Is work absence contagious?*

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Abstract

A unique randomized controlled experiment is used to study social interactions in the Swedish sickness insurance. The experiment was conducted in the Gothenburg municipality during the second half of 1988. Those born an even day where treated and those born an odd day remained untreated for the same period. The treated where monitored less hard and this treatment increased their sickness absence. Panel data for the 1987-1989 period, including information on every sickness absence spell is used in the estimations. Estimations using difference in difference estimations strategies are performed on the prevalence, incidence and duration of sickness absence. The results shows that the endogenous effects exist in the Swedish sickness insurance, and that a 10 percent increase in the means absence would decrease the hazard from work absence to work by about 1.7 percent because of endogenous interactions.

Keywords: Cox regression, Difference in difference; Hazard from absence, Incidence into work absence; Prevalence of work absence; Network

JEL: Z13, J22. C14, C23, C41

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1. Introduction

Figure 1 shows the sickness absence rates, from the National Social Security Board (NSSB), for males and females in Sweden 1974-2005. The variation in sickness absence is, at least to some extent, explained by changes in the institutions and differences in data collections (see e.g. Hesselius (2006) for a description of the reforms and problems with data registering). However, the presence of social interactions has also been put forward as an explanation for the large variation (see e.g. Lindbeck et. al. 2004). Social interactions or difference in norms has also been suggested as determinants for the large variation in sickness absence between regional areas (and even local areas) in Sweden. The effects of social interaction and norms has also been an important topic in the Swedish policy discussion lately.[1]

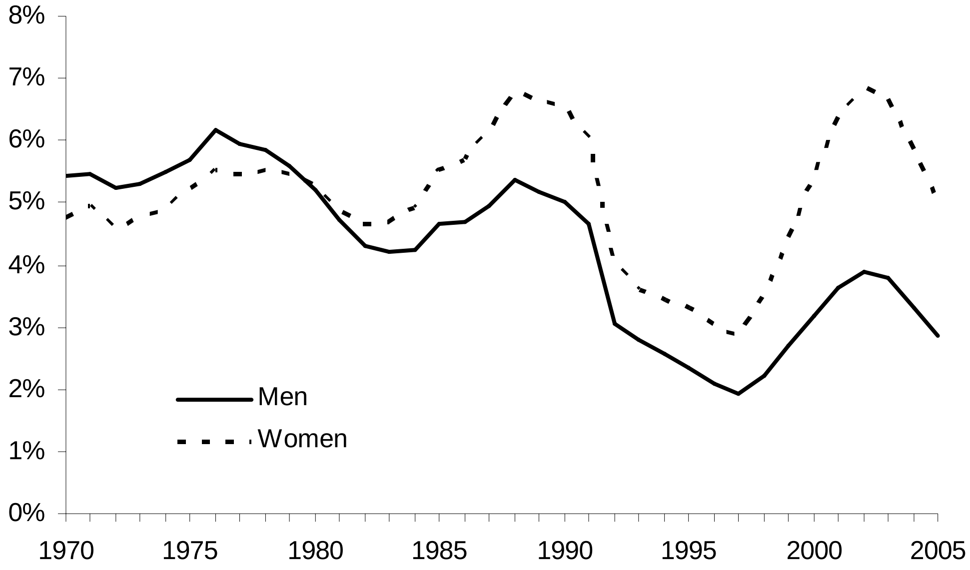

Figure 1. The sickness-absence rate of working-age population (16-64 year olds) during 1970 to 2005.

In this study a randomized controlled experiment conducted in Gothenburg municipality during the second half of 1988 is being used to identify an endogenous (or social multiplier) effect. The randomized controlled experiment implied that half of the inhabitants (born on an even day) in the Gothenburg municipality were exposed to less control of their eligibility to use the sickness insurance. The treated did not need to submit a doctors certificate before day

[1] See e.g. Socialförsäkringsutredningen - samtal om socialförsäkring, 2005(1,3).

eight of every sickness period, instead they were allowed to have fourteen days of “own” decided sickness absence before submitting a doctors certificate. The direct treatment effect of the experiment has been investigated by Hesselius, Johansson & Larsson (2005). Their results show that the relaxed control increased the duration of short-term sickness absence, and had an insignificant effect on the incidence into sickness absence. Thus control of the eligibility during the benefit period is essential.

When social interaction effects are identified it is in general important to have prior information about what constitute an individuals network. It is not possible to deduce these networks simply from the data (Manski, 1993). Individuals are involved in different networks, at work, in the residence neighborhood, ethnic group and in different associations. Usually the definition of networks are often rather ad hoc and assumes that individual interact in a small local environment (see e.g. Bertrand, Luttmer & Mullainathan (2000) and Åberg, Hedström & Kolm (2003)).

To investigate the social interactions we assume that networks are constituted by immigrants from the same country of origin. We utilize the fact that the experiment only where conducted in Gothenburg municipality and assume that immigrants interact in the whole Gothenburg region. This means that each immigrant in the Gothenburg metropolitan statistical area (MSA) will have a different proportion of directly treated in his/her network, depending on where those in his network live (outside or inside Gothenburg municipality). Thus, also here an ad hoc definition of the network is used. However we do not need to rely on social interaction within small local areas, instead we assume that immigrants interact with their ethnic group in the whole Gothenburg MSA. In Sweden, and in many other countries, the connections between immigrating ethnic groups are often close, for example through ethnic group associations. It is thus reasonable to believe that immigrants not only interact with their ethnic groups in their local neighborhood. Ethnic associations and religious meeting places are two obvious interactions places.

Our study adds evidence to the rather small empirical literature investigating social interactions in social insurance.[2] There are a couple of studies investigating social interactions in unemployment, see e.g. Åberg, Hedström & Kolm (2003) and Clark (2003). Ichino & Maggi (2000) and Lindbeck, Palme and Persson (2004) studies social interactions in work

[2] The effects of benefits, experience rating and monitoring and sanctions in the social insurance system is both theoretically and empirically thoroughly investigated. See Fredriksson & Holmlund (2003) for an review of the unemployment insurance monitoring literature and Hesselius, Johansson & Larsson (2005) offers evidence for monitoring of sickness absence. For studies concerning experience rating, for unemployment see e.g. Topel (1983, 1985), Card & Levine (1994) and Anderson & Meyer (1994, 2000), and for disability/sickness see e.g. Koning (2004), Hyatt & Thomasson (1998), Ruser(1985, 1991, 1993), Chelius & Kavanaugh (1998) and Moore & Viscusi (1989). Finally there are a couple of studies on Swedish data which suggest that the sickness benefit size play an important role, see e.g. Johansson & Palme (1996, 2002, 2005), Larsson (2005) and Henreksson & Persson (2004).

absence. In the latter, the authors finds that the large workplace variation in absence still remains after controlling for observed variables. In the former, the authors investigate the absence differences between south and north Italy, using a sample of bank employees. In one approach they focus on movers, to control for individual unobserved effects. They find significant social interactions, but one basic problem is that the reason for moving may be endogenous.

