with the size of the workplace (e.g., Moore 1911; Davies et al. 1995, p. 312). Kossoris initial finding was, however, soon buttressed by a number of other studies, and in an early review of literature on employee absenteeism it was noted that the "Research relating work unit size to absenteeism has been very consistent: the larger the work group, the greater the absenteeism" (Muchinsky 1977, p. 329).

In the 1960s and 1970s social psychologists and sociologists advanced different explanations to account for the observed correlation between workplace size and absenteeism. Typically, these explanations centered on some notion of worker satisfaction or morale and assumed a negative relationship between these factors and the size of the workplace (see Allen 1982 for an overview). A particular influential explanation along these lines was suggested by Geoffrey Ingham who argued that the absence rate among workers varies inversely with the level of morale involvement and identitive power in an organization, and that workers in large workplaces – due to the greater degree of impersonality and bureaucracy associated with large organizations – are considerably less likely to develop these traits than are their counterparts at small work sites (Ingham 1970).

A common criticism against explanations focusing on worker attachment and morale was that these factors could only help explain short-term uncertified work absence, but not the cases of absence due to genuine sickness, which, at the time, made up the great bulk of the overall absence rate in most firms. To this Ingham responded that the distinction between uncertified and certified absence was not very meaningful:

> First, such a distinction neglects the factor of psychosomatic illness, which may be at least partly induced by a depriving work situation. Similarly, it becomes difficult to deal with the size-accident relationship if short *deliberate* absences are viewed as the most important measure. Furthermore, I would like to suggest that the actual length of 'illness' may be, to some extent, deliberately calculated: that is to say, dissatisfied workers may be more reluctant to return to worker after illness (Ingham 1970, p. 20).

Consequently, according to Ingham worker satisfaction and attachment should be of explanatory value for short-term and long-term cases of absence alike.

The interest for the size-absence relationship waned among sociologists and social psychologists during the 1980s and 1990s, but has resurfaced in economics in the last decade. An article by Barmby and Stephan named “Worker absenteeism: why firm size may matter” constituted an important impetus for the renewed interest in the issue. In this article the sociological explanations for the firm size effect on absence, discussed above, were criticized for being difficult to quantify and falsify (Barmby and Stephan 2000, p. 569). Instead, Barmby and Stephan, building on a conjecture of Coles and Treble (1996), offered a labor demand explanation for the observed correlation between size and absence.

More precisely, the authors developed a formal model in which larger firms find it optimal to have higher absence rates in equilibrium since they face lower unit costs of absence. The variation in absence costs, it is argued, arises from complementarity in production; that is, because the output of one worker often depend on the output of other workers, if a worker is absent it is not only the production of that worker that is lost, but it will also negatively affect the productivity of his or her colleagues. Barmby and Stephan maintained that large firms, due to economies of scale, are able to insurance against the risk associated with a given level of complementarity more cheaply than are small firms.

Much of the subsequent literature has focused on employee monitoring as a means for employers to achieve the desired level of absenteeism (Heywood and Jirjahn 2004; Heywood et al. 2008; Lanfranchi and Treble 2010). Employers with high absence costs, it is argued, tend to devote more resources to monitoring and enforcement, which reduces absenteeism in these firms. A problem with explanations focusing exclusively on monitoring, however, is that they could be criticized on the same grounds as the sociological explanation of Ingham and others. That is, explanations of this type seem more apt to explain differences in short-term uncertified absence than in certified absence due to genuine illness.

Nonetheless, to the extent that employers, due to their size, face different absence costs this should provide employers with different incentives to invest in other absence reducing technologies as well. Firms with high absence costs could not only be assumed to spend more resources on monitoring but also on different kinds of health promoting activities thought to affect long-term absence rates. In its general form the absence costs argument should therefore be applicable to short-term and long-term absence alike.

A more important issue, in my view, and one that has not been discussed in the previous literature is whether we should expect workplace size to have a similar effect on inflows to and outflows from sickness absence. Ingham maintained that we should expect this to be the case. Due to their lower level of moral commitment, workers in large workplaces should be both more likely to become absent to begin with and more reluctant to return to work once absent (Ingham 1970, p. 20).

However, the predictions of the Barmby and Stephan model seem less clear in this regard. Although it follows from their argument that smaller work units would have larger incentives to adopt various techniques to reduce the inflow into sickness absence, it is not obvious that higher absence costs imply greater incentives for small firms to provide absent workers with the opportunity to return to work. On the contrary, it might be conjectured that since absenteeism is more costly for small firms, and that past absence is often a good predictor of future absence, small firms could be more reluctant to welcome back absent workers than are their larger counterparts.

More formally, we could think of this as a situation in which the health status (or work morale) of a worker upon employment is private information that is later fully or partially revealed by an individual's absence behavior. Frequently occurring or long absence spells might then induce the employer to lower his or her beliefs about the health status (work morale) of a worker. And because the costs to insure against work absence are higher for employers in small workplaces they could be expected to face greater incentives to try to replace absence-prone workers.

This is not to say that small firms are necessarily more likely to fire workers on certified absence, they may only be less likely to carry the costs for rehabilitation or for changing the work environment or work tasks in a way that enables absent workers to quickly return to work.[3] To the extent that this is the case, this could actually give rise to a negative relationship between workplace size and sickness absence duration.

The upshot of this argument is that it is not obvious that we should expect workplace size to affect the incidence and duration of sickness absence spells in the same way. Although workers in small workplaces are less likely to become absent in the first place, they could also be less likely to return to work once they become absent, either because

[3] This mechanism should be further strengthened by the fact employer absence costs, both the direct ones such as sick-payments and the indirect ones, such as hiring and training replacement workers, are highest in the beginning of an absence spell.

they have longer absence duration or because they are more likely to leave (or to be separated from) their jobs while absent.

In addition, workplace size may also carry different importance for different labor market groups. For instance, recent research on Swedish data shows that there is important variation in sickness absence behavior between women and men as well as between different economic sectors (Angelov et al. 2011). Likewise, there are related studies showing that the strength of the correlation between wages and workplace size varies across industries and sectors **(**Belman and Heywood 1989**;** Lallemand et al. 2005), and that the size-wage premium is lower for women than for men (Oi and Idson 1999, p. 2177). Although the exact reasons for these differences are still rather poorly understood, results such as these suggest that the relationship between workplace size and sickness absence behavior may be less uniform than has previously been acknowledged.

In sum, there is a relatively large cross-disciplinary literature dealing with the relationship between workplace size and sickness absence. By now there exist a number of contemporary empirical studies supporting earlier findings of a positive relationship between workplace size and absence rates (Barmby and Stephan 2000; Dionne and Dostie 2007; Heywood et al. 2008; Ose 2005; Vistnes 1997). Both the estimation methods and the type of data used differ a great deal across these different studies. A common denominator of most previous studies in the field, however, is that the main focus has been on how employer size relates to the total number of (sickness) absence days over a particular period. That is, the interest has been with the direction and strength of the statistical association between workplace size and overall absence rates.

