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ABSTRACT

Sickness Absence and Local Benefit Cultures*

In many countries, sickness absence financed by generous insurance benefits has become an important concern in the policy debate. It turns out that there are strong variations in absence behavior between local geographical areas, and it has been difficult to explain these variations by observable socioeconomic factors. In this paper we investigate whether such variation is related to group effects in the form of social interaction among individuals within neighborhoods. Well-known methodological problems arise when trying to answer such a question. A special feature of our attempt to deal with these problems is that we adopt several alternative approaches to identify group effects. We base the study on a rich set of Swedish panel data, and we find indications of group effects in each one of our approaches.

JEL Classification: H56, I38, J22, Z13

Keywords: sick-pay insurance, work absence, moral hazard, reflection problem, social norms

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1. Introduction

The build-up of the welfare state has affected individual economic behavior not only through traditional economic incentives but, most likely, also through non-economic factors such as group effects – i.e., social processes making individuals adjust their behavior to what is "normal" behavior among their peers. Although group effects have been extensively analyzed theoretically, empirical analysis has been held back by lack of data and by methodological problems.[1]

There is, however, an emerging empirical literature on group effects, dealing with such diverse fields as schooling, criminality, shirking among employees, and the individual's choice of pension plans; see, for instance, Ammermueller and Pischke (2006), Sacerdote (2001), Glaeser *et al*. (1996, 2003), Ichino and Maggie (2000), and Duflo and Saez (2002, 2003). A number of studies of group effects have also focused on the utilization of various welfare-state arrangements. For instance, Moffit (1983), Bertrand et al. (2000) and Åslund and Fredriksson (2008) have dealt with the utilization of social assistance ("welfare dependency"), Aizer and Currie (2004) have studied the utilization of publicly funded maternity care, and Hesselius et al. (2008) have analyzed the consequences for sickness absence of relaxing the requirements for medical certification.

In this paper, we ask whether individual differences in sickness absence can be explained by group effects, defined as the average utilization of the sickpay insurance in the individual's neighborhood. The empirical background for the study is that there are huge local variations in sickness absence in several countries. As will be shown later, it is difficult to explain much of this variation by observable local socio-economic factors. This raises the suspicion that there is a large variation in local social norms, which could be characterized as "local benefit-dependency cultures".

[1] For theoretical analyses of the influence of social norms on individual behavior, see, for instance, Parsons (1952), Bicchieri (1990), Manski (1993), Lindbeck (1995) and Lindbeck, Nyberg and Weibull (1999).

Our methodology is first to use a theoretical model to show that – provided group effects do exist – the outcome may be a multiple equilibrium where the utilization of the sickpay insurance varies between different regions although there are no underlying difference in population health between the neighborhoods. In the empirical analysis we use a very large data set covering all persons living in Sweden, with detailed information on area of living and the utilization of the sickpay insurance, to investigate whether or not group effects actually exists.

Of course, group effects may operate also on other arenas than local communities. For instance, national mass media may influence individuals to behave according to "normal" behavior in the nation as a whole. Social interaction may also take place within country-wide professions or organizations, including workplaces. When studying personal interaction at local levels, we define group effects as the individual's adjustment to normal behavior among his or her neighbors. The basic idea is that everyday encounters with neighbors are one important mechanism by which social norms are transmitted and upheld.[2] Indeed, in the sociological literature, it is often assumed that social norms are established and enforced through the approval and/or disapproval among neighbors (cf. Parsons, 1952).

There are several characteristics of the Swedish sick-pay insurance system that facilitate the emergence of local benefit-dependency cultures. First, the replacement rates are quite high for a majority of employees (80-90 percent of insured earnings), which is likely to create a strong temptation to "overuse" the system (moral hazard). Second, the administration of the system was quite lax during the period under study, i.e., individuals themselves could to a large extent choose whether to live on sickness benefits rather than working. Sickness spells longer than one week require a doctor's certificate, but there is strong evidence that doctors rarely turn down requests for such certificates. For instance, Englund (2008) found that doctors were prepared to provide certification in 80 percent of

[2] In one specification ("Approach A"), we also allow for influences from workmates.

the cases where the doctors themselves believed that sick leave was either not necessary or even harmful to the individual.[3]

In the literature, there is a discussion of whether observed group effects on individual behavior are caused by the dissemination of *information* or by *social norms*. For instance, Duflo and Saez (2002, 2003) point out that their estimates do not tell how much of the group effect that is due to the transmission of information and how much is due to social norms. By contrast, the study by Aizer and Currie (2004) is designed to make such a distinction for the participation in publicly funded maternity care. They assume that mothers who have previously used such care do have information about the availability of the services. The authors therefore argue that the estimated group effects for such mothers reflect social norms, rather than the transmission of information.

Since the Swedish sick-pay insurance system is mandatory and uniform, it has been easy for the authorities to inform the citizens about the rules.[4] The acquisition and interpretation of information about the system is therefore a trivial task. Mass media have also contributed to this information, and it is therefore reasonable to assume that there are very small local variations in the knowledge about the system.[5] This means that local variations in behavior can hardly be interpreted as the result of local variations in information, which strengthens our interpretation of group effects as the result of social norms, rather than of information.

Following Manski (2000), we identify three main methodological problems associated with estimating causal effects of group behavior on individuals in this context. (i) Correlated effects resulting from endogenous formation of groups; (ii) effects from common shocks generating a spurious correlation between the group's and the individual's behavior; (iii) the mechanical reflection problem in the sense that the individual's own behavior is included in the group mean.