These prior studies have established that network and/or neighborhood effects seem to be important for explaining differences in social insurance use. But none of the studies have used an exogenous intervention, which is necessary to really separate social interactions from other group common effects. They don't either attempt to separate endogenous effects from exogenous social interaction effects. Our study extends to the prior literature, by using a local intervention, which allows us to identify endogenous social interaction effects and calculate a dynamic multiplier.

The empirical analysis is based on the IFAU database to which we have matched sickness absence data on all individual sickness absence in Sweden 1987-1989 from the National Security Board (NSB). This allows us to control for individual and networks effects and also to perform extensive sensitivity checks.

The main result is that we find statistically significant endogenous social interaction effects. The effect is quite large. A 10 increase in the means absence would decrease the hazard from work absence to work by about 1.7 percent because of endogenous interactions.

The rest of the paper has the following organization. The Swedish sickness insurance system and the randomized controlled experiment conducted in Gothenburg are both explained in Section 2. The empirical identification strategy is discussed in Section 3. Section 4 describes the data, the sample selection made and provides a first look at the data. A theoretical framework is outlined in Section 5. Section 6 presents the regression models and the empirical results. Finally, Section 7 concludes.

2. The Swedish Sickness Insurance and the Experiment

2.1 The Swedish Sickness Insurance

Sweden has compulsory national sickness insurance. It is financed by a proportional payroll tax and replaces earnings forgone due to temporary health problems that prevent the insured worker from doing his regular job. All employed workers are covered by the insurance. Benefits are related to the lost income during the sick spell.

Sickness benefits are and have been rather generous: in 1988, a vast majority of workers received 90 percent of their lost income from the public insurance. A benefit cap excluded workers at the very top of the income distribution from receiving a full 90 percent. However, besides the public insurance, most Swedish workers are covered by negotiated sickness insurance programs regulated in agreements between the labor unions and the employers' confederations. In general, these insurances replace about 10 percent of forgone earnings, but there is considerable variation. In addition there were no qualification day in 1988.

The public insurance has no limit for how often or how long benefits are paid. Many sick spells continue for more than a year but there are examples of even much longer durations. These long spells ends mostly in early retirement or in retirement.

Since compensation levels are high, one would expect monitoring of the benefit claimants to be strict. However, this is not the Swedish case; sickness benefits are paid for a week before checking the claimants' eligibility. A sick spell starts when the worker calls the public social insurance office (and her employer) to report sick. Within a week, at latest on the 8th day of sickness, the claimant should verify eligibility by showing a doctor's certificate that proves reduced working capacity due to sickness. The public insurance office judges the certificate and decides about further sick-leave. It is very rare that the certificate is not approved, at least in 1988.

Of course, some exceptive rules make it possible for the public insurance offices to monitor more (or less) strict. In case they suspect abuse, they can visit the claimant at home. Claimants who have been on sickness benefits too many times during the past year may be asked to show a doctors certificate from day one. Moreover, a new sick spell starting within five working days from the first is counted as a continuation of the first making it impossible to report sick every Monday (and returning 'back to work' for the weekends) without ever visiting a doctor. Persons with chronic illnesses, on the other hand, do not necessarily have to verify their eligibility each time the illness forces them to stay at home from work.

2.2 The Randomized Control Experiment

The experiment we use to identify the effect of social interactions was carried out in the second half of 1988 in Gothenburg municipality, the second largest city in Sweden.[3] It was initiated by the local social insurance offices.[4]

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[3] The same experiment where conducted in Jämtland, a large county in the sparsely populated Northern part of Sweden. There population is small and it is also difficult to define networks due few immigrant. Therefore we only use the experiment in Gothenburg in our empirical analysis. Hesselius et. al. (2005), which investigate the direct effects of the reform use the experiment in both Gothenburg and Jämtland.

[4] Until recently, the public insurance was administered by 21 independent local social insurance offices that were quite free to design exceptions from the general rules (as long as they were towards more generosity). Today, the administration is centralized.

The purpose of the experiment was to see whether and how sickness absence is altered when monitoring of the insurance claimants is reduced. A randomly assigned treatment group was allowed to receive sickness benefits for two weeks without showing a doctor's certificate, instead of one week as usual. The randomization was performed by using the date of birth. All insured born on an even date were asked to show a doctor's certificate after two weeks, whereas insured born on an odd date had to show one already after one week.

The insurance authorities had several arguments for running the experiment. In short, all of them were based on an idea that it would imply saving and less sickness absence. First, unnecessary visits to a doctor would decrease implying less cost for individuals, the medical care system and thereby for the state budget. The implementing authorities also believed that doctors, in a routine way, prescribe longer absence from work than necessary. With a two week's time limit, many individuals would have time to get back to work before receiving any such prescription. Finally, and perhaps somewhat contradictory to the above arguments, some sick spells were indeed expected to get longer, but for a good reason as sick individuals no longer were pushed back to work. This in turn would decrease illness recurrence of those individuals.

The experiment was a non-blind experiment in that all were informed about it in advance or at latest during the experiment. In fact, it was preceded by quite massive local information campaigns. Besides the personnel at the local social insurance offices, all employers and medical centers were informed in advance about the set-up of the experiment. Also the mass media were an important channel to inform the insured.

The direct effects of the experiment have been evaluated by Hesselius, Johansson and Larsson (2005). They use data from the National Social Insurance Board to reconstruct the treatment and control samples. There are no significant differences between the treatment and control groups with respect to any of the important characteristics including absence prior to the reform, thus the randomization seem to be valid. To strengthen the argument that the experiment was well conducted we display in Figure 2 the fraction still absent due to sickness the half year before the experiment was conducted. From this figure it is evident that there is no difference between the controls and treated in the hazard rate to return from a work absence spell before the experiment was run.