However, in principle a positive relationship between workplace size and the overall sickness absence rate could arise for, at least, four different, but not mutually exclusive, reasons. First, the correlation could be due to a higher frequency of absence spells in large work sites. Second, it could be due to a longer duration of absence spells in large work sites. Third, it could be the combination of high frequency and long duration. Fourth, and finally, the correlation could be driven by non-random sorting of workers across employers of different size. That is, absence-prone workers could either be less likely to be hired by small employers to begin with, or, as suggested above, they may be more likely to leave their employers during absence. In either case we would expect absence rates to increase with employer size.

In order to interpret the observed size-absence relationship it is necessary to disentangle these different channels through which the association can arise. Admittedly, the problem of non-random sorting has received a fair amount of attention in the previous literature. For instance, in a well-crafted study using longitudinal linked employer-employee data Dionne and Dostie (2007, p. 119) showed that controlling for various types of worker characteristics severely weakens the positive relationship between firm size and the number of yearly absence days (although the relationship remains statistically significant).

In comparison, considerably less attention has been paid to the issue of how workplace size relates to the transitions between work and sickness absence. To some extent this is to be expected since this type of analysis places great demands on data, i.e., it is required that we can observe actual sickness spells rather than merely the aggregate number of sickness days. Yet, studies of this type are necessary if we are to understand the mechanisms that underpin the observed correlation between workplace size and worker absence.

Towards this end, the rest of this article undertakes a detailed empirical analysis of the relationship between workplace size and (long-term) sickness absence using sickness spell data from Swedish registers over the period 1994–2008.

3 Data and institutional setting

The dataset used for the analysis combines information from different administrative registers and include records on sickness absence, employment history, and various demographic characteristics for all Swedes aged 16–64 over the period 1994–2008.

Data related to sickness absence come from The Swedish Social Insurance Agency and contains a complete account of all sickness absence spells for which the individual was entitled to sickness benefits from the social insurance system. During the study period the replacement rate was 80 percent of previous income (up to a ceiling), except for the years 1996–1997 when it was 75 percent, and the benefits could, at least in principle, be paid for an unlimited period of time.[4] All residents with an annual estimated earned income above a certain minimum level (less than 2000 dollar per year for the entire period) are entitled to income related sickness benefits.

For most of the period the employers have been obliged to provide sick pay for the first two weeks of an absence spell. The exceptions from this general rule were the periods between January 1997 and Mars 1998, when employers had to provide sick pay for four weeks, and the last two quarters of 2003 when employers carried the financial responsibility for the first three weeks of worker absence.

One reason why the length of employer responsibility is important is that for a sickness absence spell to be included in the administrative registers it must include some payments from the national insurance agency. For most years, the data thus contain an account of all sickness spells lasting for at least two weeks, but for a few years the required minimum length is extended to three or four weeks.

The fact that we cannot observe sickness spells shorter than two weeks is obviously a limitation of the data. In particular this means that we cannot know whether the findings of the study can be generalized to short duration sickness spells. Consequently, to the extent that we are not willing to assume that short-term and long-term spells are governed by identical behavioral processes we need to acknowledge that this study deals with the relationship between workplace size and (relatively) long-term sickness absence, rather than with the relationship between size and sickness absence in general. This is a limitation the present study shares with related studies using data from public registers to study sickness absence behavior (e.g., Nordberg and Røed 2009; Markussen et al. 2011). However, the fact that the most popular explanations for the size-absence relationship are applicable to short-term and long-term absence alike serves to reduce the severity of this problem.

Granted that the absence spell lasts the required amount of time the administrative registers provide us with high quality information on start and end dates of the spell, daily benefits amounts, and individual employment status at spell beginning. Given the purpose of the study, however, these sickness spells need to be complemented with information on work spells. More precisely, I will follow previous work in the area and

[4] Restrictions on the maximum duration of sickness benefits payments were put in place in July 2008 but these do not affect the present analysis. In addition to public insurance benefits, sickness absent workers may also receive additional compensation from collective sickness insurances (Hesselius and Persson 2007).

study work duration (i.e., the time until relapse into sickness absence) for those individuals that return to work when exiting sickness absence (e.g., Broström et al. 2004; Nordberg and Røed 2009). Practically speaking this means that I study how the size of the workplace relate to the likelihood of relapse into sickness absence.

The duration of work spells are somewhat more difficult to measure than are the duration of sickness absence spells. A first problem is to decide whether an individual returns to work at the end of his or her sickness spell, since this information is not contained in the sickness absence registers. Here this problem is handled by using earnings information from the tax records. These data contain information on the annual earnings received by an individual from each of his or her employers together with information on the first and last remunerated months in these employment relationships.

For the purpose of the present study an individual is said to return to employment at the end of a sickness spell if the earnings data show that the individual was remunerated by an employer for at least two months following the end of the sickness spell. That is, if a sickness spell ended at July 15 a particular year the individual is coded as returning to work if the earnings data show that he or she received earnings from one or more employers in July and August that year. If an individual is not remunerated for the next two months following the end of the sickness spell he or she is said to have left sickness absence for other reasons, which would include things such as retirement, unemployment, or change from sickness to disability benefits. A work spell is thus taken to start on the date an individual return to work after a previous sickness spell.

Likewise, it is assumed that work spells can end for either of two reasons. First, an individual can relapse into sickness absence in which case the work spell ends at the date of the beginning of the new sickness spell. Second, the work spell could end for other reasons, which could include things such as retirement, unemployment, and withdrawal from the labor force. This is taken to happen when we observe a gap in the remuneration period for an individual. For instance, if an individual return from sickness in January 15 a particular year and the earnings data show that the individual received remuneration from January to May that year, but not in June, the work spell is coded to have lasted from January 16 to May 31 (regardless of what happens at later time periods).

Admittedly, this approach to date work spells and code different exit reasons is somewhat blunt and will no doubt err in some cases. For instance, we might slightly overestimate the number of individuals returning to work after a sickness spell since an individual can receive earnings from an employer for a few months following the end of an employment relationship. Or, some of the gaps that are used to mark the end of a work spell will be imaginary rather than real since they can occur when an individual changes job and the individual does not receive his or her first wage payment until the second month on the new job. That said, it seems unlikely that the measurement errors caused by these problems should bias the results, since there is little reason to expect the measurement errors to be correlated with the size of the work units.

Another difficulty is that sickness absence can be part-time[5] as well as full-time and many absence spells include periods of both types. In order to keep the empirical analysis tractable I will not attempt to distinguish between full-time and part-time absence, instead a worker is considered absent whenever he or she receives at least some sickness benefits for a particular day. This choice, obviously, has bearing on how we interpret the transitions between work and sickness absence. In particular, in this study a worker is said to have transited into sickness absence on the day he or she receives sickness benefits (on full- or part-time) from the Social Insurance Agency whereas he or she is said to have transited out-of sickness absence when he or she no longer receives any sickness benefits payments. In the empirical analysis to follow a worker that combines part-time work with part-time sickness absence is thus considered to be sickness absent. This is also the approach taken in the related study of Markussen et al. (2011).