[3] There is no limit to the number of days that an individual may receive sickness benefits.
[4] Even immigrants are informed about the details of the social insurance system when settling down in Sweden.
[5] By contrast, the pension plans studied by Duflo and Saez (2002, 2003) are quite complicated and difficult to digest; thus the dissemination of information is likely to be important in this case.

Our study employs three different strategies to deal with the first problem. (A) We investigate whether the strength of networks – defined as the share of the individual's neighbors who work at the same workplace – are correlated with the average sickness absence in the individual's neighborhood, (B) we estimate individual fixed effects, on the entire population as well as on the subgroup of individuals who move between neighborhoods; (C) we study how sickness absence behavior of immigrants to Sweden (basically refugees) relates to behavior in the neighborhood where they settle down.

There is an obvious advantage of such a "diversified portfolio method" to identification of group effects. If all three approaches show that group effects are present, this is more compelling evidence that if we had confined the study to one approach only. This is the reason why we have chosen three different approaches, each one with a different identifying assumption. One can regard this methodology as a robustness check with respect to identifying assumptions, rather than confining the robustness analysis to variations in the vector of covariates. In fact, it turns out that we can observe significant group effects for each one of our three identifying assumptions.

We combine each one of the latter two of these approaches (i.e., the use of individual fixed effects and the study of immigrants) with an instrumental variable approach in order to deal with the second problem (i.e., common shocks). As an instrument for the average absence in a neighborhood, we use the share of the population in the neighborhood who are public-sector employees. Finally, we remove the individual from all group averages in order to avoid the third problem (the mechanical reflection problem).

2. A Model of Geographic Heterogeneity

If we use the local variation in sickness absence when studying the transmission of norms, we first need to understand how such a variation can emerge in the first place. Of course, geographic variation might arise if people are inherently different, and are randomly assigned to different locations, if the number of individuals in each location is small. There may then be a variation in absence even without any social norms. But with social norms, geographic variation may arise even if all individuals are *ex ante* homogenous. To show this, we use the insurance model of Lindbeck and Persson (2010) with a representative individual.

Assume that a person's utility when working can be written

$$u^W = u(c^W) + \theta, \tag{1}$$

where θ is a continuously distributed random variable that represents the utility (when $\theta > 0$) or disutility (when $\theta < 0$) of working *per se*. Although θ could depend on many things, we will for simplicity regard it as a health variable that affects the pain (or pleasure) felt when working, but not when having leisure.[6] We use c^W to denote the consumption available to the individual when working – normally, his net wage. Using c^A to denote the consumption opportunities of the individual when absent from work,[7] we can write the individual's utility when absent from work as

$$u^A = u(c^A) - \phi, \tag{2}$$

where ϕ is the social stigma associated with living on benefits instead of working. Let us first treat ϕ as an exogenous constant. The individual is indifferent between working and staying home if θ takes such a value, say $\hat{\theta}$, that $u(c^W) + \hat{\theta} = u(c^A) - \phi$. The individual thus stays home from work if $u^A > u^W$, i.e. if the realization of the random health variable is such that

[6] In the real world, where an individual's health status is to a large extent unobservable for the insurer, the individual's decision to stay home from work may depend on a number of factors that are not immediately connected to health (like work environment, and the mood of the boss).

[7] With no sickness benefits, c^A would be zero. We assume that the individual also has some other means of support and thus *u*(0) is not equal to minus infinity.

$$\theta < \hat{\theta} \equiv u(c^A) - u(c^W) - \phi . \tag{3}$$

Let $F(\theta)$ be the distribution function of the random health term θ. Since c^A and c^W are given by the insurance system, we see from (3) that the total absence in society, π, is given by

$$\pi \equiv F\left(u(c^A) - u(c^W) - \phi\right), \tag{4}$$

And hence depends on the insurance system (c^A, c^W). For any insurance system, for instance, a system that satisfies the insurer's budget balance constraint[8] $\pi \cdot c^A + (1-\pi) \cdot c^W = (1-\pi) \cdot 1$, equation (4) yields a unique absence rate π.

Assume now that the social stigma of being absent from work is not an exogenously given constant, but is a decreasing function of the total absence in society:

$$\phi = \phi(\pi^{TOTAL}), \qquad \phi'(\pi^{TOTAL}) < 0 . \tag{5}$$

There are a large number of possible mechanisms behind such a derivative. For instance, the social stigma of being absent from work may decrease if many of one's friends and neighbors are also absent. It is also possible that one enjoys leisure activities more, if there are more people around to share the enjoyment.

Inserting (5) into (4) and setting $\pi^{TOTAL} = \pi$ in a rational expectations equilibrium with *ex ante* identical individuals, we have

$$\pi = F\left(u(c^A) - u(c^W) - \phi(\pi)\right). \tag{6}$$

[8] We assume that in individual, when working, produces one unit of the consumption good. With an absence rate $\pi = F(\hat{\theta})$, $(1-\pi)$ units will thus be produced.

Since $F(\cdot)$ is in general non-linear, equation (6) may have multiple solutions even if the $\phi(\cdot)$ function is linear – and when both functions are non-linear (which is quite realistic), the likelihood of multiple solutions is quite large. This is illustrated in Figure 1, where the left-hand side of (6) is represented by the straight, 45-degree line, and the right-hand side is represented by the non-linear curve. The way we have drawn the latter, there are three equilibria in the model, i.e., three values of π for which (6) is satisfied.