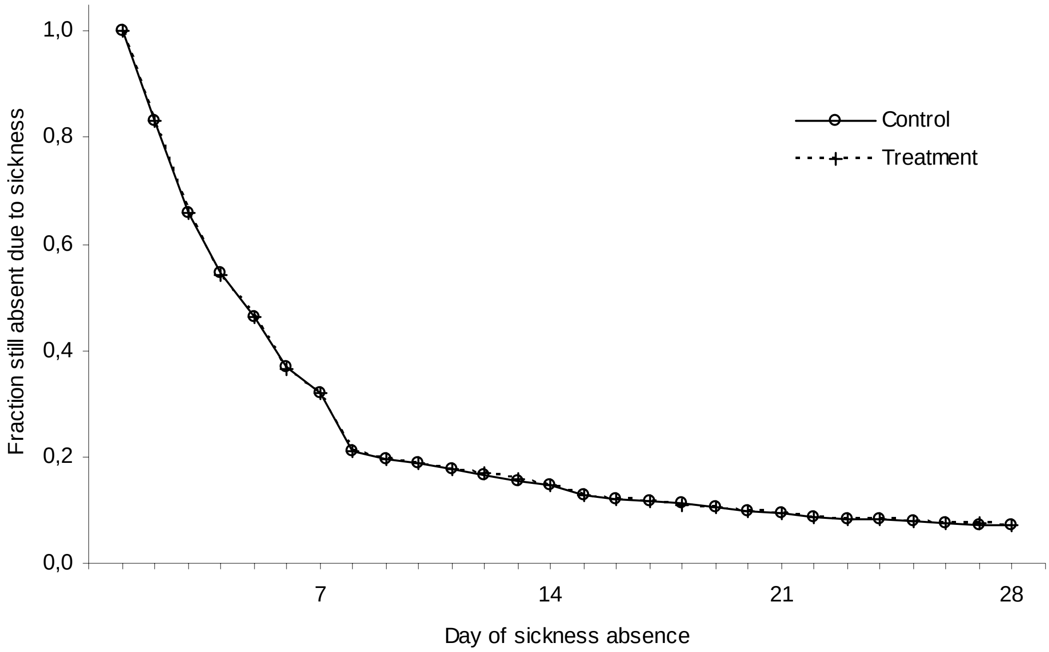

Figure 2: Fraction still absent due to sickness in Gothenburg during the half year before the experiment period (1/1/88 – 6/30/88). Source: Hesselius, Johansson and Larsson (2005)

The empirical results in Hesselius, Johansson and Larsson (2005) shows that less strict control increases the duration of absence but have no significant effect on the incidence into sickness absence. Figure 3 displays the survival functions of the treated and control for the first 28 days in an absence spell, respectively. From this figure we can see that the treatment in form of two weeks own decided absence, affected mainly the hazard rates for the first fully 14 days of every sickness absence period.

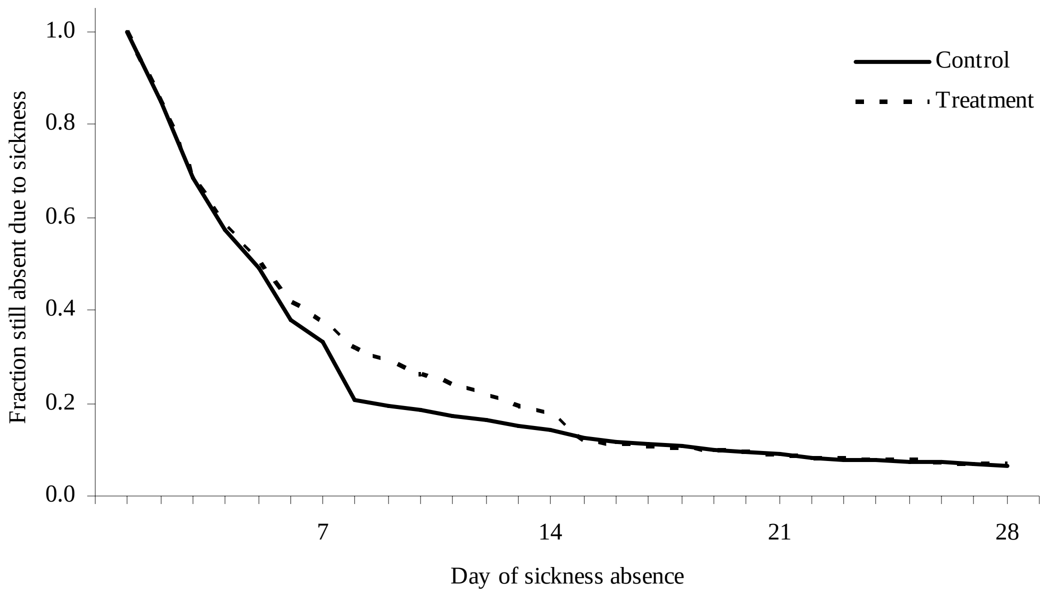

Figure 3. Fraction still absent due to sickness in Gothenburg during the experiment period.

3 Identification strategy

The studies refereed to in the introduction indicate that network and neighborhood effects are important. However, empirical identification of social interactions is problematic and therefore requires a rather lengthy discussion. The requirement for identifying endogenous effects are strict (cf. Manski (1993 and 2000)). In order for an intervention to be useful for identification it is required that the intervention: (i) only affect a proportion of the individuals in each network and the proportion have to differ between at least some of the groups, (ii) has to be exogenous with respect to unobserved variables (or at least with trends in those) and (iii) cannot change the group composition.

The first requirement is trivial since if all individuals are directly treated then there is obviously no possibility to disentangle the direct treatment effect from the social interaction effect. Requirement (ii) and (iii) enables identification of the combination of the direct treatment effect and the social interaction effect, because we can separate this combined effect from effects depending on, for us, unobserved variables.

In order to generalize the results and calculate a dynamic multiplier we need a forth requirement that is not often discussed in the social interactions literature: (iv) the affected must be representative for the network. Imagine an intervention that only affects a certain age group but that requirement (i) to (iii) is fulfilled then we would only identify the social interaction impact from the affected age group on the other age groups and thus in general it would not be possible to generalize the results (see also section 5.1 for the definition of how individuals interact).

Before we discuss how the Gothenburg experiment relates to these requirements, it is of interest to imagine the ideal experiment to test for social interactions in social insurance. The ideal experiment (assuming away the potential problems with randomized experiments) should use networks for which it is impossible for the individuals to change network under the experiment, and it should randomize on two levels. First randomly assign the proportion (covering from zero to one) that should be treated in each network and in the next step inside each group randomly select the treated. Randomization in the first step assures that requirement one and two is fulfilled, and the second step randomization assures that requirement four is fulfilled. Requirement three is obviously also fulfilled.

Such a hypothetical experiment is though to our knowledge non existent. We will though argue that the experiment described in section 2.2 fulfill these four requirements, even though we don’t have randomization at two levels. The experiment is clearly an exogenous change in the behavior of those treated by the experiment. Because the experiment only was conducted in Gothenburg municipality, the immigrants in the Gothenburg MSA have different proportion of treated in their network, depending on were their network members live (cf. Table 1). Gothenburg MSA is a fairly homogenous region, and the boarder of Gothenburg municipality cuts through homogenous areas. Individuals’ network areas will therefore reach over the boarder of Gothenburg municipality. If those with a high proportion of treated in their network change their behavior more compared with those with low proportion, it is a sign of endogenous social interaction.

Since we observe all individuals before and after the experiment was conducted we have the possibility to control for individual (and network) heterogeneity in sickness absence behavior. Thus, unless there are trends in unobserved variables affecting sickness absence that are correlated with the proportion treated in the network, requirement (ii) is fulfilled. Sensitivity analysis will be performed to check this assumption. The same regressions are estimated for 1987 and 1989 when no experiment were conducted. A significant social interaction term is then an indication of a violation of the assumption. Estimations are also performed for the Stockholm region in 1988, estimating a treatment from a hypothetical experiment similar to the one in Gothenburg.