Admittedly, disregarding the distinction between part- and full-time sickness absence might have consequences for the results if the relative incidence of part- and full-time spells differ across workplaces and spell duration depends on whether they are part- or full-time. Nonetheless given that full-time days constitute the great majority of all sickness absence days during the period under study, I do not consider this to be a severe problem.[6] On the other hand, since a number of sickness spells contains periods

[5] More precisely, part-time absent workers can receive benefits for ¾, ½, or ¼ of a day.
[6] Figures published at the webpage of the Swedish Social Insurance Agency show that the share of full-time sickness absence days, as a percentage of all sickness absence days, ranged from 63 to 75 percent over the years 1994 to 2008.

of both part- and full-time absence attempts to differentiate between part- and full-time sickness absence would create a number of new, and likely more severe, problems.

In a few cases sickness benefits are accompanied by other forms of insurance payments from the social insurance agency, such as for instance work accident benefits. In the following I consider all absence spells that include at least some payments in the form of “standard” sickness benefits.

The data on sickness spells are then merged with other administrative registers containing information on various employee and employer characteristics, recorded on a yearly basis. For the employees I have gathered information from the income and population register (LOUISE) on things such as sex, age, education, martial and immigration status, wage, and place of residence. For the employers information on firm and establishment size, industry belonging, and geographic locality where collected from Statistics Sweden’s Business Register. The subsequent analysis will thus be based on a longitudinal linked employer-employee data set in which we can follow both the individuals and their employers over time.

Finally some additional restrictions will be placed on the data. Since the focus of the paper is to examine how the size of the workplace relates to transitions in and out of sickness absence I only include spells in which the individual was employed at spell start. Because the sickness benefits rules differ between employed and self-employed workers the self-employed will be excluded from the subsequent analysis.

Moreover, given the nature of the data not all spells will have an end date. To handle this problem all sickness and work spells that are still ongoing at January 1st 2009 are right censored at that time. In addition, since the tax registers used here only contain information on individuals until they turn 65 all sickness and work spells are right censored the 1st of January the year an individual is to turn 65.

Table 1 Descriptive statistics

Variables	Sickness spells	Work spells
Age	44.26	44.45
	(11.61)	(11.45)
Female	0.63	0.63
	(0.48)	(0.48)
Years of education	11.52	11.53
	(2.55)	(2.55)
Establishment size	440.37	453.83
	(1178.64)	(1187.70)
Firm size	6281.05	6405.38
	(11466.45)	(11517.54)
Daily income (SEK)	500.30	593.64
	(138.93)	(328.52)
Immigrant	0.15	0.14
	(0.35)	(0.35)
Married	0.49	0.50
	(0.50)	(0.50)
Child under 11	0.26	0.26
	(0.44)	(0.44)
Local unemployment	4.67	4.72
	(2.15)	(2.11)
Public sector	0.48	0.49
	(0.50)	(0.50)
Exit type		
Resumption/Relapse	0.92	0.60
Other	0.06	0.15
Censored	0.02	0.25
Spell length (days)		
Average	149	892
Median	44	536
Number of spells	5,383,155	4,506,157
Number of individuals	2,450,889	2,130,379
Number of establishments	322,510	284,370
Number of firms	233,262	199,882
Fraction of individuals with multiple spells	0.52	0.50

Table 1 provides some descriptive statistics divided by spell type. In total almost 2.5 million employed individuals experienced at least one long-term sickness absence spell during the study period. More than 90 percent of the absence spells eventually ended with work resumption. However, the risk of sickness relapse is substantial, 60 percent of the work spells ended due to the onset of a new sickness absence spell. As a result of this the fraction of the individuals experiencing multiple sickness and work spells is fairly high (around 50 percent).

Another thing to note is that most sickness absence spells are of a rather short duration. The median spell length is about one and a half month and the median duration of the subsequent work spells is about a year and a half.[7] However, for both types of spells average duration is considerably longer than median duration indicating that the both spell distributions are heavily right-skewed.

4 Empirical specification

As mentioned before, the dominant methodological approach in previous research on the subject has been to study how the aggregate number of sickness absence days differs across workplaces of different size. However, given that the aim of this study is to examine how the size of the workplace relates to inflows and outflows into sickness absence this “stock approach” will not suffice. Instead I will follow a related strand of the literature that has utilized multivariate hazard models to study sickness incidence and recovery for different types of workers (e.g., Lindeboom and Kerkhofs 2000; Broström et al. 2004; Nordberg and Røed 2009; Markussen et al. 2011).

In most important respects the set-up of my statistical model mimics that of these previous studies. At each point in time an individual is assumed to be in either of two states: working or sickness absent. Moreover, and as discussed in the previous section, work spells and sickness spells can terminate for different reasons. More precisely, I will here differentiate between those sickness spells that end because the individual returns to work and those that end for other reasons, and between the work spells that end due to a relapse into sickness absence and those that end for other reasons.

The empirical model to be estimated thus contains four different transition rates (combining two states and two exit types). In line with common practice I take the transition rates to be of the proportional hazard type:

$$h_i^{s,c}(t|\boldsymbol{X}_i) = h_o^{s,c}(\mathrm{t})\exp(\boldsymbol{X}'_{it}\boldsymbol{\beta}^{s,c}), \qquad (1)$$

where i denotes individuals, t indicates (continuous) time, $h_o(t)$ is the so-called baseline hazard, $\boldsymbol{X}_{it}$ is a covariate vector, and $\boldsymbol{\beta}$ is a vector of regression coefficients, and the superscripts s, c denotes the states (work and absence) and the exit causes

[7] The length of the spells is computed using censored and uncensored durations.

(relapse/return and other), respectively. The hazard rate in equation (1) is known under different names in the literature, such as the crude or cause-specific specific hazard rate. Informally, it describes the instantaneous risk ("probability") that a spell of type s ends from cause c in the small interval between t and $t + \Delta$, provided that the spell has not ended, for whatever reason, before time t.

The thrust of the theoretical arguments discussed in the previous section is that sickness incidence and recovery of a particular individual will vary with the size of his or her workplace. Ideally, we would therefore like to observe how a change in workplace size affects the various hazard rates in equation (1) keeping worker characteristics as well as all workplace factors that are not a direct function of workplace size fixed. Or to state the last point more clearly, if we believe that production technology affects both workplace size and the sickness absence rates we would like to control for this factor in the analysis, whereas we would not like to control for factors which themselves can be thought to be an effect of workplace size, such as, for instance, worker satisfaction or workplace specific "absence cultures".[8]

Consequently, we would therefore like to include a rich set of relevant individual and workplace factors among the controls when estimating the hazard rates of interest. Unfortunately, not all relevant individual and workplace factors are readily available in existing data. This means that there is likely to be remaining unobserved heterogeneity both at the individual and the workplace level even after controlling for a large set of observables (see e.g., Arai and Skogman Thoursie 2004). A common way to try to account for such unobserved heterogeneity is by relaxing the assumption that the baseline hazard in equation (1) is identical for all individuals.

More concretely, the (state and cause specific) baseline hazard could be assumed to take a multiplicative frailty form:

$$h_o^{s,c}(\mathrm{t})=\alpha(t)^{s,c}\theta_i^{s,c}\delta_j^{s,c}\ , \qquad (2)$$

where $\alpha(t)$ is an unspecified function of time and θ_i and δ_j are time-invariant unobserved worker and workplace characteristics, respectively. One approach to models of

[8] This is also the reason why the interesting topic of peer-effects falls outside the scope of this study (see e.g., Hesselius et al. 2009).

this sort is to treat θ_i and δ_j as random variables with a continuous or discrete distribution. This is for instance the approach taken in the studies of Nordberg and Røed (2009) and Markussen et al. (2011). A drawback with this approach, except from having to assume a particular distribution for the unobserved heterogeneity[9], is that we typically need to assume that θ_i and δ_j are independent of the included regressors. However, if we have access to multiple-spell data, as we have here, this restrictive assumption can be relaxed by treating θ_i and δ_j as fixed constants that are either estimated directly or conditioned out of the likelihood.