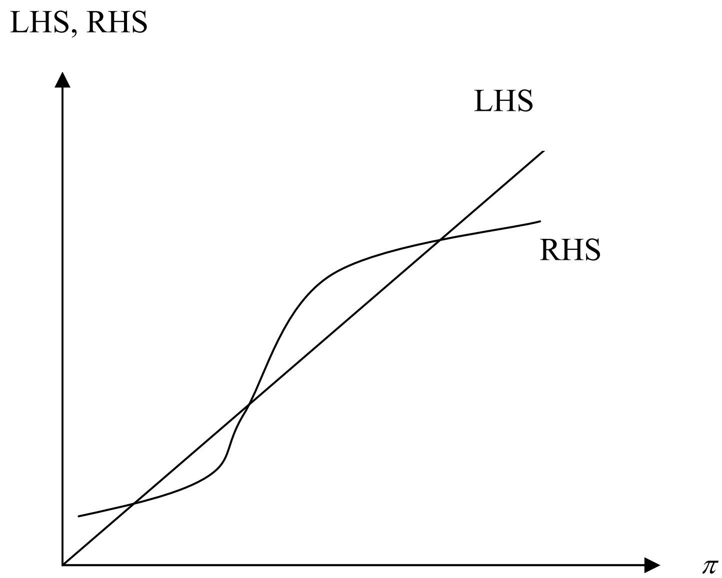

Figure 1: Multiple solutions of equation (6)

For a given insurance system (c^A, c^W), the absence rate π is thus in general not unique. And if a nation-wide system is imposed on the whole country, different equilibrium configurations are likely to emerge in different regions. Thus geographic variation in absence is likely to emerge even when individuals are homogenous *ex ante*, i.e., even if they have the same probability distribution $F(\theta)$ of the health variable.

3. A First Look at the Data

Our data set combines individual sickness absence data from the Swedish National Insurance Agency with a large number of socioeconomic variables obtained from the LISA database, compiled by Statistics Sweden. In addition to providing information on numerous individual characteristics, the combined data set allows us to identify each individual's neighborhood and workplace. The data consist of an unbalanced panel for the seven-year period 1996-2002. Although the data set covers the entire population in Sweden, we confine our study to private- and public-sector employees in the age group 18-64 (almost 5 million individuals, generating about 25 million observations in the entire panel). The data set includes all spells of sickness replaced by the national sickpay insurance. This means that only spells longer than 14 days are recorded, since the first 14 days in a spell is paid for directly by the employer.[9] Table 1 reports descriptive statistics for the dependent variable under study.

Table 1. Average number of days compensated by the sickpay insurance. Ages 18-64.

	1996	1997	1998	1999	2000	2001	2002
Average number of days	8.64	7.90	9.59	11.81	14.34	16.36	17.74
Standard deviation	41.73	40.97	44.93	50.40	56.03	60.29	62.21
Number of observations, thousands	5,336	5,348	5,369	5,389	5,414	5,439	5,466

When studying local social norms, a first issue is to determine the most relevant geographical domain. Municipalities may be too large for this purpose. We therefore chose to use the so-called Small Area for Market Statistics unit (SAMS) for geographical

[9] Individual data on spells replaced directly by the employer are not systematically reported. The total average number of paid sick days in 2002 (including short spells) was about 25 per year during the period under study, as compared to 17.3 in our data set (which does not contain the first 14 days of a spell). For the period 1997 to April 1998, the period replaced by the employers was 28 days. This implies that our data for 1997 and 1998 are not fully comparable to the other years.

domains in Sweden.[10] A SAMS area is defined by individuals living in the same type of housing within the same church parish. There are 8,951 SAMS in our database, with an average population of 404 persons. In the following, we use the term "neighborhoods" for these areas.

While acknowledging that social interaction may occur in different arenas, we focus on direct interaction at the personal level between people living in the same neighborhood, which is likely to be important when it comes to the formation and monitoring of social norms. For this purpose, the SAMS seems to be an appropriate geographical unit.

Let us first give a broad picture of local variations in sickness absence by looking at days of absence across neighborhoods during a single year, namely 2002, which is the last year in our panel. For this purpose, let S_{in} denote the number of sick days of individual *i* living in neighborhood *n* in a particular year, say 2002, and let $\bar{S}_n$ be the average number of sick days in that neighborhood. While the average number of sick days (above 14) in our data is 17.8, the standard deviation of $\bar{S}_n$ is 13.2 days in 2002. How can this wide variation across neighborhoods be explained?

First, to see whether the local variation simply reflects observable socioeconomic factors, we ran a multivariate regression of the form

$$S_{in} = \alpha + X_{in}^{'}\beta + \varepsilon_{in}, \qquad (7)$$

where the *X* vector contains three types of socioeconomic variables: individual characteristics (such as age and education), characteristics of the individual's workplace (such as industry and plant size), and neighborhood characteristics (such as urban/rural, local unemployment, and a local health variable). We have chosen explanatory variables that, in many studies, have turned out to be important for sickness absence. Due to the large number of observations, we have been able to apply a flexible specification of the

[10] See Statistics Sweden (2005) for a detailed description of this geographical specification.

regression equation, using dummies rather than specific functional forms; see Table A1 in the Appendix for a full list of the variables in the *X* vector.[11]

As expected, the *X* vector explains very little of each individual's behavior, since idiosyncratic factors tend to dominate at the individual level. More surprisingly, the *X* vector also explains very little of the variation in *average* sickness absence, $\overline{S}_n$, across neighborhoods. While the standard deviation of average absence across neighborhoods in the 2002 raw data was 13.2 days, it was almost the same (12.9 days) after controlling for all of the socioeconomic variables in the *X* vector. To find out whether the remaining differences among neighborhoods (the average residuals $\overline{\varepsilon}_n$) are systematic rather than random, we also estimated an equation with neighborhood-specific intercepts α_n:

$$S_{in} = \alpha_n + X_{in}^{'}\beta + \varepsilon_{in} \,. \qquad (7')$$

An *F* test suggests that (7') fits the data significantly better than the original specification (7) with a uniform intercept ($F = 2.650$, implying significance at the one-percent level[12]). To rule out the possibility that this simply reflects fixed unobservable factors, we also estimated (7) and (7') in terms of *changes* in sickness absence. As in the case of levels, the average residuals of changes between 2001 and 2002 across neighborhoods vary systematically, i.e., in a non-random fashion ($F = 1.370$, again implying significance at the one-percent level). Thus, there is systematic local variation in average sickness absence not accounted for by the socioeconomic factors in our *X* vector. As we have seen, this holds not only for levels, but also for changes. Indeed, this result holds for the entire panel, and not only for specific years.