Requirement (iii) is violated if those who have preference for increasing their absence move into areas were many in their ethnic group are treated. The argument for this is that since people are affected by the reform absence is more acceptable in areas with more treated.

We believe that the problem with endogenous moves is not a problem in this study given the short experiment period of only 6 months.

Gothenburg municipality include dissimilar areas, with for example both high and low income areas. We therefore argue that the composition of those born on a even date and living in Gothenburg municipality in each network is highly representative for the whole network, fulfilling requirement (iv).

4 Data

The empirical analysis is based on the IFAU database to which we have matched sickness absence data from the National Security Board (NSB). The IFAU database is a population register and includes a large set of socioeconomic variables (e.g. age, sex, incomes, immigration status and employment status). It includes information on country of birth, which we use to define ethnic groups as those with the same country of birth. The sickness absence data covers all absence periods for which sickness benefits are paid. Because the NSB at the time of the experiment where responsible for paying benefits from day one, it is a complete register of all absence due to sickness.

4.1 Sample selection

We restrict the population to the employed immigrants living in the Gothenburg MSA in 1988. The MSA is a fairly homogenous area, defined by Statistics Sweden, including 13 municipalities[5] in the area around Gothenburg municipality. The MSA had in 1988 a total population of 428,730 between 20 to 60 years of age, and of those were 59,152 immigrants. The municipalities are of different size, from the smallest, Öckerö with 5,487 inhabitants between 20 to 60 years of age to the largest, Gothenburg with 242,447. We use the employment register included in the IFAU-database to identify working individuals. An individual is included in the analysis if they work at least 5 months in the first respectively second half of 1988 and is between 20 to 60 years old, excluding self employed, farmers and seamen.

4.2 A first look at the data

Our data set include immigrants from 84 countries, who had more than 10 network members in Gothenburg MSA in 1988, from all parts of the world. The countries with the largest immigrant group is from Finland, other large immigrants groups are the other Nordic

[5] The municipalities are Ale, Alingsås, Gothenburg, Härryda, Kungsbacka, Kungälv, Lerum, Lilla Edet, Mölndal, Partille, Stenungsund, Tjörn and Öckerö.

countries, Hungary, Former Yugoslavia, Poland, Germany, Iran, Estonia, Turkey and Chile. Table 1 present some descriptive statistics for the major ethnic groups in our sample of working immigrants living in the Gothenburg region. From this table we can see that there is considerable variation in the size of also the major groups (53 - 10,755 individuals).

There is also a considerably large variation in the proportion of treated (23 – 52 percent). The major variation in this variable comes from were the immigrant groups live in the Gothenburg MSA. Turning to our outcome, sickness absence, we can also see a large variation in the number of absence days in spells shorter than 15 days (2.7 – 9.1percent). This definition of sickness absence, denoted short term sickness absence, is used in a first step analysis of social interactions. Descriptive statistics of other definitions of sickness absence, that is analyzed in section 6, is given in Tables 2 and 3. From these tables we can, primarily, see an increase in the number of spells over the period.

Box plots of the mean short-term sickness absence among the immigrants over the period 1987:1-1989:2 is displayed in Figure 4. We can see that there is considerably variation in the sickness absence rates and that there is no general trend. However, a clear increase in the median (mean) sickness absence in the second half of 1988 is visible. When examining box plots for the treated and non treated populations separately, one can clearly see that the increase in sickness absence is mainly for the treated population, however a small "effect" for the non-treated is potentially discernable (see Figure 5), especially considering that for 1987 and 1989 the absence is lower during the autumns.

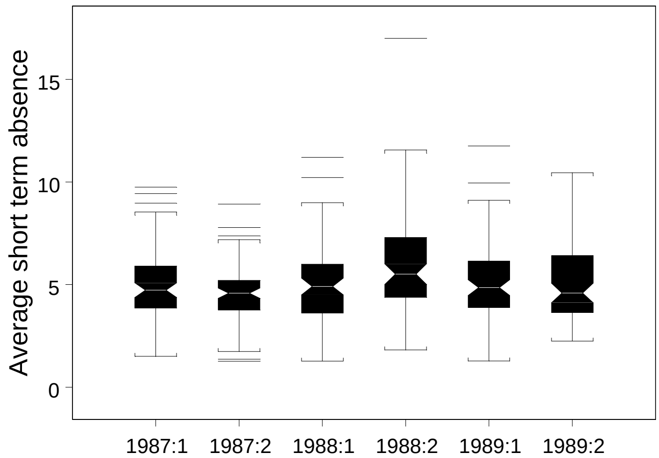

Figure 4: The distribution of the average short term sickness absence among the 84 immigrant groups in the Gothenburg MSA over the three years period, 1988 to 1989.

Figure 5: The distribution of the average short term sickness absence among the 84 immigrant groups in the Gothenburg MSA over the three years period, 1988 to 1989, for the individuals who where not assigned to treatment in the second half of 1988.

If the intervention increase sickness absence among the non-treated individuals, this may stem from social interactions. In addition if the effects are larger in groups with many treated than in group with few treated this is a first indication of evidence of social interaction. This will be studied in the next sub section.

4.2.1 Short term sickness absence

To more formally test for the mean effects that were potentially seen in Figure 5 we estimate separate - for the non-treated and treated - linear regression models with the half year difference in short term sickness absence as the dependent variable and yearly dummy variables as independent variables (using 1987 as the reference year). The results from these estimations are given in Table 4. From this table we can see a large increase in the sickness absence during the second half of 1988 also for the non-treated. For the treated, the increase in short term absence is almost 2.5 day as compared with 1987. For the non-treated, the increase in comparison with 1987 is by more than one day.

The results for the non-treated could be from confounding factors (e.g. a flue epidemic in the autumn 1988 not prevalent the other years) and not from social interactions.[6] In an attempt to test for an effect we subdivide our sample into quartiles of the fraction treated in their network. Detailed descriptive statistics of the difference in sickness absence in 1988 (the

[6] Under the assumption that this last estimate is not an effect of the reform, the effect of the extended waiting period was to increase the short term absence by about 1.5 days.

second half sickness absence subtracted with the first half sickness absence) is given in Table 5. What is most striking is the large heterogeneity and that there is a positive correlation between the fraction treated and the difference in sickness absence. A similar pattern can be seen for the treated and non-treated: on average the individuals are more absent in the second half than in the first, but the average is (non-monotonous) increasing with the fraction.

In Table 6 results from a regression of the difference in short term sickness absence against the quartiles are presented. For the treated there are statistically significant larger “effects” of the reform for the 2nd to 4th quartiles than for the 1st quartile. For the non-tread the “effects” for the 4th quartile is marginally statistically significant larger than then the “effects” for the 1st quartile.

Thus, all in all, the intervention seems to affect the non treated, as well as the treated, individual in a way that its hard to not believe that it is not from social interactions. In the following section we will more formally discuss the identification of endogenous effects in the sickness insurance.