A simple way to get rid of the fixed effects without having to estimate them is to use the stratified partial likelihood approach founded on the seminal work of Cox (1972). When utilizing the stratified partial likelihood approach we proceed on the assumption that all observations belonging to the same stratum (somehow defined) have an identical baseline hazard of experiencing the event of interest, whereas the baseline hazard is allowed to vary freely across strata.

If we take the workplace of an individual to refer to the establishment in which he or she works, as I will do for most parts of this study, we may thus difference out θ_i and δ_j in equation (2) by forming strata consisting of all unique individual-establishment combinations. In essence, this approach amounts to using only information about the rank ordering of spell lengths within unique individual-establishment pairs to estimate the parameters of interest (Allison 1996, p. 210). By relying solely on the within individual-establishment variation we automatically adjust for many types of unobserved heterogeneity at the individual and workplace level, such as the innate health or work morale of the individual or the relative hazardousness of the type of production that take place within a particular establishment. In fact, the stratified partial likelihood approach can handle more general forms of unobserved heterogeneity than that described in equation (2) since it allows the entire baseline hazard to vary arbitrarily across strata.

In the empirical analysis to follow, I thus use the well-known stratified Cox model to estimate the (crude) hazard rates defined in equation (1). The fact that the baseline hazard drops out of the calculations in the Cox model is an attractive feature in this case

[9] More precisely, the two studies cited in the text assume the unobserved heterogeneity terms to be jointly discretely distributed and attempts to estimate the number of mass-points for these distributions from the data.

since this means that we can allow for many different types of unobserved heterogeneity and duration dependence in the data. Or as noted by Abbring and van den Berg (2003, p. 709), for multiple-spell competing risks model of the type considered here "the stratified partial likelihood estimator provides estimates of proportional covariate effects under weak identifying conditions".[10]

Moreover, under the assumption that observed differences in (long-term) sickness absence behavior can be attributed to either observed covariates or time-invariant unobserved heterogeneity at the individual or the workplace level little is lost by estimating separate Cox models for the transitions rate of interest, rather than to estimate the hazard rates jointly. This is because the overall likelihood will then factorize into separate parts, with each part associated with a specific type of transition (Lindeboom and Kerkhofs 2000, p. 669).[11]

5 Empirical results

As a prelude to the main empirical analysis, Table 2 displays the results from regressing (yearly) sickness absence days on log establishment size for all workers that were employed in November a particular year between 1994 and 2008.

The results in Table 2 are well in line with previous research in that they indicate that workers in large establishments on average have more sickness absence days than their counterparts in small establishments. Interesting to note is that the strength of the relationship increases when either individual fixed effects (model 2) or individual-establishment fixed effects[12] (model 3) are included in the model. The observed differences also seem to be of substantive importance. For instance, to judge from the results of model 3 a worker employed in a large establishment with 1000 employees can be expected to have 2.4 more (long-term) sickness absence days in a year than a similar worker employed in a small establishment with 5 employees (the mean number of sickness days over this period was 13.2).

[10] The estimator is not without limitations, however. In fact Chamberlain (1985) who first proposed this method also raised some important theoretical problems with the method. Nonetheless, the Monte Carlo simulations conducted by Allison (1996) indicate that the model work well under a large range of circumstances.

[11] Admittedly, if one is to use the results to make individual predictions about the total time spent in sickness absence over an extended period of time we need to recover the worker and workplace fixed effects. For suggestions on how to do that see Ridder and Tunali (1999) or Lindeboom and Kerkhofs (2000).

[12] In this case we include fixed effects for each unique individual and establishment combination found in the data.

Table 2 OLS regression on (yearly) sickness absence days

	(1)	(2)	(3)
Log establishment size	0.33*	0.37*	0.46*
	(84.99)	(54.79)	(18.92)
Controls	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes
ID fixed effects	No	Yes	No
ID × est. fixed effects	No	No	Yes
Observations	53,942,871	53,520,434	48,578,383

Notes: Model 1 includes controls for age, age squared, gender, martial- and immigrant status, years of education, local unemployment, and an indicator for whether any children under the age of 11 is living in the household. Models 2 and 3 exclude gender and immigrant status from the set of controls. t-values for the coefficients are in the parentheses. *indicates that the coefficient is statistically significant at the 0.01 level.

However, in this study the main concern is not with the size-absence relationship per se, but with the relative importance of the channels underlying this relationship. In particular, we are interested in finding out whether the positive correlation between workplace size and (long-term) sickness absence rates are due to a higher incidence or longer duration of sickness absence spells in large workplaces. In order to answer this question we will now study how establishment size relates to transitions in- and out-of sickness absence by applying the multivariate hazard model described in the previous section.

5.1 Absence to work transitions

In this section we start by studying sickness to work transitions. Table 3 presents the results for sickness spell duration. In models 1–5 work resumption constitutes the event of interest and all sickness spells that ended for other reasons are censored at the date of spell ending. In model 6–10 things are reversed and all sickness spells that ended with work resumption are censored.

Table 3 Hazard rates: sickness spells

	Work resumption					Other				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
HR- Log est. size										
All workers	1.033*					0.931*				
	(134.07)					(-71.22)				
Private sector		1.056*					0.911*			
		(171.02)					(-79.96)			
Public sector		1.003*					0.990*			
		(7.47)					(-5.20)			
Male-private			1.063*	1.040*	1.021*			0.924*	0.907*	0.579*
			(153.99)	(22.48)	(3.78)			(-53.71)	(-12.69)	(-11.11)
Female-private			1.047*	1.021*	1.004			0.894*	0.896*	0.628*
			(107.40)	(13.80)	(0.79)			(-68.43)	(-15.57)	(-10.52)
Male-public			1.015*	1.012*	1.004			1.017*	0.972*	0.574*
			(26.32)	(5.42)	(0.60)			(6.65)	(-2.42)	(-8.84)
Female-public			1.000	0.994*	0.987*			0.984*	0.961*	0.623*
			(0.70)	(-4.76)	(-2.91)			(-8.05)	(-4.72)	(-8.83)
Spells	5,383,155	5,383,155	5,383,155	4,206,424	3,447,065	5,383,155	5,383,155	5,383,155	4,206,424	3,447,065
Failures	4,958,558	4,958,558	4,958,558	3,923,954	3,252,765	300,612	300,612	300,612	193,137	126,345
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Strata				ID	ID × Est.				ID	ID × Est.

Note: Models 1-4 and 6-9 include controls for age, age squared, gender, martial- and immigrant status, years of education, sectoral belonging (private or public), local unemployment, and an indicator for whether any children under the age of 11 is living in the household. Models 5 and 10 exclude gender, age, age squared, and immigrant status from the set of controls. Z-values for the coefficients on the original log hazard scale are in parentheses. *indicates that the hazard ratio is statistically different from unity at the 0.01 level.

