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SOCIAL INTERACTIONS AT THE WORKPLACE: EXPLORING SICKNESS ABSENCE BEHAVIOR.

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Social Interactions at the Workplace: Exploring Sickness Absence Behavior*

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Abstract
We investigate whether a worker's sickness absence is affected by her colleagues' absences from the workplace. The analysis is based on unique matched employer-employee data for Norwegian schoolteachers for the period 2001 to 2006 with information on different types of absences and multiple teacher and school characteristics. Using different approaches where methodological problems such as the reflection problem and intra-group correlation are mitigated, we look for evidence of social interaction effects. Our results show that the significance of the social interaction effects critically depends on our ability to control for unobserved school characteristics.

JEL codes: C23, C31, H55, I38, J22.
Keywords: Social interaction, peer effects, sickness absence.

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1. Introduction

Social interaction in the form of group effects has attained increased attention in recent years. Such effects have obvious policy implications because group interaction may create social multipliers. This implies that a policy change or a reform will not only affect the behavior and outcome of the target group, but the effect will also be reinforced by the fact that individual behavior is mutually dependent through group interaction. The empirical economic literature is growing fast, but the research is in general faced with several methodological challenges (Manski, 1993; Blume *et al*., 2010; Durlauf, 2004). Nevertheless, studies have found credible evidence that group behavior influences individual decision-making in different settings. In relation to the labor market, studies from the US, the UK, and China find that workers' productivity depends on their coworkers within the same team or social network (Bandiera *et al*., 2005; Mas and Moretti, 2009; Kato and Shu, 2008) and that social ties and networks with one's coworkers increase one's own performance (Bandiera *et al.*, 2009).[1]

In this paper, we estimate whether a worker's sickness absence is affected by the absence of her colleagues from the workplace. Sickness absence is a major problem in several European countries and is the single most important cause of lost labor time (OECD, 2010; Treble and Barmby, 2011). Empirical evidence suggests that the level of absence is correlated with the generosity of the sick-pay system; sickness insurance in many countries has low co-payments and very lenient ways of controlling sickness claims, with the probability of moral hazard problems being correspondingly high. Group effects, if present, reinforce the alleged moral hazard problems and work as a social multiplier on the propensity of being absent. We can distinguish between two channels through which peer mechanisms can work. First, an individual can be influenced by the peers' threshold value for when to report sick. Second, peers can influence the tendency to shirk. If social interactions lower the marginal cost of shirking, employees can be more prone to dodging work.

A handful of empirical studies have explored social interactions in relation to sickness absence behavior. Ichino and Maggi (2000) study employees who move between branches within a large Italian bank. The authors find that the movers adapt to the average absence

[1] Lazear (1989), Kandel and Lazear (1992), Rotemberg (1994), and Lindbeck *et al.* (1999) have suggested peer group influence in employees' behavior at the workplace.

level of the colleagues at the arriving branch. Bradley *et al.* (2007) use a matched teachers-school data set to explore absence reported as illness among Australian school teachers. The authors also find evidence of interaction effects applying approximately the same empirical model as Ichino and Maggi, but the coefficients are partly insignificant when controlling for simultaneity bias. Hesselius *et al.* (2009) use a reform that extended the self-reported absence for half of all employees in a Swedish municipality in 1988 to identify interaction effects. The authors find strong effects for the non-treated employees in workplaces with high proportions of treated co-workers, where the suggested explanation is that the non-treated adjust their behavior due to social interaction with the treated. Other studies use the neighborhood as the reference group. For example, Lindbeck *et al.* (2008) adopt several different approaches to investigate peer effects with a data set covering the entire Swedish population. The authors use the mover-model suggested by Ichino and Maggi, but also immigrants vs. resident citizens and private-public sector differences. Overall, the authors obtain statistically significant estimates of interaction effects. In two recent Norwegian studies, Dale-Olsen *et al.* (2010) and Markussen and Røed (2012) analyze social interaction effects in sickness behavior. The former uses employer-employee data and analyzes group effects at the workplace based on the instrumental variable approach, while the latter uses fixed-effects methodology and analyzes group effects within networks in neighborhoods, families, ethnic minorities, and former schoolmates. The authors of both studies report sizeable social interaction effects in sickness absence. Closely related to sickness absence, Rege *et al.* (2007) report significant group effects in disability pension behavior in Norwegian neighborhoods, and Aslund and Fredriksson (2009) find that long-term welfare dependency is affected by one's peers among Swedish refugees.[2]