5 Theoretical framework

In this section we present our theoretical framework. The starting point is the regular labor supply model, in which work implies increased monetary income as well as an utility loss in form of lost leisure. We also introduce social interactions through a deterministic social utility. A rational individual will work if the utility from work is larger than the utility from being absent from work. The set up to obtain a self consistent model when social interactions are included builds on the work by Brock and Durlauf (2005).

We assume that the individuals belong to a well defined network *j*, consisting of n_j individuals. Let $d_{ij} = 1$ if an individual *i* in this network is absent from work and $d_{ij} = -1$ if working. Denote the vector of sickness absence for this individual network, excluding the individual self, $\tilde{d}_{-ij}$, thus $\tilde{d}_{-ij} = (d_{1j}, ..., d_{i-1,j}, d_{i+1,j}, ..., d_{n_j,j})$.

The asset values for individual i in network *j* associated with job and sickness absence are

$$V_{ij}^{w} = U(w) + a_j + \varepsilon_{ij} + g(d_{ij} = -1, E_i(\tilde{d}_{-ij})) \qquad (1)$$

and

$$V_{ij}^{S} = U(wb) + g(d_{ij} = 1, E_i(\tilde{d}_{-ij})), \qquad (2)$$

respectively. Here U is the individuals' private utility and $E_i(\tilde{d}_{-ij})$ is individual i:s beliefs about the choice of the network members.[7] If the individual is at work it receives the wage, w, but there is also two utility variables a_j and ε_{ij}. a_j is network j:s value of leisure on average and ε_{ij} is the stochastic daily variations in the individual value of leisure. If the individual is absent from work it receives bw, where b is the replacement ratio. We assume here, for simplicity, that the wage rate and the replacement ratio to be the same for all individuals and exogenously given.

A deterministic social utility $g(d_{ij}, E_i(\tilde{d}_{-ij}))$ is also included in the model. Our basic assumption regarding $g(d_{ij}, E_i(\tilde{d}_{-ij}))$ is that individuals prefer to behave in the same way as those in their network, for example if a individual expect their network members to be absent from work in high degree the social utility cost from being absent is low. In the next section we motivate and define $g(d_{ij}, E_i(\tilde{d}_{-ij}))$ in detail.

Individual i will be absent from work if $V_i^S - V_i^w > 0$ and because each individual have the same wage, replacement ratio, utility function as well as monitoring cost, the cut off value will be the same for all individuals in network j. The stochastic and unobserved component ε_{ij} is assumed to be independent of the individual's beliefs about the network members' choice as well as independent between the individuals in the network. These assumptions are not restrictive considering that a_j is included in the model. The probability that a randomly drawn individual in network j is absent is then equal to

$$\Pr(d_{ij} = 1) = \pi(w, b, E_i(\tilde{d}_{-ij})) = \Pr(\varepsilon_{ij} \le U(wb) - U(w) - \alpha_j + g(1, E_i(\tilde{d}_{-ij}) - g(-1, E_i(\tilde{d}_{-ij})) \qquad (3)$$

In order to obtain some theoretical predictions and to obtain a model that allows us to empirically test for social interactions, we need to make some assumptions on how the interactions are formed and on how the individuals make their predictions.

5.1 Social interactions

There are several reasons to expect a social utility, (1) synchronized leisure, (2) norm effects, and (3) health spillovers. The utility from leisure is expected to be higher if it can be shared with other members in the network. Low absence in the network may imply harsh norms against being absent, introducing a stigma cost. Finally the value from leisure (or the

[7] Thus, we assume that the individuals interactions are formed from expectations about their network members behavior and not from their member actual behavior.

cost of being at work) is affected by the individuals' health, which could be influenced by the network members' health through health spillovers.

To define the social utility we need first to assume something about the functional form of the social utility. We follow Brock and Durlauf (2005) and assume quadratic conformity effects

$$g(d_{ij}, E_i(\tilde{d}_{-ij}) = -E_i\left(\sum_{k\neq i}^{n_j}\frac{J_{ik}}{4}(d_{ij}-d_{kj})^2\right),$$

where E_i is the subjective expectation of individual *i* and J_{ik} represents the weights individual *i* give the interaction between individual *i* and individual *k* in the network. If J_{ik} = 0, then individual *i* disregard the actions of individual *k*. We further assume that all individuals (including the individual him self) have the same weight when forming the expectation, thus $J_{ik} = J$ for all *j*, *k*. We also assume that the individuals have rational expectations (this means that $E_i = E$, for all *i*, where *E* is the mathematical expectation). A specific set of actions by the individuals then constitute an equilibrium if the individuals correctly anticipate the actions by their network members, thus $E(d_{kj}) = \bar{\pi}_j$, where $\bar{\pi}_j$ is the mean absence rate in network *j*.

We now get (see the footnote for a derivation, which builds on Brock and Durlauf (2005)) a closed expression for the social utility parameter[8]

$$g(d_{ij}, E(\tilde{d}_{-ij}) = 0.5J(d_{ij}\bar{\pi}_j - 1) \tag{4}$$

The social utility of being absent from work (at work) is then increasing (decreasing) in the mean absence level, $\bar{\pi}_j$, in network *j*, thus $g_{\bar{\pi}_j}(d_{ij} = 1, E(\tilde{d}_{-ij})) > 0$, where $g_x(.)$ denotes the first order derivative with respect to *x*. In other words the social cost of being absent is proportional to the mean absence level in the network. This gives us a closed expression for equation (3)

[8] The social utility term is, since $d_{ij}{}^2 = d_{kj}{}^2 = 1$, equal to

$$g(d_{ij}, E_i(\tilde{d}_{-ij}) = -E_i\left(\sum_{k\neq i}^{n_j}\frac{J_{ik}}{4}(d_{ij}-d_{kj})^2\right) = -0.5\sum_{k\neq i}^{n_j}J_{ik}(d_{ij}{}^2 - d_{ij}E_i(d_{ij}) + d_{kj}{}^2) = 0.5\sum_{k\neq i}^{n_j}J_{ik}(d_{ij}E_i(d_{ij}) - 1).$$

Further using the assumption if equal weight, $J_{ik} = J$ we get

$$g(d_{ij}, E_i(\tilde{d}_{-ij}) = 0.5J(d_{ij}E_i(d_{ij}) - 1)$$

Then imposing the self-consistency condition leaves us with equation (4).

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$$\Pr(d_{ij}=1)=\pi(b,\overline{\pi}_j)=\Pr(\varepsilon_{ij}\le U(wb)-U(w)-\alpha_j+J\overline{\pi}_j) \tag{5}$$

In order to estimate J we assume ε_{ij} to be complementary log-log distributed. This leaves us with the discrete time Cox proportional hazard model, thus

$$\lambda_i(\tau)=h_{0j}(\tau)\exp(\gamma\overline{\pi}_j(\tau)) \tag{6}$$

Here τ is the duration in work absence and $h_{0j}(\tau)$ is the baseline hazard to leave sickness absence for work in network *j*. The complete set of parameters are not identified. J is only identified up to scale and hence only $\gamma=J/\sigma$, where σ is the standard deviation of the complementary log-log distribution, is identified. The baseline hazard includes, among others, the effect of wages and sickness benefits, as well as the network specific effects α_j.