To handle time-varying covariates spells are split at calendar year (since most data are annual).[15] Another advantage of splitting data in this way is that observation year dummies can be included in the analysis, which can be used to account for the pronounced hump-shaped trend in sickness absence rates over the period under study, as well as the fact that the length of employer responsibility for sickness pay has varied over time.[16] In the analysis of sickness to work transitions establishment size is measured at spell start.

Column (1) displays the average impact of (log) establishment size on the hazard of work resumption among all absent workers. To mitigate the problem of non-random sorting of workers across establishments of different size the model includes a comprehensive set of individual controls, such as gender, age, age squared, years of completed schooling, martial and immigration status, and an indicator for whether there are any children under the age of 11 living in the household. In addition the specification contains year- and region (county) specific effects as well as controls for the local (municipality) unemployment rate and sectoral belonging (private or public sector).

The results of column (1) show that the hazard of work resumption is increasing in establishment size. The reported hazard ratio of 1.033 indicates that a one unit increase in (log) size can be expected to increase the hazard of work resumption, at any given point in time, by about 3 percent. Or more concretely, an absent worker in a workplace with 1000 employees is estimated to have a 19 percent higher probability to return to work at a given day than an absent worker employed in an establishment with only 5 employees.[17] Compared with the impact of the other variables in the model this is, indeed, a sizable difference. For instance, the estimated difference in work resumption hazard rates (not reported here) between males and females is slightly above 5 percent.

The results presented in column (1) thus show that, on average, the time to work resumption for absent workers differs across establishments of different size. Two different questions then immediately suggest themselves. The first is whether the

[15] This convenient and frequently used method to include time-varying covariates in duration models is known as episode splitting.
[16] That is, we allow the baseline hazard of experiencing the event of interest to vary by year.
[17] The comparison between workplaces with 5 and 1000 employees will be used to throughout the text to exemplify the magnitude of the estimates. During the period of interest about 10 percent of Swedish workers were employed in establishments with less than 5 employees and another 10 percent were employed in establishments with more than 1000 employees.

relationship is uniform across sectors and workers or if the average relationship conceals important heterogeneity, and the second question is to what extent the observed differences could be given a causal interpretation.

The model in column (2) is designed to shed some light on the first of these questions, by allowing the coefficient of establishment size to differ in the private and the public sector.[18] The results indicate that the average relationship is entirely driven by workers in the private sector. Among private sector workers an increase in establishment size by one log unit is estimated to increase the work resumption hazard by more than 5 percent, whereas the corresponding figure for public sector workers is as small as 0.3 percent.

However, it could be suspected that the different role played by establishment size in the private and the public sector reflects the unequal split of male and female workers across the two sectors rather than sectoral differences per se. To investigate whether this is the case the model in column (3) relaxes the assumption that the coefficient of establishment size is the same for women and men. As can be seen, important sectoral differences remain also when allowing for separate coefficients for males and females. In the private sector we find a strong positive association between establishment size and the work resumption hazard for both genders, whereas the relationship is less pronounced in the public sector (or even non-existent for women). At the same time, the results suggest that establishment size might have a somewhat different effect on the sickness absence behavior of women and men. In both the private and the public sector we find more pronounced effects for men than for women.

Even though the model in column (3) is quite rich and substantially reduces the risk that the results are due to non-random sorting across establishments of different size it is still an open question whether the observed differences can be given a causal interpretation. In particular, one could still worry about the possibility that the results are driven by unobserved heterogeneity at either the individual or the workplace level. As discussed above I will attempt to overcome this problem by utilizing the fact that the data contain multiple-spells for many individuals. More precisely, I estimate stratified

[18] Technically this is done by adding the product of log size and sectoral beloning (0=private and 1=public) to the previous model. The results are, however, very similar if we instead estimate separate models for workers in the private and public sector.

Cox models in which the baseline hazard are allowed to vary across individuals (model 4) or across unique individual-establishment combinations (model 5).[19]

A side effect of restricting the analysis to individuals with repeated sickness spells is that the number of spells that can be analyzed drops by about a quarter (and the number of individuals by about one half). Important to note, however, is that the results in columns 1–3 look very similar for this restricted sample.

It is obvious from the estimates in columns 4 and 5 that unobserved individual and workplace heterogeneity is part of the explanation for the differences in resumption hazards across establishments of different size. More precisely, the hazard ratios for all subgroups are reduced once we take individual and workplace effects into account. In column 4, when individual specific effects are taken into account, the hazard ratios range from slightly below one for female public sector workers to 1.04 for male workers in the private sector.

In column 5, when both individual and workplace specific effects are being accounted for, the hazard ratios are reduced even further. In this case we only find statistically significant coefficients for two of the groups, and these effects go in different directions. For private sector male workers a one unit increase in (log) size is associated with an increase in the hazard of work resumption by about 2 percent, whereas the same increase in establishment size can be expected to decrease the work resumption hazard by about 1 percent among female workers in the public sector. Or more substantively, a male worker in the private is estimated to have a 12 percent higher probability of returning to work a given day if he works in an establishment with 1000 employees than if he works in an establishment with 5 employees. For a female worker in the public sector, however, the instantaneous probability of returning to work is 7 percent lower if she works in a large establishment (1000 employees) than if she works in a small establishment (5 employees).

So far we have focused on work resumption as the outcome of interest. In models 6–10 I instead focus on those spells that end for other reasons than work resumption. Such

[19] Practically speaking this mean we allow for the fact that different individuals or individual-workplace combinations may possess certain, unobserved and time-invariant, attributes that make them more or less likely to resume work at a given point in time.

other reasons could be things such as unemployment, withdrawal from the labor force, or the change from sickness to disability benefits.[20]

The model specifications of columns 6–10 are analogous to those of columns 1–5 and the overall pattern is pretty clear. Workers in large workplaces are considerably less likely to end their sickness absence spells due to other reasons than work resumption than are workers in small workplaces. Starting with the average association in column (6) we find a hazard ratio of .931, indicating that a worker employed in an establishment with 1000 employees is about two-thirds as likely to exit a sickness absence spell for other reasons than work resumption than is a worker employed in a workplace with 5 employees.

Continuing to the other columns of the table we see that the relationship again is somewhat more pronounced among workers in the private sector than among workers in the public sector, but when we stratify the baseline hazard by unique individual and establishment combinations we find very large differences in the hazard rates for all subgroups. It should, however, be noted that since only a small fraction of all sickness spells ends for other reasons than work resumption the model in column (10) places great demands on the data and the results should be interpreted somewhat cautiously. Overall we find only small differences between male and female workers when examining how the hazard of exiting sickness spells for other reasons than work resumption varies across unequally sized establishments. For both genders the likelihood of ending a sickness spell for other reasons decreases substantially with the size of the establishment.

In sum, the results in Table 3 suggest that the positive correlation between workplace size and sickness absence rates reported in Table 2 is not due to a longer duration of absence spells in large establishments. If anything, the results presented in this section suggest that absent employees are likely to return to work somewhat more quickly in large workplaces (with female workers in the public sector as a potential partial exception).