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[11] We did not include income in the *X* vector because reported income is affected by the individual's sickness absence. Including income among the explanatory variables would have given rise to a bias in the estimates. Several of our explanatory variables are, however, correlated with income – for instance, age, education, gender, and industry. As regards local unemployment, there are arguments for and against including it among the explanatory variables. Here, we report the results from regressions where local unemployment *is* included – although excluding it would not change the results noticeably in terms of the influence of social norms on individual sickness absence.

[12] See, for instance, Greene (2003, chapter 13).

4. Measuring the Effect of Social Interactions

4.1 General Considerations

Our basic hypothesis is that differences in individual utilization of the sickness benefit system are causally related to differences in local social norms concerning benefit dependency. We measure group behavior by the average number of sick-absence days in a neighborhood. That is, using a simple linear framework, we aim to estimate the following relation:

$$S_{\mathrm{in}} = \alpha + X_{\mathrm{in}}^{'}\beta + \gamma \overline{S}_n + \varepsilon_{\mathrm{in}}\,, \tag{8}$$

where S_{in} is individual *i*'s utilization of the sickpay insurance; X_{in} is a vector of observable characteristics of individual *i* and the neighborhood *n* where he/she lives; $\overline{S}_n$ is the average absence of the neighbors of individual *i*; ε_{in} represents individual unobservable characteristics and random events affecting the utilization of the sickpay insurance of individual *i*.

Using OLS to estimate equation (8) may yield an upward bias to the estimate of γ , for the three reasons mentioned in the Introduction:

(i) *Unobserved heterogeneity correlated with the average utilization of the sick-pay insurance.* Individuals living in the same neighborhood may have similar unobservable characteristics, which, in turn, tend to be correlated with the utilization of the sickpay insurance. For example, it is well known that individuals with a low socio-economic status (SES) tend to have both worse health outcomes and higher utilization of all social programs. Since we are not able to observe all aspects of an individual's SES in the data it will induce a bias of the OLS estimates.[13]

(ii) *Correlated shocks affecting the utilization of the sickpay insurance.* One can think of many regional specific shocks that are likely to affect both average and individual sickpay insurance utilization in a neighborhood similarly. Examples include contagious diseases, major accidents or regional economic shocks. Previous research has documented an inverse relation between the unemployment rate and the sickpay insurance (see e.g. Larsson, 2004). Local unemployment shocks may therefore potentially induce a spurious correlation in model (2).

(iii) *The inclusion of the individual in the neighborhood mean.* This effect is sometimes referred to as the *mechanical* reflection problem, it induces a bias since outcome variable are by definition included in the neighborhood mean. Throughout we will handle this problem by simply excluding the individual from the calculation of the neighborhood average.

In addition, there may also be *measurement errors.* As explained in the data section, there are several problems related to measuring the utilization of the sickpay insurance, also as a neighborhood average. This will bias the OLS estimates toward zero.

We will use several alternative approaches to deal with the problems (i) and (ii) listed above, i.e., the correlated effects and the common shocks. One of these approaches aims at detecting *indications* of social interaction in the neighborhoods. Although not giving us a quantitative estimate of the full neighborhood effect, this approach is interesting since it relies on rather weak indentifying assumptions. Further, we use two approaches to quantify *the full neighborhood effects*. Although each of these two approaches may rely on somewhat stronger identifying assumptions than the first approach, they provide a kind of robustness test of the quantitative results; if these results are similar, regardless of which approach we use, one might argue that the results are reliable.

[13] This is labeled *correlated effects* in e.g. Manski (2000).

4.2 The Different Approaches

Approach A: Interaction between neighborhhod absence ($\overline{S}_n$) and the strength of networks. The first approach, which identifies traces of social interaction, includes neighborhood-specific effects. We use the following model:

$$S_{inw} = \alpha + X_i^{'}\beta + \nu \cdot (CA_{inw} \cdot \overline{S}_n) + \kappa_w + \mu_n + \varphi CA_{inw} + \varepsilon_{inw}, \qquad (9)$$

where the subscript *w* denotes the workplace. Here, S_{inw} is the number of days of paid sickness absence of individual *i*, living in neighborhood *n* and working at workplace *w*, in a particular year. CA_{inw} is defined as the fraction of the individual's neighbors who are also his coworkers; it can be regarded as a measure of the additional strength of the network facing individual *inw* at time *t* when he belongs to two different networks. The parameters κ_w and μ_n are fixed effects for workplace and neighborhood, respectively.[14]

The fixed effects μ_n and κ_w control for omitted variables in the *X* vector.[15] In addition, equation (9) includes the density (concentration) measure CA_{inw} separately. This allows us to control for the possibility that the strength of the network in itself may be correlated with unobservable characteristics systematically related to the propensity to be absent from work. Our identifying assumption then presumes that there is no correlation between the interaction term $CA_{inw} \cdot \overline{S}_n$ and any remaining non-observable variables that affect sickness absence, i.e.,

$$E\left(\varepsilon_{inw} \mid CA_{inw} \cdot \overline{S}_n, CA_{inw}, \overline{S}_n, X_{inw}, \mu_n, \kappa_w\right) = E\left(\varepsilon_{inw} \mid CA_{inw}, \overline{S}_n, X_{inw}, \mu_n, \kappa_w\right).$$

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[14] Equation (9) has basically the same analytical structure as the corresponding equation in Bertrand et al. (2000). In an analysis of the utilization of social assistance ("welfare" in U.S. terminology) among ethnic minorities in the United States, they studied the interaction between language groups and neighborhoods.