6. Estimation and Results

In section 3 we argued that it is possible to identify $\gamma=J/\sigma$ using the randomized experiment. In section 4 we saw that the intervention increased the absence among the treated (thus an exogenous change in their absence rate), and that the non-treated also seemed to be affected by the intervention: more if they had more individuals in their network. In this section we will formally test the hypothesis of endogenous social interactions. The basis for estimation of the transition from work absence to work is the Cox model in equation (6).

The mean absence in network *j* can be decomposed into the work absence of the non-treated and treated, respectively

$$\overline{\pi}_j=\frac{n_{0j}}{n_j}\,\overline{\pi}_j(R=0)+\frac{n_{1j}}{n_j}\,\overline{\pi}_j(R=1).$$

Here R = 1 if the individual is directly treated in the monitoring experiment, i.e. have 14 days of own decided sickness absence and n_{0j} and n_{1j} are the number of non-treated and treated, respectively in network *j*. The experiment gives us, given the assumptions described in section 3, exogenous variation in the second term of the equation.

In our data we observe every sickness absence day for the whole three year period 1987 to 1989. Thus we have the possibility to study the effects of social interactions on the prevalence, incidence and duration. The total effects of social interactions is revealed from the prevalence but it is also of interest to study the effects on duration and incidence separately.

Based on Hesselius et al. (2005) our prior is that, if there is an effect, it is mainly from increased duration and not from increased incidence.

In all three specification we use similar strategy for identification. This strategy is a difference in difference (DID) approach. To this end, we split our data into monthly intervals and calculate the monthly prevalence of sickness absence for each immigrant group. This is calculated as the mean number of short term sickness absence days during a month.

The effect of social interaction on the prevalence is then studied by estimating

$$y_{ijm} = \delta_j + \delta_1 \overline{\pi}_{ijm-1} D_{im} + \delta_2 \overline{\pi}_{ijm-1} + month + \varepsilon_{ijm}, m = 1,...12. \quad (7)$$

where y_{ijm} is the number of short term sickness absence days during month *m* for individual *i* in network *j*, D_{im} is a step function that takes the value zero until August and thereafter it takes the value one (i.e. from August to December) and $\overline{\pi}_{jm} = (n_{1j} / n_j)\overline{\pi}_j(m, R = 1)$, where $\overline{\pi}_j(m, R = 1)$ is the fraction of days on short term sickness absence during month *m* for the treated population. Season is controlled for by the monthly factor *month*. Note that $\overline{\pi}_{jm}$ is increasing in the fraction treated in the network and in the level of sickness absence among the treated.

If social interactions are present then the treated are affected by the non treated sickness absence and to avoid this complication we use the monthly lagged absence among the treated. There are some reasons why ε_{ijm} and $\overline{\pi}_{jm-1}$ might be correlated still. For instance if there is an trend in sickness absence in the network (e.g. from a spread in the network of a bad long term disease). Then without exogenous variation in the mean network absence, one cannot take a correlation between individual and mean absence as evidence for social interactions. Thus the before and after time of the social experiment to solve this problem. This identification strategy relays on additive separability of $\overline{\pi}_{jm-1}$ and seasonal effects. Thus, if there are health chocks in the Gothenburg MSA then these chocks are not allowed to be unevenly spread among the immigrant groups and correlated with the fraction treated.

The duration in work absence (see Table 2 and 3 for a description of the durations) is studied by estimating a similar DID Cox regression model:

$$\lambda_i(\tau) = h_{0j}(\tau)\exp\left(\gamma_1 \overline{\pi}_{jm-1}(\tau) D_{im}(\tau) + \gamma_2 \overline{\pi}_{jm-1}(\tau) + month\right), \quad (8)$$

Here we have indexed the covariates with the duration, τ. Thus the covariates changes value if the duration of a work absence spell stretches over months. This model is estimated using the stratified partial maximum likelihood estimator, with an exact method to handle ties (see e.g. Kalbfleisch and Prentice (1980) for one version of the general formula).

The incidence to work absence is studied by once again estimating a DiD linear regression model:

$$I_{ijm} = \alpha_j + \alpha_1 \bar{\pi}_{jm-1} D_{im} + \alpha_2 \bar{\pi}_{jm-1} + month + \varepsilon_{ijm} . \quad (9)$$

Here I_{ijm} is the frequency of work absence spells for individual *i* in network *j* in month *m* (see Tables 2 and 3 for a description of the count data). If individuals who prior to the reform never were absent from work start to be absent from work due to the social interactions then the composition of individuals working and in work absence before and after the reform will differ. At a specific duration in work absence or work the population will be different before and during the reform. Instead of estimating the hazard to work absence the specification based on the frequency of spells circumvents this problem since we use the same population in the post and pre experiment period when estimating (9).

6.2 Results

The result for the prevalence is first presented. The following two sub-section provides the results from the estimation of the Cox regression model and from the incidence to work absence.

The results presented here are the results without any further control variables. In the Appendix (see Table 10 and 11) we provide additional analysis where we (i) control for age, income, type of employment parish and (ii) when excluding $\bar{\pi}_{jm-1}$ from the analysis. The results are qualitatively insensitive to these extensions and restrictions

6.2.1 First step least squares

The estimation of equation (7) is performed using the ordinary lest squares estimator (OLS). The main results are given in the first row and columns 3 and 4. In addition, the results from three sensitivity analyses are provided. First, the same regression equations, as for 1988, are specified for the periods 1987 and 1989, respectively. Thus, we are assuming an artificial direct treatment effect and an artificial social interaction effect, between the first and second half in 1987 and in 1989. Immigrants are considered to be treated if living in Gothenburg municipality 1987 or in 1989 and born on an even date. The same estimators as above are used in the estimation. In a second sensitivity analysis we perform an artificial experiment in the Stockholm MSA. Stockholm is the capital of Sweden and the largest city in Sweden. Individuals living in the Stockholm municipality in 1988 and born on even date are considered as treated after July 1 1987.

From Table 7 and columns 3 and 4 we find statistically significant endogenous effect on the number of short term sickness absence days. If the number of short term sickness absence days increases by one day the number of short term sickness absence days in a month increases with on average by 0.16 days. From table 1 we can see that the average number of short term sickness days is 5, this means that the average number of short term sickness absence days in a month is 5/6. Thus a 10 percent increase in the mean sickness absence (i.e. an increase by 0.08 days) would lead to a further increase from the endogenous effects of about 1.3 percent. The results are quite robust with respect to the definition of the networks (see columns 5 to 8)., however, partly, as a results of the smaller sample sizes the standard errors of the effects are smaller and thus the effects are no longer statistically significant.

With regard to the sensitivity analyses we find no statistical significant effects of $\hat{\gamma}_1$, however for the Stockholm MSA statistical significant, and large estimates, for $\hat{\gamma}_2$ is obtained, for all network sizes. These effects could stem from endogenous effects, however it could also be from other, confounding, factors.