The present paper uses a unique matched employer-employee data for Norwegian teachers in the period 2001 to 2006. Different forms of absence (self-reported and doctor-certified) are registered for the majority of the primary and secondary schoolteachers, together with individual information on earnings, education, gender, age, localization, etc. In addition, we have a broad range of school characteristics, including average level of absence, share of female workers, age distribution, etc. Such group variables (unfeasible with ungrouped

[2] See also Lalive and Parrotta (2011) and Hesselius (2009).

individual data alone) are important when analyzing the effects of social interaction on sickness absence at the workplace.

We consider the workplace, and not neighborhoods, the most important arena for group effects to take place regarding sickness absence behavior.[3] Ideally, we want data for employees from all branches, and the fact that we have information only on teachers' absence limits the generality of our results. However, schools as workplaces are relatively homogenous regarding organization, job tasks, interactions with workmates in the common room, etc. Comparing absence behavior and peer effects between schools therefore makes more sense than comparing between workplaces where we know little or nothing about how work is organized, how workmates interrelate, etc. In addition, we have access to self-reported (short-term) and doctor-certified (short- and long-term) sick leave. To our knowledge, no other data set offers the possibility of studying social interaction effects on the complete range of absence.[4]

Separating the different types of group interaction is challenging, particularly with administrative register data. Manski (1993) distinguishes between three mechanisms that explain why individuals in the same group behave similarly: The behavior of an individual can directly influence the outcome of his or her peers (endogenous interaction), the individual characteristics of an individual can affect the outcome of his or her peers (contextual interaction), or peers may behave similarly because they have similar individual characteristics or because the peers face the same institutional environments (correlated effects). The challenge is to disentangle the true social interactions (endogenous) from the correlated effects. In the identification process, two major complications are encountered: the simultaneity problem, also known as the reflection problem in the peer literature, and problems of intra-group correlation. Both must be addressed to credibly identify peer effects.[5]

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[3] Neighborhoods are often used in papers on peer effects, but one might argue that it probably is a less relevant unit, since many of us hardly know our neighbors, let alone their absence and presence at their different jobs.

[4] Ichino and Maggi (2000) have access to short- and long-term absence, but only as number of episodes "due to illness." Bradley *et al.* (2007) and Hesselius *et al.* (2009) use only short-term absence while Lindbeck *et al.* (2008) use long-term absence.

[5] For more details on the methodological problems and identification strategies, see Manski (1993, 2000), Brock and Durlauf (2001), Moffitt (2001), and Graham (2008). In addition, Soetevent (2006) provides a thorough overview.

Our approach to shed light on social interaction effects is to study teachers who move between schools. We compare sickness absence at the new and old schools and test whether the change in sickness absence can be explained (partly) by the change in the level of the teachers' co-workers' sickness absence. The richness of individual and school characteristics allow exploration of heterogeneity across different genders and school types, and along other lines. Furthermore, the longitudinal nature of our data allows testing of the dynamics of the interaction process. For newly employed teachers at a school, we would somehow expect that the behavior of the group will have increasingly stronger influence as the months and years pass.

Our model resembles the one pioneered by Ichino and Maggi (2000) and followed up by Bradley *et al.* (2007), but our data allow a more comprehensive control for unobserved school effects. This difference turns out to be critical: Our findings suggest significant social interaction effects as long as we control for the unobserved effect in line with the quoted papers, but not after including a more detailed control for unobserved school effects.