[15] The vector X_i in (9) is a subset of the previously used *X* vector, in the sense that neighborhood and work place characteristics have been excluded. The reason is that the neighborhood and workplace variables in *X* become redundant when we enter neighborhood and workplace fixed effects into the regression equation. The network-intensity variable only varies on the neighborhood/workplace level; we therefore adjust the standard errors for clustering within the cells consisting of the intersection of neighborhoods and workplaces (see e.g. Moulton, 1986).

The interaction model deals with the endogeneity problem and the problems with correlated shocks, i.e., items (i) and (ii) in the list of problems with the OLS approach, by the inclusion of fixed neighborhood and workplace effects. However, measurement errors will still bias the estimated effects towards zero.

The coefficient ν in equation (9) has no direct interpretation. To get the "marginal effect" of the average utilization of the sickpay insurance in the neighborhood, it is straightforward to differentiate equation (9) with respect to $\overline{S}_n$. This gives us $\nu \cdot CA_{inw}$ as the marginal effect on an individual's sickness absence from an increase in the average number of sick days, $\overline{S}_n$, in his neighborhood.

It is important to note that this marginal effect does not measure the full effect of social interaction at the neighborhood level. It measures only the magnification of interaction effects that are caused by neighbors also meeting each other at the workplace. The fixed neighborhood effect will be a proxy for, among other things, all possible other channels for social interaction at the neighborhood level, that we also want to measure in this study. The estimates from this model would therefore serve as a conservative "lower bound" for the full effect.

Approach B: Controlling for unobserved heterogeneity by using individual fixed effects. To estimate the full effect of social interaction on the neighborhood level, we use three different approaches. The first of these is to use panel data to estimate (8) with individual fixed effects:

$$S_{in} = \alpha_i + X'_{int}\beta_1 + \overline{X}_{nt}\beta_2 + \gamma\,\overline{S}_{nt} + \lambda_t + \varepsilon_{int}\,, \qquad (10)$$

where λ_t is a fixed time effect. The reason why we include the neighborhood average $\overline{X}_{nt}{}'$) in the regression is that we want to control for neighnorhood-specific observables with respect bto demography (avergae age, education, etc.). The identifying assumption is that the error term, conditional on observables and individual and time fixed effects, is independent of the average utilization of the sickpay insurance in the neighborhood, i.e., $E\left(\varepsilon_{\text{int}} \mid \bar{S}_{nt}, X_{\text{int}}, \bar{X}_{nt}, \alpha_i, \lambda_t\right) = 0$. As mentioned above, an obvious candidate for violation of this assumption is common neighborhood level shocks. If people living in the same area are affected by, for example, a contagious disease or a natural disaster, this will induce a correlation between the interaction and the error term in model (10).[16] To deal with this problem, as well as the attenuation bias caused by measurement errors, we use an instrumental variable for the average utilization of the sickpay insurance. We use the share of public sector workers in the neighborhood as instrumental variable.

The rationale for our choice of instrument is that in a number of previous studies, it has been recognized that public-sector employees have a higher work absence rate than private-sector employees in most countries. A plausible explanation to this empirically observed pattern is that, since work absence is generally costly for the employment, private sector employers have stronger incentives to organize their workplaces to avoid work absence. It could also be the case that workers with preferences for frequent absence tend to self-select into the public sector. For these reasons, neighborhoods with a large share of public-sector employees are, on average, likely to have a higher work-absence rate than other neighborhoods.

The identifying assumption underlying this approach is that the share of public-sector employees in a neighborhood is unrelated to unobserved random shock affecting work absence behavior; formally, $E\left(Z_{nt}\varepsilon_{inwt} \mid X_{\text{int}}, \alpha_i, \lambda_t\right) = 0$, where Z_{nt} is the public sector's share of employment in neighborhood *n* in year *t*.[17] Thus, conditional on all the variables including neighborhood fixed effects, we assume that any shock in the individual's sickness is uncorrelated with the share of public sector employees in the individual's neighborhood. It is important to note that we *do not* assume that workers with specific absence behavior do not choose to reside in neighborhoods on the basis of the proportion

[16] The most likely contageous disease that could create neighborhood-specific variation in sickness would be an ordinary flu epidemic. This would however not be reflected in our data, since we exclude absence spells shorter that fourteen days (cf. footnote 9).
[17] More exactly, *Z* is the ratio of the number of public-sector employees to the sum of public- and private-sector employees.

between public- and private-sector employees in these neighborhoods. The assumption *we do make* is much weaker.