6.2.2 Hazard regression

The main results from the Cox Regressions are given in the first row and columns 3 and 4 in Table 8. In addition, the results from the same sensitivity analyses as was provided in table 7 is provided.

From Table 8 we can see that an increase in the mean sickness absence by one day in a month would decrease the hazard by about 17 percent (a hazard ratio of 0.83). An increase by 10% (0.1 day) would decrease the hazard by about 2%. Assume the hazard is 5%, then would an increase in the level by 10% lead to a decrease in the hazard to 4.9%.[9]

We can see that results are robust with respect to the definition of the networks (see columns 5 to 8: columns 5 and 6 shows the parameter estimates with larger networks only and columns 7 and 8 shows the results when larger networks are excluded). The results from the pre- and post period in Gothenburg MSA are given in rows 2 and 3 and the parameter estimates from the Stockholm MSA in 1988 is shown in row 4. For the Stockholm MSA we find a, at the 10% level, statistical significant effect. However this “effects” is less than ¼ of

[9] Under the assumption that the elasticity with respect to the replacement level is 0.25 (this estimate is from Johansson and Palme (2005) would an increase in the replacement rate by approximately 40% lead such a decrease in hazard rate.

the effect in Gothenburg and would not have been statistical significant if the population size would have been the same as the Gothenburg MSA population.

6.2.3 Incidence

The results from the OLS estimation of equation (9) are given in Table 9. From the Table we cannot find any effects from social interactions on incidence. This results is robust with respect to the definition of the size of the network. Since no significant effect is found we do not provide results from sensitivity analyses.

6.3 Summing up

We found no effect of social interactions on the incidence. This implies that the effect found in the initial analysis on short terms absence stem from the increased durations in work absence. From the first step analysis we found that a 10 percent increase in the mean sickness absence implies would lead to a further increase from the endogenous effects of about 1.3 percent. Form the hazard regression model we found that an increase by 10% would decrease the hazard by about 1.7 percent. Under the assumption of no duration dependence in either the incidence and duration in work absence, the two estimates suggest quite similar effects of social interactions despite the quite substantial duration dependence in work absence (see figure 2).

Under the assumption of a constant hazard we make a back to the envelop calculation and compare the effects of social interaction with effects of economic incentives. We assume that the monthly hazard is constant and 0.20 for the first 14 days (see figure 2) and that the monthly incidence is on average 0.20 (see table 6). Based on these simplifying assumptions we get that the prevalence is one (0.20*(1/0.2)) which is close to the average short-term monthly prevalence in our data that was 5/6.

Now assume that there is a 10 percent increase in the replacement rate for the first days in a sickness absence spell. Under the assumption that the elasticity on the incidence from an decrease in the replacement rate is -.91[10] we get an new incidence of 0.218 instead 0.20 and the new level of sickness absence would now be about 1.09. Thus an increase in the level by 9.1 percent. From the endogenous effect this would lead to a decrease the hazard by 1.55 per cent and then we would finally reach the new equilibrium of about 1.11 days of monthly

[10] Johansson and Palme (2005) estimates the elasticity's to be -0.91 for the males and -.72 for the women on incidence.

absence. Thus the effects of social interactions are quite large in comparison with the effect from economic incentives.

7. Conclusion and discussions

An unique randomized controlled experiment conducted in Gothenburg is used to estimate the effects of social interactions in sickness insurance. Since we have data on all individual sickness absence in Sweden 1987-1989 we have the opportunity to control for individual effects and also to perform extensive sensitivity checks.

Our study adds evidence to the rather small empirical literature investigating social interactions in social insurance. We find evidence of endogenous social interaction effects in the sickness insurance. A 10 percent increase in the means absence would decrease the hazard from work absence to work by about 1.7 percent because of endogenous interactions. This effects is in the same magnitudes as of increasing the replacement level by 1 percent.

We are very confident of the existence of social interaction in the sickness insurance however one may question the size of the endogenous effects. The reform used to identify the effects was run for half a year. Under the presumption that norms are only slowly and gradually forming the effect can be considered as large.

The drawback with our study (an with many others) is that the identification relays on (i) ad hoc specification of the network and that all individuals in this network have the same weight and (ii) that the expectations are formed from rational expectations of the individuals behavior. If assumptions (i) are not correct, then the above estimate is most likely attenuated. Thus the estimates provides a lower bound for the endogenous effects.

We have assumed that the endogenous effects are from changed norms via the expectation of others absence an that these expectations are formed from rational expectations of the individuals behavior. These two assumptions are restrictive. There are other reasons to expect endogenous effects in sickness absence e.g., (1) utility effects (synchronized leisure), and (2) information effects. If the utility from being absent is increased when the network members are absent there is a utility effect. Changed sickness absence in the network group due to health changes, may influence individuals absence through health spillovers. An information effect are also potentially plausible. It may well be the case that immigrants are not aware of the generosity of the Swedish sickness insurance system, then because of the experiment the information about the possibilities to be home without a certificate from a doctor for the first 7 days may influence also those not directly treated.

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Socialförsäkringsutredningen – samtal om socialförsäkring; No 1 – Vad är försäkring?

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Table 1: Descriptive statistics for source countries