[20] Admittedly, it is somewhat inaccurate to refer to things such as unemployment or work resumption as reasons for leaving sickness absence, rather they constitute different exit types (usually the reason for leaving sickness absence is recovery from illness). Yet, since in this context the meaning should be clear I will use the terms reasons and exit types interchangeably.

5.2 Work to absence transitions

In Table 4 it is investigated how the risk for relapsing into sickness absence differs across establishments of different size. In models 1–5 relapse into sickness absence constitutes the outcome of interest and work spells that ended for other reasons (such as unemployment or withdrawal from the labor force) are treated as right censored. The statistical specifications are analogous to those of Table 3.[21]

As can be seen the hazard ratio in column (1) indicates that a one unit increase in log size is associated with an expected increase in the hazard of sickness relapse by about 3 percent, i.e., on average employees in large workplaces are more likely to relapse into sickness absence than are employees in small workplaces.

Considering the results in columns (2) to (4) we again see the tendency that the relationship between establishment size and long-term sickness absence is particularly pronounced in the private sector, whereas we find no important differences between women and men. It is interesting to note that the coefficient of establishment size, in all groups, is strengthened when we allow for unobserved heterogeneity at the individual level (model 4). One potential explanation for this is that there is a greater abundance of relatively healthy workers in large establishments, which tend to suppress the raw correlation between establishment size and the relapse hazard. If that is the case it could also help explain why the differences in work resumption hazards across unequally sized workplaces decreased once individual heterogeneity was accounted for.

Turning, finally, to the results of column (5) we can note that accounting for unobserved heterogeneity at the workplace level affects the results in some important regards. Most importantly, we see that the hazard ratios for the private and the public sector converge once we rely solely on the within individual-establishment variation to estimate the coefficients of interest. In addition, we now also find some differences in the hazard ratios for women and men. This being said, the observed effects are sizeable for all subgroups.

[21] One difference compared with the previous analysis is that establishment size is here measured annually within each work spell.

Table 4 Hazard rates: work spells

	Sickness relapse					Other				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
HR- Log est. size										
All workers	1.030*					0.903*				
	(90.22)					(-150.26)				
Private sector		1.056*					0.877*			
		(122.01)					(-161.26)			
Public sector		1.000					0.966*			
		(1.94)					(-29.45)			
Male-private			1.053*	1.085*	1.064*			0.882*	0.830*	0.409*
			(91.23)	(34.41)	(9.01)			(-124.21)	(-38.81)	(-31.52)
Female-private			1.059*	1.083*	1.054*			0.871*	0.850*	0.464*
			(97.37)	(38.64)	(9.00)			(-122.15)	(-37.56)	(-33.15)
Male-public			0.996*	1.020*	1.061*			0.976*	0.949*	0.500*
			(-5.11)	(7.06)	(7.48)			(-15.04)	(-7.78)	(-19.55)
Female-public			1.002*	1.019*	1.051*			0.963*	0.972*	0.567*
			(3.65)	(10.75)	(8.55)			(-30.64)	(-6.02)	(-21.39)
Spells	4,506,157	4,506,157	4,506,157	3,440,421	3,393,524	4,506,157	4,506,157	4,506,157	3,440,421	3,393,524
Failures	2,717,481	2,717,481	2,717,481	2,544,352	2,301,253	679,478	679,478	679,478	356,741	333,014
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Strata				ID	ID × Est.				ID	ID × Est.

Note: Models 1-4 and 6-9 include controls for age, age squared, gender, martial- and immigrant status, years of education, sectoral belonging (private or public), local unemployment, and an indicator for whether any children under the age of 11 is living in the household. Models 5 and 10 exclude gender, age, age squared, and immigrant status from the set of controls. Z-values for the coefficients on the original log hazard scale are in parentheses. *indicates that the hazard ratio is statistically different from unity at the 0.01 level.

To judge from model 5 an increase in log establishment size by one unit can be expected to raise the instantaneous probability of relapsing into long-term sickness absence by between 5.1 to 6.4 percent, depending on group belonging. This means that the likelihood of relapsing into sickness absence for a male worker in the private (public) sector at a given day is 39 (37) percent higher if he is employed in a workplace with 1000 employees than if he is working in an establishment with 5 employees. For a female worker employed in the private (public) sector the corresponding difference is 32 (32) percent. To put the magnitude of these figures in perspective we can note that that average differences in relapse hazards between women and men are 28 percent.

In columns 6–10 exit reasons other than sickness relapse are treated as the outcome of interest. As can be seen, the hazard of ending a work spell for other reasons than sickness relapse varies quite dramatically across establishments of different size. Employees in small workplaces are considerably more likely to end work spells for other reasons than sickness relapse than are their counterparts in large establishments. The hazard ratios in the private sector range from 0.409–0.882, whereas the corresponding range in the public sector is 0.500–0.976. Here the gender variation in the size effect appears comparatively small.

The results presented in Table 4 thus indicate that workers in small workplaces are less likely to fall back into sickness absence than are workers in large workplaces, and this for two reasons. First, and most importantly, the cause-specific hazard of sickness relapse is considerably lower among workers in small establishments. Second, work spells of workers in small workplaces are more likely to end for other reasons than sickness relapse, such as for instance unemployment or withdrawal from the labor force (which, in turn, reduces the probability that a work spell will end with a relapse into sickness absence).

The findings of this and the previous section thus suggest that the positive correlation between establishment size and long-term sickness absence rates is driven by a higher incidence, rather than a longer duration, of sickness absence spells in large workplaces. Before jumping to conclusions, however, we should examine the robustness of these findings somewhat further.

5.3 Robustness checks

The purpose of this section is to check the robustness of the previous findings. For reasons of space the analysis focuses on the two outcomes of most interest. The results in columns 1–3 of Table 5 thus refer to the cause-specific hazard of returning to work and the estimates in column 4–6 refer to the cause-specific hazard of a subsequent relapse into sickness absence.

In columns (1) and (4) all sickness and work spells occurring in establishments with less than 10 and more than 2000 employees are excluded from the analysis. The purpose of this analysis is to investigate whether the previous results are unduly driven by the sickness absence in the very smallest and the very largest workplaces. However, this seems not to be the case. Overall the results remain rather similar when excluding the smallest and largest establishments from the analysis.

In columns (2) and (5) we allow the baseline hazard to vary across all unique individual-industry combinations found in the data. Industry belonging is measured by 5-digit industry codes. As can be seen, using the within individual-industry variation in the data, rather than the within individual-establishment variation, does not alter the substantive conclusions previously reached (although a few coefficients decrease somewhat in size).[22]

So far I have proceeded on the assumption that it is the establishment, rather than the firm, that constitutes the relevant work unit. This choice could be debated, however. In columns 3a and 3b, and 6a and 6b, I therefore examine what happens if both firm and establishment size is allowed to enter the model. In this case the identifying variation comes from firms with multiple establishments. As can be seen from columns 3a and 6a, which displays the hazard ratios from a one unit increase in log establishment size, not much happens to the coefficient of establishment size when firm size is controlled for. Turning to columns 3b and 6b, which displays the hazard ratio associated with a one unit increase in log firm size (holding establishment size fixed), we can note that the coefficients associated with firm size are usually of smaller magnitude than those associated with establishment size.