2. Empirical strategy

Our starting point and reference is a model in which a teacher's sickness absence is a linear function of the average sickness of the co-workers, controlled for individual and peer group characteristics, school and municipality characteristics, etc.:

$$S_{ijmt} = \beta \overline{S}_{-ijt} + \varphi \overline{X}_{it}^{'} + \gamma \overline{X}_{-ijt}^{'} + \mu W_{jt}^{'} + \eta Z_{mt}^{'} + \alpha_i + \sum_j \delta_j D_{jt}^{W} + \sum_m \phi_m D_{mt}^{Z} + \tau_t + \varepsilon_{ijmt} \quad (1)$$

S_{ijmt} is the sickness absence of individual i at school j and municipality m at time t. $\overline{S}_{-ijt}$ is the average sickness absence of all co-workers, excluding individual i, at school j at time t. $X_{it}^{'}$ and $\overline{X}_{-ijt}^{'}$ are vectors of observable individual characteristics of individual i at time t and the average of the colleagues' individual characteristics, excluding individual i, at school j at time t, respectively. $W_{jt}^{'}$ is a vector of observable characteristics of school j at time t, and $Z_{mt}^{'}$ is a vector of observable characteristics of municipalities m at time t. Finally, we have the fixed effects controlling for unobserved characteristics at different levels. α_i is the time-invariant individual effect. $\sum_j \delta_j D_{jt}^W$ are dummies for each j school at time t, and $\sum_m \phi_m D_{mt}^Z$ the equivalent for the m municipalities.[6] τ_t is a time fixed effect, and ε_{ijmt} is an i.i.d. error term. The parameter of main interest is β, representing the endogenous social interaction effects.

The alleged interaction effects from this model, however, will be biased. One of the methodological issues that create this bias is intra-group correlation. If a group-specific component of the error term varies across groups and is correlated with the individual characteristics of the peers, it will lead to the standard problem of omitted variables bias. The second methodological problem is simultaneity. Because the sickness absence of the teachers in the peer group affect each other simultaneously, it is difficult to separate out the genuine causal effect that one teacher's absence has on another teacher's absence in the group. This situation, known as the reflection problem after Manski (1993), creates a simultaneity bias. The strategy we pursue to isolate the causal effect of peer groups on sickness absence involves using the variation caused by teachers who change workplaces. If teacher i quits her job at the old school at time t-1 and starts working at the new one at time t, the equation of interest is

$$\begin{aligned}(S_{ijmt}^{New} - S_{ijmt-1}^{Old}) = {} & \beta(\overline{S}_{-ijt}^{New} - \overline{S}_{-ijt-1}^{Old}) + \varphi(X_{it}^{'} - X_{it-1}^{'}) + \gamma(\overline{X}_{-ijt}^{'New} - \overline{X}_{-ijt-1}^{'Old}) \\ & + \mu(W_{jt}^{'New} - W_{jt-1}^{'Old}) + \eta(Z_{mt}^{'New} - Z_{mt-1}^{'Old}) \\ & + \sum_j \delta_j (D_{jt}^{W,New} - D_{jt-1}^{W,Old}) + \sum_m \phi_m (D_{mt}^{Z,New} - D_{mt-1}^{Z,Old}) + (\varepsilon_{ijmt}^{New} - \varepsilon_{ijmt-1}^{Old})\end{aligned} \tag{2}$$

where the notation follows equation (1). Note that the school average level of absence and the vector of school characteristics vary by school and year. This means that school fixed effects, δ_j, can be identified.[7]

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[6] δ_j and ϕ_m measure time-invariant effects from schools and municipalities, respectively. However, the teachers are not necessarily at the same school and/or municipality in every time period, hence, the dummies' subscript t.

[7] The same goes for the municipality fixed effects ϕ_m when changing municipality, since there is time variation in the vector of the municipality characteristics.

Model (1) can be applied to all teachers, while model (2) contains the movers between jobs only, and therefore represents a dramatic drop in the number of observations. The procedure, however, has several advantages. First, by taking the first-difference the unobserved individual heterogeneity is controlled for. Health is an important example here. Since there is no health information in our data, it is reassuring that at least time-invariant health heterogeneity is controlled for. Furthermore, since the observations in period *t* relative to period *t*-1 represent different schools, we expect far more variation in the peer group and the school characteristics than we would typically find in a standard first-difference transformation of model (1) (which would be dominated by non-moving teachers).