A variation of Approach B: The behavior of movers. The identifying variation in the fixed effects model comes from two separate sources. Partly, it comes from changes over time in the average utilization of the sickpay insurance in the neighborhood. But it comes also from the fact that some individuals move from one neighborhood to another with different average utilization of the sickpay insurance. In our alternative version of estimating the full effect of social interaction by using fixed individual effects, we restrict the sources of variation to the second one by limiting our sample to movers between neighborhoods. Denoting the old neighborhood by *m* and the new by *n*, we estimate the following version of equation (10):

$$
\begin{aligned}
&S_{int}^{mover} - S_{im,t-1}^{mover} = \\
&= (X_{int}^{mover'} - X_{im,t-1}^{mover'})\beta_1 + (\overline{X}_{nt}^{all'} - \overline{X}_{m,t-1}^{all'})\beta_2 + \gamma(\overline{S}_{nt}^{non-mover} - \overline{S}_{m,t-1}^{non-mover}) + \lambda_t + \varepsilon_{int}
\end{aligned} \quad (11)
$$

We use this specification to investigate whether people who move from neighborhood *m* to neighborhood *n* adjust their behavior in response to the difference in average absence between these two neighborhoods.

Our identifying assumption in this analysis is that people who plan to *change* their absence behavior in the future do not tend to move to neighborhoods with a particular level of average sickness absence. This means that people are assumed to move for a variety of reasons (such as changes in the family, in the job prospects, etc.) but not as a result of expected future changes in their own sickness absence. Again, we use the instrumental variable approach to handle the problems with common shocks and measurement errors.

Approach C: Immigrant interaction with natives in the neighborhood of placement. Finally, our second empirical strategy to estimate the full effect of social interaction at the neighborhood level is to restrict the analysis to newly arrived immigrants. We use the following model:

$$S_{int}^{f} = \alpha + \lambda_t + X_{int}^{f\,'}\beta_1 + \overline{X}_{int}^{f\,'}\beta_2 + \gamma\,\overline{S}_{nt}^{s} + \varepsilon_{int}\,, \qquad (12)$$

where S_{int}^{f} is the number of sick days of immigrant *i* in neighborhood *n* at time *t*, while $\overline{S}_{nt}^{s}$ is the average number of sick days among native Swedes in that neighborhood.

The idea behind this strategy is that we avoid the problem of endogenously formed neighborhoods since the largest part of the immigration to Sweden during the time under study was from refugees that were initially placed in neighborhood with vacant housing. An advantage with this strategy is that we are able to investigate whether immigrants with a cultural background similar to that of Swedes tend to adjust more than other immigrants to the behavior of native Swedes. The rationale for this question is that one would expect that such immigrants are particularly likely to interact with Swedes.

As we want to study the transmission of norms to immigrants, it is natural to exclude neighborhoods where immigrants constitute a majority of the population. Indeed, we confine this regression to neighborhoods where the fraction of immigrants is less than 30 percent of the total population.[18]

Again, to handle the common shocks problem we use the instrumental variable approach and the share of public sector employees as instrumental variable.

5. Results

5.1 Approach A: Interaction between neighborhood absence ($\overline{S}_n$) and the strength of networks.

Table 1 shows the estimates from the interaction model (9), estimated on data from 2002. In addition to the estimates of the interaction coefficient, $\hat{\nu}$, Table 1 also reports the point

[18] We also tried 20 and 50 percent; the results are quite insensitive to the choice of cut-off value.

estimates of the marginal effects of changes in the average utilization in the neighborhood, $\partial S_{inw} / \partial \overline{S}_n$ (for each individual we have $\partial S_{inw} / \partial \overline{S}_n = \nu \cdot CA_{inw}$, but in the table we report the nation-wide average i.e., $\nu \cdot \overline{CA}$) This number tells us how an increase in the average absence $\overline{S}_{nt}$ in a neighborhood influences individual absence through the interaction between neighborhood and workplace networks.

It can be seen in Table 2 that estimates of ν are significantly different from zero, i.e., we can strongly reject that individual utilization of the sickpay insurance is independent from the average utilization in the neighborhood. It is also apparent that there is a very small difference between the two specifications – including and excluding controls for individual observable characteristics, respectively. As expected, the point estimate of 0.073 is small since it does not reflect the full effect of local ineteraction, but only the extra effect due to interaction with neighbors who are also workmates. Nevertheless, the estimate is statistically highly significant, indicating that there are effects on sickness absence of social interaction.

Table 2: Estimates from the interaction model measuring the strength-of-network effect on sickpay insurance utilization.

	(1)	(2)
$\hat{\nu}$	3.642***	3.421***
	(0.434)	(0.399)
$\partial S_{inw} / \partial \overline{S}_n = \hat{\nu} \cdot \overline{CA}$	0.078	0.073
X vector included	No	Yes
N		
*N*T*	3,595,798	3,595,798
R^2	0.095	0.108

Note: *** indicates significance at the one-percent level.
The numbers of observations and individuals in this table are somewhat smaller than the corresponding numbers in subsequent tables. The reason is that for each individual, we deleted the

individual himself from the data when computing the averages $\overline{S}_n$. For some neighborhoods, there is only one individual who works in each workplace; these cases therefore do not appear in the regression.

5.2 Approach B: Controlling for unobserved heterogeneity by using individual fixed effects.

Let us first look at the estimates obtained from *the whole population*, and later turn to the *movers*. Table 3 shows the results from our estimation of the fixed-effects model (10). For computational convenience, given the very large number of observations, we have estimated the model in first differences, using White's robust standard errors accounting for possible heteroscedasticity or autocorrelation in the error term. We have also restricted the sample to three years, between 1999 and 2002. Columns 1 and 2 show the estimates when the model is estimated using OLS, including and excluding potentially time-varying controls for confounders, respectively. Columns 3 and 4 show the corresponding results from the estimation of the first stage and, finally, column 5 and 6 show the IV estimates.