[22] Another interesting possibility would be to stratify the analysis according to occupation. Unfortunately occupational codes are only available for a subset of the sample studied here.

Table 5 Robustness checks

	Work resumption				Sickness relapse			
	(1)	**(2)**	**(3a)**	**(3b)**	**(4)**	**(5)**	**(6a)**	**(6b)**
HR- Log est. size								
Male-private	1.026*	1.025*	1.021*	0.999[a]	1.064*	1.051*	1.076*	0.982*[a]
	(3.19)	(7.43)	(3.33)	(-0.13)	(6.29)	(10.75)	(9.39)	(-3.21)
Female-private	0.998	1.005	1.003	1.003[a]	1.059*	1.035*	1.055*	0.996[a]
	(-0.23)	(1.86)	(0.52)	(0.90)	(6.85)	(8.97)	(8.38)	(-0.84)
Male-public	1.003	1.011*	1.000	1.009[a]	1.060*	1.039*	1.076*	0.971*[a]
	(0.35)	(2.96)	(-0.02)	(1.85)	(5.34)	(7.68)	(8.62)	(-5.38)
Female-public	0.976*	0.992*	0.982*	1.013*[a]	1.055*	1.024*	1.055*	0.984*[a]
	(-3.86)	(-3.38)	(-3.88)	(3.81)	(7.12)	(7.33)	(8.97)	(-4.05)
Spells	2,822,915	3,752,058	3,477,065		2,853,944	3,398,672	3,393,524	
Failures	2,670,085	3,442,462	3,252,765		1,899,864	2,340,654	2,301,253	
Controls	Yes	Yes	Yes		Yes	Yes	Yes	
Year-FE	Yes	Yes	Yes		Yes	Yes	Yes	
Region-FE	Yes	Yes	Yes		Yes	Yes	Yes	
Strata	ID × Est.	ID × Industry.	ID × Est.		ID × Est.	ID × Industry.	ID × Est.	

[a] The coefficient refers to the hazard ratio for log firm size.

Notes: All models include controls for martial status, years of education, local unemployment, and an indicator for whether any children under the age of 11 is living in the household. Z-values for the coefficients on the original log hazard scale are in parentheses. *indicates that the hazard ratio is statistically different from unity at the 0.01 level.

Finally, the overall results have also been shown to be stable with respect to other types of changes (not reported here). For instance, the results look very similar if we control for income[23] (i.e., insurance benefits or wages) or if establishment size is measured by means of a set of dummy variables (indicating various size classes). Moreover, the establishment size variable has been interacted with spell duration to examine the accuracy of the proportional hazard assumption. Generally, no important departures from proportionality were detected, although it should be acknowledged that the results become considerably more sensitive to the exact model specification once we allow the importance of establishment size to vary over time.

5.4 Overall incidence

In the analysis above the sole concern has been with how workplace size relates to the cause-specific hazard of different outcomes. This type of analysis is appropriate if our intent is mainly to uncover the underlying relationship between a particular independent variable and an outcome of interest. In the presence of competing risks, however, there is no direct relationship between the underlying hazard for an event of interest and the actual incidence of this event in a particular population. This is because, unless the competing risks are independent the overall incidence will be a function of *all* cause-specific hazards (Putter et al. 2007). Or to be more concrete, we cannot tell whether the incidence of work resumption is higher in larger workplaces only by analyzing the cause-specific hazard for work resumption since the number of workers returning to work at the end of a sickness spell will also depend on the probability of exiting sickness absence for other reasons.

To get a better sense of how the time spent in different states varies across establishments of different size it is therefore useful to study so-called cumulative incidence functions. These functions describe how the share of individuals experiencing a particular event evolves over time. Formally the probability that an individual *i* who is in state *s* will fail from cause *c* before time *t* can be written as:

$$\mathrm{CIF}_i^{s,c}(\mathrm{t}) = \int_0^{\mathrm{t}} h_i^{s,c}(u) S_i^s(u) du, \qquad (3)$$

[23] The reason for not including income in the specification is obviously that there is a large literature that argues that income is partly an effect of establishment size.

where $h_i^{s,c}$ is the state and cause-specific hazard rate and S_i^s denotes the probability of not failing for *any* cause prior to time *t*. Consequently, the cause-specific hazard rates analyzed above together with an estimate of the underlying baseline hazard provides us with sufficient information to construct the cumulative incidence functions of interest.[24]

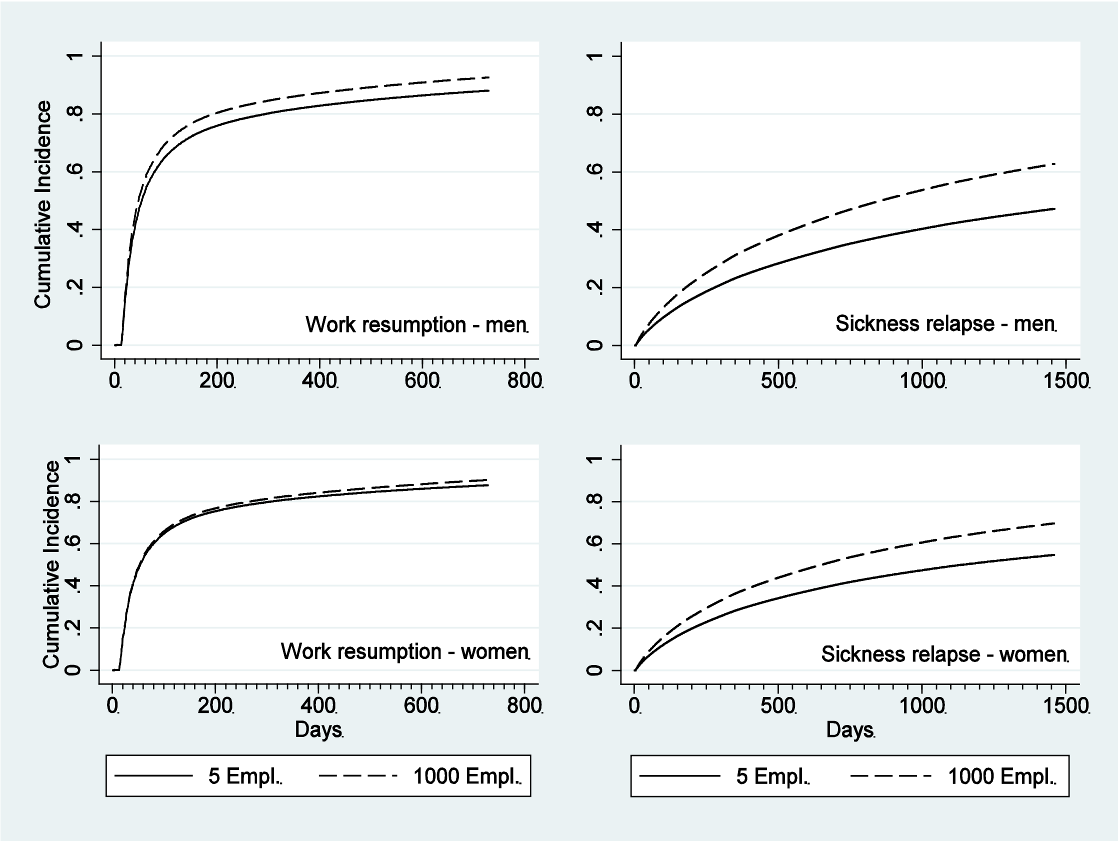

Figure 1 Cumulative incidence functions for private sector workers

The upper two graphs in Figure 1 display the estimated time to work resumption (the leftmost graph) and sickness relapse (the rightmost graph) for private sector male workers employed in establishments with 5 and 1000 employees, respectively. The lower two graphs provide the same information for female workers in the private sector. In each graph the difference between the two curves is based on the estimates of columns (5) and (10) of Table 3 and Table 4.[25] I have chosen to focus on the private

[24] This approach to estimate cumulative incidence functions is described in more detail in Lunn and McNeil (1995). Alternatively, the cumulative incidence function can be modeled directly using the method of Fine and Gray (1999). This approach is, however, very computational demanding for large data sets.