Finally, model (2) might offer a solution to the reflection problem. A standard solution in fixed-effect models is to drop the average sickness absence of the colleagues and replace it with its lagged value. This approach, however, does not eliminate the simultaneity bias if (i) the reference group is similar in all periods and (ii) the average sickness absence of the colleagues is correlated over time. By using job-changers, we circumvent this problem. Because the moving teacher has not yet begun working at the new school at time *t*-1, she does not interact with the employees at the new school at time *t*-1, and hence, she does not influence the future peers' sickness absence level at that time. Thus, by instrumenting $\bar{S}_{-ijt}^{New}$ with its lagged value $\bar{S}_{-ijt-1}^{New}$, we eliminate this potential source of simultaneity bias. The identifying assumption is that the average sickness absence at the arriving school is not a determinant for the future expected level of absence for the teachers who plan to move. In other words, we assume that no self-selection is going on regarding the average absence level (that is not picked up and integrated out by the individual fixed effect). Note that we cannot fully circumvent the problem at the *old* school. Even if we instrument the average sickness absence at time *t*-1 with its leaded value, *t*, the average sickness absence at the old school might still be influenced by earlier interaction with the departed teacher.

A noteworthy concern is potential endogenous group membership. Teachers who search for new jobs may self-select or unconsciously sort themselves into different schools based on unobservable individual preferences or school characteristics. If such sources of group endogeneity are not dealt with in a satisfactory way, empirically identified interaction effects may be spurious. A vital question is whether the selection process is constant across time or not. The individual-, school-, and municipality-specific fixed effects in the model will control for any time-invariant components of the error term. However, if the source of selection varies across time, the model will fail to absorb the components, and the resulting coefficient estimates become inconsistent.

3. Data and institutional settings

3.1 Data sources

We use register data for Norwegian schoolteachers from the Association of Local and Regional Authorities (KS). The data set covers teachers employed in public[8] primary, lower secondary, and upper secondary schools in the majority[9] of the municipalities from 2001 to 2006. The information provided is sickness absence records, educational attainment, labor market status, earnings, age, and gender. Sick leave is divided into self-reported absence and absence certified by physicians. Both are recorded as the cumulative number of days per calendar quarter.

With an employee-employer identifier, the data set is merged with multiple administrative registers to provide detailed information about the peers, schools, and municipalities.[10] The information includes school size, school type, and social insurance agreements as well as unemployment rates, centrality indexes, and each municipality's expenditure on schools. Finally, the fact that we can identify all teachers at the respective schools allows us to construct variables indicating the average of the peer characteristics. This is a major advantage of using merged register data of the present type.

3.2 Institutional settings

Sickness insurance is mandatory in Norway, and covers all workers employed for more than four weeks. The compensation ratio is 100 percent from day one, for a maximum period of one year. There is an upper compensation limit of approximately €50 000, but through

[8] Less than 2% of the total number of pupils attend private schools in Norway.

[9] The main exception is schools in Oslo, the Norwegian municipality with the highest number of inhabitants. The municipality is not a member of KS so the teachers working at these schools are not recorded in the data set.

[10] Information about the municipalities is from the Municipal-State-Reporting register (KOSTRA) and the Norwegian Social Science Data Service (NSD).

negotiations between employers and employees, this ceiling has been removed in the public sector and in the majority of the private sector.

A worker reporting sick will be financed by his or her employer from day 1 to day 16, after which the National Insurance Administration takes over from day 17 and up to one year, at the maximum. No medical certification is required for sickness spells lasting from one to three days. This self-reporting opportunity can be applied up to four times per year.

As of 2001, firms have been encouraged to join a publicly organized campaign called Including Working Life (*Inkluderende arbeidsliv*, IA), allowing self-reported absence spells up to eight days, three times per year. Spells lasting more than three/eight days require a medical certificate, and an even more detailed one after eight weeks. However, Norwegian general practitioners (GPs) are considered very liberal gatekeepers. Moral hazard, which is always a problem with this type of social insurance, is, accordingly, very much an issue.