As expected, the estimates shown in column 1 and 2 are larger than those obtained from the strength-of-networks model (Table 2), which only measured a partial effect of the social interaction on the neighborhood level. Column 3 and 4 show that we have a very strong first step, partly reflecting the fact of the large number of observations included in our data. Despite this fact, the precision in the IV estimates in the specification shown in column 5, excluding controls for confounders, is not sufficient to make it significantly different from zero at the 10 percent level. However, the results from the specification when confounders are included reported in column 6 are highly significantly different from zero. The estimate 0.366*** means that if the average neighborhood absence increases by one day, the individual would, as a consequence of this, increase his absence by 0.366 days per annum. This estimate is significantly larger than the corresponding one shown in column 2, meaning that we would reject the null hypothesis in a Hausman test.

In interpreting this result, the effect of common shocks would bias the OLS results upwards and the measurement errors downwards. The results presented in columns 2 and 6 indicates that the latter effect dominates the first.

Table 3: First difference-IV estimates of the effect of the average utilization of sickpay insurance on individual utilization. Standard errors in parentheses.

	OLS		First stage		IV	
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\eta}$	0.101***	0.108***	-	-	0.262	0.366***
	(0.010)	(0.012)			(0.164)	(0.075)
Change in share in private sector			-3.288	-8.044		
	-	-	(0.017)	(0.017)	-	-
Inluding X and $\bar{X}$	N	Y	N	Y	N	Y
$N*T$	2,204,170	2,204,170	2,204,170	2,204,170	2,204,170	2,204,170
R^2	0.000	0.002	0.003	0.269	0.000	0.002

Note: Sample covering the period 1999 to 2002.

Let us now look at the movers between neighborhoods with different level of sickness absence. Table 4 shows the first set of estimates from equation (11). Columns 1 and 2, with OLS estimates, show that the estimates of the overall effect is, although significant, quite small. According to the estimate including controls for observable characteristics, one additional day in average neighborhood sickness absence leads to 0.019 days in individual sickness insurance utilization. The results shown in columns 3 and 4 suggest that the fraction of public-sector employees is quite a strong instrument for the average sickness in a neighborhood. Finally, columns 5 and 6 show the IV estimates of $\hat{\gamma}$. According to these, an increase in average neighborhood sickness by one day leads to an increase in individual absence by between 0.505 and 0.222 days, estimates that are well in line with the fixed-effects estimates for the whole sample reported in Table 2.

Table 4: First difference estimates of the effect of the average utilization of the sickpay insurance in individual utilization from movers between neighborhoods.

	OLS		First stage		IV	
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\gamma}$	0.042***	0.019***	-	-	0.505***	0.222***
	(0.0062)	(0.0071)			(0.184)	(0.054)
Change in share in private sector			-1.793***	-6.519***		
	-	-	(0.036)	(0.044)	-	-
Inluding X and $\bar{X}$	N	Y	N	Y	N	Y
$N*T$	2,204,170	2,204,170	2,204,170	2,204,170	2,204,170	2,204,170
R^2	0.000	0.002	0.0012	0.2448	0.0005	0.0028

Note: Sample covering the period 1999 to 2002.

5.3 Approach C: Immigrant interaction with natives in neighborhood of placements

Table 5 presents the results from the analysis of immigrants. Column (i) shows the OLS results including only neighborhood characteristics in the specification to control for observable confounding effects; column (ii) the results when including fixed neighborhood effects – identifying on changes in the neighborhood average utilization of the sickpay insurance; column (iii) shows the 2SLS-IV results when we use the neighborhood share of public sector employees as instrument for average sickpay insurance utilization; the last column gives the *P*-value for the excluded instrument in the first stage regression.

The highly significant estimate of the overall effect, estimated on the entire group of immigrants, suggests that a one day higher work absence rate leads to an about 0.4 days increase in the utilization of the sickpay insurance among immigrant. Comparing the estimates in column (*i*) and column (*iii*), it can be seen that they are remarkably similar and a Hausman test would not reject the null hypothesis of no difference. It should also be noted that they are significantly larger than the ones from the previous methods. However, they are obtained on different populations and therefore no fully comparable.

The estimates obtained on immigrants originating from the Nordic countries are significantly larger than for the ones on the entire group, suggesting that there is a stronger social interaction with the native population among immigrant groups that are culturally close to the natives. It seems to be an overall pattern across the immigrant groups that estimates decreases with cultural distance to the Swedish population.

Table 5: Estimates of immigrant adoption to social norms on sickpay insurance utilization among natives by immigrant group. (*i*) the $\bar{X}$ vector included; (*ii*) fixed neighborhood effect; (*iii*) IV and $\bar{X}$ vector included. P-values for the instrumental variables in the first step of the 2SLS estimates.

Region	Number of ind. and obs.	Estimate of γ (*i*)	(*ii*)	(*iii*)	*P*-value, instrument first stage
All regions	618,460 ind. 2,756,607 obs.	0.392*** (0.0120)	0.357*** (0.0106)	0.396*** (0.0199)	0.000
Nordic	193,221 ind.	0.602***	0.562***	0.786***	0.000

countries	974,791 obs.	(0.0215)	(0.0197)	(0.0386)	
EU (except Nordic countries)	72,067 ind. 323,704 obs.	0.318*** (0.0320)	0.422*** (0.0281)	0.623*** (0.0553)	0.000
Europe (except EU)	130,641 ind. 588,651 obs.	0.223*** (0.0269)	0.220*** (0.0244)	0.084** (0.0419)	0.000
Africa	28,924 ind. 110,887 obs.	0.166*** (0.0496)	0.160*** (0.0452)	0.036 (0.0886)	0.000
North America	19,886 ind. 81,298 obs.	0.164*** (0.0492)	0.177*** (0.0426)	0.266*** (0.0793)	0.027
Latin America	30,158 ind. 126,665 obs.	0.310*** (0.0536)	0.325*** (0.0459)	0.408*** (0.0890)	0.025
Asia	136,059 ind. 518,147 obs.	0.306*** (0.0248)	0.141*** (0.220)	0.044 (0.0386)	0.000
Oceania	3,405 ind. 12,951 obs.	0.151 (0.0967)	0.270*** (0.0869)	0.170 (0.1641)	0.872
Former Soviet Union	3,894 ind. 18,926 obs.	0.291* (0.1547)	0.114 (0.1196)	-0.063 (0.2328)	0.003
Including $\overline{X}_{nt}$ vector		Yes	No	Yes	Yes
Including fixed effects μ_n		No	Yes	No	No