[25] More precisely, to estimate the cumulative incidence of workers employed in establishments with 5 employees the results from columns (3) and (8) in tables 3 and 4 have been used. The control variables was all set at their mean (continuous variables) or modal (categorical variables) value. The hazard ratios from columns (5) and (10) of tables 3 and 4 were then used to calculate the cumulative incidence of workers in establishments with 1000 employees. By using the results from columns (3) and (8) to calculate the benchmark value for workers in small establishments we

sector in this section since the previous analysis has shown that the size-absence relationship is generally more pronounced among private sector workers.

As can be seen from the two left-most graphs most sickness spells are of rather short duration. The (estimated) probability that a "typical" private sector male worker employed in an establishment with 1000 employees has returned from sickness absence after six months is just below 80 percent, whereas the corresponding figure for a worker, of the same type, employed in an establishment with 5 employees is slightly below 75 percent. After two years the likelihood of work resumption in these two groups is as high as 93 and 88 percent, respectively. As discussed above, the observed difference between establishments of different size is driven by two factors. First, and most importantly, among those workers that eventually return to work the time spent in the sickness absence state is somewhat shorter for (male) workers in large establishments. Second, workers in large establishments are also less likely to leave the sickness absence state for other reasons than work resumption than are their counterparts in small establishments.

We already know that the differences between small and large establishments are of somewhat smaller magnitude for female private sector workers. After 6 months 74 percent of the female workers in small workplaces and 76 percent of those in large workplaces have resumed work. After two years these numbers have grown to 88 and 90 percent, respectively.

We can also read off the median time to work resumption from the cumulative incidence functions. Doing so we find that the median time to work resumption for male (female) workers is 47 (53) days if they work in an establishment with 1000 employees and 54 (55) days if they are employed in an establishment with 5 employees.

Turning to the incidence of sickness relapse we see that the differences between small and large establishments are rather striking. For private sector male workers in establishments with 1000 employees the probability of relapsing into sickness absence within 6 months is 20 percent, whereas the risk is "only" 15 percent in establishments with 5 employees. Extending the horizon to three years the corresponding probabilities are 56 and 42 percent, respectively. Looking instead at the median time to sickness

circumvent the problem of having to estimate separate cumulative incidence functions for each unique individual-establishment combination.

relapse this time is little over two years (853 days) for large establishment workers, and just over four and a half years (1704 days) for those employed in small establishments.

In comparison to men we see that females in the private sector stand a somewhat greater risk of relapsing into sickness absence. Yet, the observed differences between workers in small and large establishments are of similar magnitude for women and men. The probability that a female worker employed in a large establishment (1000 employees) relapses into sickness absence within 6 months is about 24 percent, whereas the probability for a similar worker employed in a small establishment is 19 percent. Within three years 63 percent of the female workers employed in large establishments and 49 percent of those employed in small establishment are expected to have relapsed into sickness absence.

Again these differences translate into important differences in the median time to sickness relapse. In large establishment 50 percent of the female workers are expected to have returned to sickness absence in little less than two years (649 days), whereas the corresponding figure among female workers in small establishments is somewhat over three years (1137 days).

Overall, I believe that the graphs in Figure 1 serve to show that the size differences in cause-specific hazards that we found above carry economic significance. One indication of this is that the differences in time to work resumption and sickness relapse found between private workers in small (5 employees) and large (1000 employees) establishments are on par with those found between female and male workers. In addition, the cumulative incidence plots nicely illustrates the fact that the positive correlation between workplace size and (long-term) sickness absence is primarily driven by a higher incidence, rather than a longer duration, of sickness absence spells in large workplaces.

6 Discussion

Over the years, social scientists of various strands have paid interest to the importance of workplace size in explaining work absence in general, and sickness absence in particular. Although researchers in different disciplines have advanced competing theoretical arguments for why we should expect absence rates to be related to the size of the work unit, there has been large agreement on the nature of the relationship. The persistent conventional wisdom was succinctly described by Muchinsky more three decades ago: “the larger the work group, the greater the absenteeism” (1977, p. 329).

The purpose of the present study has been to provide a closer examination of the mechanisms underlying this empirical regularity. In particular, the analysis has attempted to answer the question of whether the frequently reported correlation between size and absence is mainly driven by a higher incidence or a longer duration of sickness absence spells in large workplaces.

Provided that the findings of this study could be taken to generalize to countries outside Sweden and to sickness absence spells with shorter duration than those studied here, the empirical results clearly suggest that the size-absence correlation is due to a higher incidence of sickness absence in large workplaces. Even though workers in large establishments were found to face a substantially higher risk of relapsing into sickness absence than were workers in small establishments, no corresponding difference for the length of sickness absence duration was found. On the contrary, absent workers employed in large workplaces actually tend to resume work somewhat more quickly than their counterparts in small establishments.

This being said, the empirical analysis also revealed considerably variation in the size-absence relationship across different labor market groups. The relationship was shown to be more pronounced in the private than in the public sector, and somewhat less pronounced for women than for men. Given the data at hand one can only speculate about the reasons for these differences. Looking into potential determinants of the variation in the strength of the size-absence relationship across different labor market groups therefore constitutes a central avenue for further research. Particularly since the pattern observed here, that workplace size appears to be of most importance for private sector male workers, mimics that found in the related literature on size-wage premia (e.g., Belman and Heywood 1989; Oi and Idson 1999).

Another lingering question is whether the observed relationship between workplace size and the sickness absence behavior of workers can be given a causal interpretation. Non-random sorting of workers across workplaces of different size, no doubt, constitutes the main threat to causal interpretation in this setting. In comparison with previous studies on this issue the present analysis has gone a long way toward mitigating this problem. Most importantly, the occurrence of multiple sickness spells has been used to take unobserved heterogeneity at the individual and workplace level into account. Although this approach has its obvious limitations it is probably as far as we can come in establishing a causal effect in the absence of credible instruments, or other sources of truly exogenous variation in workplace size.

To summarize, although the present study provides support for the view that workplace size can affect sickness absence rates, the empirical results suggest that the relationship may be more intricate than has hitherto been assumed. Not only have we found the size of the workplace to relate differently to the incidence and duration of (long-term) sickness absence spells, but the results also indicate that workplace size play a somewhat different role in different parts of the labor market . Establishing what lies behind these empirical patterns should be an important area for future research.

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