3.3 Sample definitions

Using the data sources mentioned above, our main sample consists of teachers in primary, lower secondary, and upper secondary schools who changed workplaces between 2001 and 2006. We maintain the division of sick leave in two categories (self-reported and doctor-certified) because of their different structure and spells. Self-reported spells are always short, while absence certified by a physician can be short or long. To our knowledge, we are the first to study peer effects for both types of sick leave.

Because the main data set is organized according to the calendar year while the registers providing school characteristics are structured according to the school year, we must limit the observation of teachers to the fall semester to ensure that we pinpoint the job-changing teachers to the correct schools at the correct point in time. To make a reliable measure of sick leave before and after the job change, we identify and measure the teachers' sickness absence in the third quarter every year. This procedure also removes seasonality from the data. The strategy implies that time $t-1$ and time t in equation (2) is the third quarter at the old and new schools, respectively.

Finally, limitations in the data set (unbalanced panel with observational gaps) restrict us from identifying the exact year of the job-change for the entire sample of movers. For the affected individuals, we measure their arrival at the new school in the first observed year. This inaccuracy in the identification process may create some noise in estimations of the empirical model.

To be able to evaluate the representativeness of the subsample of the job-changing teachers, we also construct a sample consisting of teachers who do not change workplaces during the period (2001-2006). In addition, sick leave is divided into two categories (self-reported and doctor-certified) where the absence is measured in the third quarter every year.

3.4. Summary statistics

The main sample of moving teachers constitutes about 7 percent of the teachers in the data set. As mentioned in Section 2, several endogeneity issues must be addressed. Omitted school characteristics may be correlated with included peer characteristics and contribute to bias in the estimated interaction effects. Likewise, selection of teachers to certain schools based on unobserved individual characteristics represents another possible source of endogeneity. Table 1 reports descriptive statistics for the samples of teachers not changing schools and changing schools. There is one distinct difference between the two samples, namely, that the movers are on average more than five years younger than their non-moving colleagues. This is not surprising: changing schools is more tempting and/or more necessary at the early stage of one's career. It also explains other differences, namely, that our subsample has less seniority and a lower level of doctor-certified sickness absence (which we know is positively correlated with age). Otherwise, the overall impression is that the observables are quite similar in the two samples. As explained in Section 2, our approach toward *unobserved* differences is a battery of fixed effects, on the individual as well as the school, municipality, and period level.

[Table 1 about here]

4. Results

Table 2 displays the results for self-reported (upper panel) and doctor-certified (lower panel) absences. This is what we refer to in Section 2 as our starting point and reference model (equation (1)). Before focusing entirely on the sample of teachers who change schools during our period of observation, we also estimate the reference model for the full sample; that is, for all teachers whether they change school or not, all observations are pooled for all individuals in both cases. Comparing the coefficients from the two samples will give us an idea of the representativeness of the subsample.

First we control for observable covariates on the individual, school, and municipality level, cf. Table 2 (columns 1 and 4). Then we add municipality fixed effects (columns 2 and 5) and school fixed effects (columns 3 and 6). For the full sample, individual fixed effects are not possible to identify together with school and municipality fixed effects at this stage, since the school and municipality dummies are time invariant for the vast majority (the non-movers). Furthermore, the number of schools relative to the number of teachers changing schools implies a very high number of school dummies. It turns out that to allow for reliable identification of the school and municipality fixed effects we are able to add only one level at a time, starting with municipalities and continuing with schools. The same kinds of problems are encountered in Ichino and Maggi (2000) and Bradley *et al.* (2007), who have to restrict their branch/school fixed effects considerably more than we do.[11]

[Table 2 about here]

Starting with self-reported absence, we see that the effect of the colleagues' level of absence appears to be sizeable and highly significant: If the mean sick days for the colleagues increase by one day, it is associated with about a 0.5 day increase in the individual level of absence.[12] The effect is remarkably equal for both samples. At this stage, we have done nothing to control for unobservable confounders, however. Including municipality fixed effects in columns 2 and 5 reduces the effects to 0.39 and 0.42, respectively, which still is significant

[11] Ichino and Maggi use 91 fixed effects for the administrative provinces instead of the almost 400 branches, while Bradley *et al.* use education districts instead of schools.