*** indicates significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

6. Conclusions

Previous research has shown that variations in the replacement levels and control affect the utilization of sickpay insurance programs.[19] This means that the insurer has viable policy tools to influence the utilization of the insurance. If the insured individuals are affected by the work absence behavior in their peer groups, changes in work absence rates as a response to policy changes tend to be magnified through this social multiplier. This is important to consider when interpreting changes in work absence rates over time and when designing a well-functioning social insurance program. The aim of this study has been to investigate if there are social interaction effects on the local neighborhood level in the utilization of the Swedish sickpay insurance and, to the extent that there are such effects, the economic importance of them.

The first set of results from the interaction model shows statistically significant estimates for that effect of social interaction on the neighborhood level through interaction with concentration of people also working at the same work place. Although we believe these estimates to be robust with respect to the methodological problems raised in Section 3, a major disadvantage of this method is that they only measure the additional effect through workplace interaction of social interaction on the neighborhood level. It is, thus, not possible to draw any conclusions of the economic importance of the form of social interaction under study in this paper on the basis of these results.

The second set of estimates is obtained on panel data. First, fixed effects models estimated on the entire population, identifying on changes in neighborhood average sickness absence and on movers between neighborhoods. Second, we obtain first difference estimates when restricting the sample to movers between neighborhoods with different levels of average work absence. The point estimates from preferred IV models are 0.366 and 0.222, respectively, which, in turn, yielding estimates of the social multiplier $(1/(1-\gamma))$ on 1.29 and 1.58, respectively. This means that a policy that in

[19] On replacement levels, see e.g. Johansson and Palme, 1995 and 2005 or Henrekson and Persson, 2004. On control, see e.g. D'Amuri, 2011.

absence of social interaction on the neighborhood level would increase utilization on the neighborhood level with one percentage point, would have a 29 or 58 percent larger effect when social interaction is taken into account, depending on the estimates used.

The last set of results refer to a different population: newly arrived immigrants to the Swedish labor market. The estimates for different groups, defined by geographical region of origin, support the hypothesis that social interaction effects are present on the neighborhood level. The point estimates for the entire group of immigrants are similar, although slightly larger, than the ones discussed in the previous paragraph. The estimates obtained on different sub-groups are in general stronger for groups that are ethnically closed to the native population, which supports the idea that closer interaction produces stronger results.

This study has several limitations. Norms for how social programs should be used are also formed in other areas of the society than in neighborhoods, such as via local or national mass media. Never the less, this study shows that the estimated partial effect on absence behavior of social interaction on the neighborhood level is both statistically significant and economically important.

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Appendix

Table A1: Explanatory variables in the *X* vector

For the individual	*Age* (all ages from 18 to 64, one dummy for each age, i.e., 46 dummies) *Education* (seven levels, one dummy for each level, from primary school to graduate university degrees, i.e., six dummies) *Gender* (one dummy) *Marital status* (single, married/cohabitating, divorced; two dummies) *Has children aged 3 or younger* (one dummy) *Region of origin* (Sweden, Northern Europe, rest of Europe, etc.; 10 dummies)
For the workplace	*Industry* (60 industries, i. e., 59 dummies) *Sector* (central government, state-owned enterprise, local government, local government-owned enterprise, private firm, etc.; 11 sectors, i.e., 10 dummies)* *Size of workplace* (21 dummies: 1 employee, 2-10, 11-20, 21-30, …, 91-100, 101-200, 201-300, …, 901-1000, 1001-9999 employees)
For the neighborhood	*Urban or rural* (one dummy) *Life expectancy in the municipality* (average, gender-specific life expectancy among the 291 municipalities in Sweden) *Local unemployment* (expressed as the incidence of unemployment, i.e., the fraction of the labor force in the neighborhood that has received unemployment compensation at least once during the year. 19 dummy variables, one for each 5-percent interval)

* The distinction between industry and sector is that the former refers to the type of product or service produced, while the latter refers to ownership characteristics.

Table A2: Estimates of the "placebo" regressions (7a) and (7a') with and without the $\bar{X}$ vector.

Population	Number of individuals and observations	Regressor	Reduced form: μ in eq. (4a)	
All those who work in private sector	2,839,410 ind. 14,556,753 obs.	Share of population in neighborhood *n* that works in public sector (Z_{nt})	0.034*** (0.0041) R-square: 0.2037	0.113*** (0.0046) R-square: 0.2244
All those who work in public sector	1,956,740 ind. 10,502,405 obs.	Share of population in neighborhood *n* that works in private sector $(1-Z_{nt})$	-0.165*** (0.0047) R-square: 0.2138	-0.199*** (0.0052) R-square: 0.2311
All employees	4,796,150 ind. 25,059,158 obs.	Share of population in neighborhood *n* that works in public sector (Z_{nt})	0.085*** (0.0031) R-square: 0.2315	0.148*** (0.0034) R-square: 0.2501
Including $\bar{X}_{nt}$ vector			No	Yes