[12] Note, however, that the average number of self-reported sickness absences per quarter is approximately one day, so an extra day of absence implies a 100 percent increase.

even at the 1 percent level. Switching to school fixed effects in columns 3 and 6 reduces the social interaction effect to 0.29 and 0.23, respectively, but the estimates are still highly significant.

In Table 3, we restrict the sample to the teachers who change schools during our period of observation, but pool their last observations before and the first observation after changing schools only. The effects on self-reported absence resemble those reported in Table 2, but are somewhat smaller in magnitude. Note, however, that switching to school fixed effects in column 3 has a big impact. The coefficient is reduced to -0.15 and is insignificant. This is the first signal of an important finding in our analysis: the magnitude and significance of the interaction effect are highly dependent on the inclusion of school fixed effects.

[Table 3 about here]

The following three columns ("FD") internalize that our teachers change schools, comparing the level of sickness at the arriving school relative to the departing school (cf. equation (2)). Hence, we study the *change* in the individual sickness absence, with the change in colleagues' average sickness absence as the key explanatory variable. Being a regression in first differences, this controls for individual unobserved heterogeneity, such as (time-invariant) health, motivation, etc. This returns coefficients that are clearly lower in magnitude, and in the case where we include the school fixed effect, the social interaction coefficient is no longer significantly different from zero (the point estimate is even negative).

The reflection problem is still unsolved, however. The moving teachers' sickness absence may influence the group absence level at the arriving school, in which case ($\bar{S}_{ijt}^{New}$ - $\bar{S}_{ijt-1}^{Old}$) is endogenous to (S_{ijt}^{New} - S_{ijt-1}^{Old}). We approach this problem by using the lagged value $\bar{S}_{ijt-1}^{New}$(on which the arriving teacher has no influence) as the *instrument* for the contemporaneous group level $\bar{S}_{ijt}^{New}$. As noted by Bradley *et al.* (2007), the instrumental variable (IV) estimator also corrects the (downward) bias stemming from measurement error in the average group absence at the arriving school. The results are displayed in the three last columns of Table 2. It turns out that the IV specification has a large positive impact on the estimates, suggesting that the estimates from the first difference mover model (the "FD" results in columns 4 to 6) were downward biased, due to simultaneity or measurement error or both. For the model, controlling for observables at all levels plus individual fixed effects (column 7), the results are in line with the specification in the levels (column 1). That is also the case when we add municipality fixed effects (column 8). In the final specification, where the reflection problem allegedly is mitigated by the inclusion of observed and unobserved confounders, and using the IV estimator, we cannot identify a sizeable social interaction effect at conventional significance levels.

We now turn to the part of the sickness absence that has to be certified by doctors (more than three or eight days, depending on participation in the IA agreement or not, cf. Section 3.2). Our findings are reported, first, for comparison, in the lower panel of Table 2. Once again, the way the individual sickness absence is affected by the absence level of one's peers appears to be quite similar in the non-moving sample and the sample of moving teachers. The exception is the version controlling for school dummies, where the interaction effect turns out to be unmeasurable for movers but significantly negative for non-movers. Table 4 reports the results for the movers, once again starting with ordinary least squares (OLS) and where the last observations before and the first observation after change of school are pooled. In short, there are very few measurable effects. The exception is a significantly positive effect in the OLS specification without controls for group fixed effects and, perhaps more disturbing, a significantly negative effect in our most reliable model. Negative social interaction effects cannot be ruled out, a priori. It is for example possible to think of situations where teachers arriving at a school with a high level of sick leave respond altruistically by lowering their level of absence. However, we have never seen any support for a negative correlation in the empirical literature. Together with the general lack of significance in Table 4, we are inclined to conclude that our model fits relatively poorly for exploring doctor-certified sickness absence. In the remaining part of the paper, therefore, we concentrate on self-reported absence.

[Table 4 about here]

The longitudinal nature of our data and the richness of individual and school characteristics allow testing of the dynamics and specific group effects in the interaction process. Table 3 contains separate coefficients for women (middle panel) and men (lower panel), respectively.

There are approximately twice as many women as men in our sample, which contributes to more significant coefficients in the female case. An exception is the FD-IV-case in column 9, where our model returns a negative effect for men, and even significantly so at the 10 percent level. Overall, however, the gender similarities are more noticeable than the differences. On this basis, and since there are relatively few degrees of freedom when we split our sample further into more subgroups, the female and male observations are pooled in the remaining regressions.

We explore the heterogeneity along two more directions. In Table 5, we ask if it matters whether one moves to a school with high or low average sickness absence, operationalized by stratifying the schools according to whether they are above or below the median. A relatively clear pattern immerges (once again except for the specification with school fixed effects). The interaction effect is much stronger—roughly twice as big—for teachers who move to schools where the average level of absence is above the median.

[Table 5 about here]

In addition, school type appears to matter. In Table 6, we identify teachers who work in primary and lower secondary schools ("Grunnskole") and those who work in upper secondary ("Videregående"). The latter schools are less heterogeneous compared to the former, with strong variation in tracks (vocational or preparation for high school/university), often in localization (separate buildings and common rooms for different tracks), etc. Overall, our results indicate that the interaction effect appears to be slightly stronger and/or more sharply determined in the most heterogeneous case, which comes as no surprise.

[Table 6 about here]

Thus far, we have looked only at interaction effects at the first year in the new school. However, there is no reason to believe that the interaction process is constant over time as the new teachers continue their interaction with their new colleagues. To explore whether the interaction process evolves over time, we follow the teachers into their second and third years at the new school. In the estimation, we instrument the value of $\overline{S}_{-ijt}$ with its lagged values in each year. The results are displayed in Table 7. Unfortunately, only 679 teachers were observed in the first and the second year after changing schools. As in the contemporaneous case, we see that the effect diminishes as we add municipality fixed effects, and becomes insignificant when we add the school fixed effect. As for the dynamics in the specifications that return significant coefficients, we note that there appears to be an increase in the interaction effect from year *t* to year *t*+1 and further to year *t*+2. An exception is from year *t* to *t*+1 in the "FD and IV" case, but here we see an extra strong increase from year *t*+1 to *t*+2. Hence, the tendency is that the group effect is amplified across time, which is in accordance with our a priori assumptions.

[Table 7 about here]

5. Concluding remarks

In this paper, we have investigated whether teachers' sickness absence is affected by the average absence level of fellow teachers. Although most of us easily agree that such peer effects might play a role, the empirical support is rather weak. Social norms are notoriously hard to measure, let alone their influence on behavior through individual interactions. Credible approaches involve control for observable and unobservable confounders, and way(s) to tackle the innate reflection problem in the relationship between the group and the individual. Using longitudinal teacher-school data for a period of six years and with a battery of control variables, we apply different specifications. In our preferred model, we identify teachers who change schools. We compute the difference in the level of sickness absence between the new and old schools and model this as a function of differences in observed and unobserved differences in individual, school, and municipality characteristics; the key explanatory variable is the difference in the mean level of absence for teachers in new vs. old schools. The potential simultaneity between the individual and group differences is mitigated by instrumenting contemporaneous group absence with the absence in the group before the moving teacher arrived at the new school.

Unlike the papers we have referred to, we include fixed effects at the individual, municipality, *and even school level*, for departing as well as arriving schools and municipalities. It turns out that the estimated interaction effect is highly responsive to the specification of fixed effects. Controlling for fixed individual and municipality effects, the results appear to be highly significant in almost every specification. When including fixed school effects, however, the interaction effect is no longer measurable. It might be that we ask too much from the data: Unless more than one teacher moves from one specific school to another, we are not able to identify the fixed school effect from those schools. Hence, it might be that too little variation in the school dummies creates a multicollinearity problem that blurs the coefficients. The alternative explanation is that the social interaction effects in the specification *without* school fixed effects are upward biased due to the omitted school dummies. If so, the same explanation might also apply to significantly positive interaction effects reported in other analysis of norms and absence, notably Ichino and Maggi (2000) and Bradley *et al.* (2007).

Disregarding this possible bias and interpreting the estimates from our alternative specifications at face value, we find that individual self-reported sickness absence is positively correlated with the mean sickness absence of their peers. The part of the absence that needs doctor certification is, however, not significantly affected by social interaction. As for self-reported absence, the interaction effect increases with the length of exposure time since the coefficients typically increase over the three years that we are able to check in our sample. The interaction effect appears to be stronger in primary and lower secondary schools than in upper secondary schools. Finally, a non-linearity appears to present in that the group influence is stronger in schools with a high level of absence (above the mean) compared to schools with a low level.

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Tables

Table 1. Descriptive statistics over characteristics.				
	Non-movers		Movers	
	Mean	SD	Mean	SD
Individual characteristics				
Self-reported sick leave	1.06	1.64	1.05	1.65
Doctor-certified sick leave	7.96	20.48	9.01	22.77
Female (%)	0.67	0.47	0.67	0.47
Age	41.07	10.20	46.30	10.55
Seniority (years)	15.25	9.57	20.19	10.32
Adjunkt (%)	0.80	0.40	0.74	0.44
Lektor (%)	0.10	0.30	0.11	0.31
Peer group characteristics				
Avg. age	45.53	3.95	45.91	3.74
Avg. seniority	19.49	3.78	19.82	3.60
Avg. working hours	0.91	0.05	0.91	0.05
Avg. Adjunkt	0.75	0.15	0.75	0.15
Avg. Lektor	0.09	0.14	0.11	0.15
Share of women	0.67	0.18	0.67	0.17
School characteristics				
Number of teachers	26.07	19.62	28.88	21.30
Primary schools	0.40	0.49	0.37	0.48
Lower secondary schools	0.16	0.36	0.17	0.38
Combined schools	0.15	0.36	0.16	0.36
Special schools	0.01	0.08	0.01	0.09
Upper secondary schools	0.29	0.45	0.30	0.46
Municipality characteristics				
Density index (0-9)	7.07	2.33	7.17	2.31
Urbanity index (1-7)	5.73	1.92	5.74	1.90
Unemployment rate	3.17	1.09	3.24	1.10
Sickness absence rate	6.47	1.19	6.43	1.19
Human Development Index (1-10)	5.87	1.58	5.87	1.57
Expenditure on teaching per pupil	49.98	8.10	50.18	8.02
Expenditure on school maintenance per pupil	12.74	4.04	12.47	4.13
Expenditure on after-school program per pupil	18.81	4.04	18.85	4.11
Number of observations (spells)	39,584		669,716	
Number of teachers	3,136		53,080	
Number of schools	1,737		2,079	
Number of municipalities	292		349	

Note: The samples consist of schoolteachers who changed ("movers") and did not change ("non-movers") workplaces during 2001-2006. See Section 3.2 for more details.

Table 2. OLS results for movers and non-movers.

	Movers			Non-movers		
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel a: Self-reported absence			
Social interaction effect	0.475***	0.388***	0.290***	0.490***	0.416***	0.229***
	(0.031)	(0.029)	(0.034)	(0.010)	(0.010)	(0.013)
N	39,584	39,584	39,584	669,716	669,716	669,716
R^2	0.038	0.067	0.171	0.044	0.047	0.058
			Panel b: Doctor-certified absence			
Social interaction effect	0.156***	0.106***	-0.057	0.149***	0.080***	-0.176***
	(0.038)	(0.037)	(0.043)	(0.012)	(0.013)	(0.015)
N	39,584	39,584	39,584	669,716	669,716	669,716
R^2	0.020	0.047	0.161	0.016	0.013	0.015
Ind. and school char.	YES	YES	YES	YES	YES	YES
Municipality FE	NO	YES	NO	NO	YES	NO
School FE	NO	NO	YES	NO	NO	YES

Note: The samples consist of schoolteachers who changed ("movers") and did not change ("non-movers") workplaces during 2001-2006. The table displays estimation results from OLS regressions of model (1) for self-reported absence (panel A) and doctor-certified absence (panel B) . In all regressions, year fixed effects are included. Standard errors are clustered at the school level. *** significant at 1%, ** significant at 5%, * significant at 10%.

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