Country	Sample size	Number of treated	Prop. of total sample	Prop. treated	Prop. males	Mean age	Mean sickness absence 1988:1 and	1988:2	Difference
Mean	34900	11871	1.0	0.340	0.51	40.7	4.9	5.8	0.9
Finland	10,755	3,407	0.288	0.317	0.45	40.9	5.4	6.6	1.1
Denmark	2,085	571	0.056	0.274	0.52	45.1	4.5	5.1	0.6
Island	214	81	0.006	0.379	0.43	36.1	4.2	5.7	1.5
Norway	2,326	646	0.062	0.278	0.45	43.3	4.3	4.9	0.6
Former Yugoslavia	3,746	1,566	0.100	0.418	0.56	39.8	4.5	5.5	1.0
Poland	1,753	670	0.047	0.382	0.34	39.5	5.3	6.4	1.2
Great Britain	728	232	0.019	0.319	0.58	38.6	3.5	4.3	0.9
West Germany	1,907	537	0.051	0.282	0.48	47.2	3.6	3.9	0.3
Greece	416	163	0.011	0.392	0.65	38.6	3.5	4.7	1.1
Italy	394	139	0.011	0.353	0.73	45.3	3.9	4.8	0.9
Portugal	484	210	0.013	0.434	0.54	36.7	5.2	5.9	0.7
Spain	307	102	0.008	0.332	0.64	42.6	3.7	4.7	1.0
Estonia	543	125	0.015	0.230	0.51	51.8	2.7	3.1	0.4
Latvia	66	21	0.002	0.318	0.50	51.6	3.4	4.4	1.0
Bulgaria	66	29	0.002	0.439	0.58	42.5	4.8	3.2	-1.7
Romania	214	83	0.006	0.388	0.55	39.6	5.3	6.7	1.4
Former USSR	231	62	0.006	0.268	0.45	48.3	4.3	4.1	-0.1
-”- Czechoslovakia	426	124	0.011	0.291	0.51	42.2	3.7	4.5	0.8
Hungary	990	364	0.026	0.368	0.60	45.1	4.7	5.0	0.4
France	190	65	0.005	0.342	0.57	41.0	3.2	3.6	0.4
Netherlands	247	73	0.007	0.296	0.62	43.5	3.4	3.8	0.4
Schwitzerland	71	21	0.002	0.296	0.58	42.9	2.8	3.7	0.9
Austria	302	86	0.008	0.285	0.61	44.3	3.9	3.6	-0.3
USA	490	154	0.013	0.314	0.51	40.6	3.3	4.4	1.1
Chile	548	253	0.015	0.462	0.46	36.7	6.2	7.8	1.6
Argentina	113	50	0.003	0.442	0.44	39.8	4.1	4.6	0.4
Bolivia	158	82	0.004	0.519	0.54	34.2	6.3	8.4	2.1
Brazil	82	30	0.002	0.366	0.33	36.4	5.1	6.7	1.6
Uruguay	209	80	0.006	0.383	0.53	38.6	6.7	7.6	1.0
Ethiopia	161	63	0.004	0.391	0.67	31.7	5.4	6.9	1.5
Lebanon	181	71	0.005	0.392	0.83	30.8	9.1	10.8	1.6
Morocco	146	51	0.004	0.349	0.73	37.3	6.7	7.0	0.3
Syria	55	22	0.001	0.400	0.60	34.7	5.7	8.9	3.2
Tunisia	62	22	0.002	0.355	0.87	36.7	6.9	7.3	0.5
Gambia	53	19	0.001	0.358	0.81	36.8	8.4	7.9	-0.5
Uganda	80	34	0.002	0.425	0.61	33.8	6.1	9.4	3.2
Iran	922	386	0.025	0.419	0.77	30.5	5.5	7.3	1.8
Irak	214	49	0.006	0.229	0.82	32.8	9.3	9.5	0.2
Turkey	820	319	0.022	0.389	0.55	32.8	5.6	6.4	0.8
Japan	71	24	0.002	0.338	0.28	42.2	2.6	3.1	0.6
China	171	74	0.005	0.433	0.59	41.8	2.8	3.9	1.2
South Korea	98	27	0.003	0.276	0.15	24.1	3.4	3.9	0.5
the Phillipines	142	61	0.004	0.430	0.27	34.8	6.6	7.5	0.9
Thailand	121	33	0.003	0.273	0.10	33.4	5.6	7.7	2.1
India	211	66	0.006	0.313	0.58	37.2	4.5	5.7	1.2
Pakistan	97	41	0.003	0.423	0.69	36.5	4.7	5.8	1.1

Table 2. Descriptive statistics for work absence spells for 1988 sample, and the samples used in the sensitivity analysis.

	1988 n_j>10	1988 n_j>30	1988 10> <100	1987	1989	1988 Stockholm
No. spells	64 194	63 327	22 204	49 635	67 624	285 167
Of which censored %	6.4	6.4	5.9	8.3	5.3	5.0
Mean duration (days)	10.3	10.3	9.7	12.0	10.1	8.74
% spells ending in						
0-1 week	74.91	74.93	76.11	70.26	76.59	78.90
1-2 weeks	7.79	7.78	7.55	8.65	7.03	7.15
2-4 weeks	7.58	7.47	7.34	8.97	6.94	5.79
4- weeks	7.52	7.54	6.62	9.33	7.57	5.75

Table 3. Descriptive statistics incidence into work absence spells for 1988 sample, and the samples used in the sensitivity analysis.

	1988 n_j>10	1988 n_j>30	1988 10> <1000	1987	1989	1988 Stockholm
Sample Size	24 279	24 000	8 355	23 489	25 575	90 594
No. spells started	64 194	63 327	22 204	49 635	67 624	285 167
Mean incidence in						
January	0.19	0.19	0.19	0.23	0.24	0.24
February	0.24	0.24	0.23	0.21	0.24	0.29
Mars	0.24	0.24	0.25	0.24	0.23	0.29
April	0.22	0.22	0.23	0.18	0.23	0.25
May	0.21	0.21	0.20	0.18	0.22	0.24
June	0.20	0.20	0.20	0.17	0.19	0.22
July	0.12	0.12	0.13	0.11	0.14	0.15
August	0.22	0.22	0.22	0.18	0.22	0.25
September	0.24	0.24	0.24	0.20	0.23	0.27
October	0.23	0.23	0.24	0.21	0.26	0.28
November	0.25	0.25	0.25	0.22	0.25	0.30
December	0.28	0.28	0.28	0.18	0.19	0.36

Table 4: Parameter estimates (OLS) when regressing half year differences in sickness absence on yearly dummies for 1988 and 1989. Data on short term sickness absence in Gothenburg MSA. Excluding individuals in ethnic groups with 10 or less inhabitants.

	Estimate	Standard error	t-ratio
		Non-treated[1]	
Intercept	-.540	.048	-11.30
1988	1.011	.067	15.07
1989	.412	.066	6.34
		Treated[2]	
Intercept	-.431	.080	-5.41
1988	2.445	.110	21.85
1989	0.300	0.11	2.72

Notes: [1]n = 73 343, [2]n = 32 144

Table 5. Descriptive statistics of the difference in short term sickness absence (Ds = sickness absence 1988:2 – sickness absence 1988:1) subdivided into the four quartiles of proportion treated in the network.

Proportion Tretead	1th quartile 9.1-25.6	2nd quartile 25.6-28.6	3rd quartile 28.6-33	4th quartile 33.4-62
Ds		Non-treated		
Min	-45	-41	-45	-47
1:st quartile	-2	-2	-2	-2
Mean	0.33	0.52	0.41	0.56
Median	0	0	0	0
3rd quartile	3	4	3	4
MAX	56	42	31	43
St. dev.	6.67	7.58	7.16	7.59
N	6,126	8,920	1,985	5,171
		Treated		
Min	-55	-35	-34	-41
1:st quartile	-2	-1	-1	-1
Mean	0.91	2.5	1.7	2.1
Median	0	0	0	0
3rd quartile	3	7	5	6.0
MAX	41	54	42	57
St. dev	7.55	9.5	8.0	9.3
N	1,949	3,578	1,579	2,375

Table 6. Parameter estimates from an OLS regression of the difference in short term sickness absence against the four quartiles of proportion treated in the network.

	Non-treated		Treated	
	Estimate	Standard error	Estimate	Standard error
Intercept	0.333	***0.093	0.91	***0.20
2nd quartile	0.183	0.121	1.63	***0.25
3rd quartile	0.072	0.189	0.81	***0.30
4th quartile	0.232	*0.138	1.22	***0.27
R2	0.016 %		0.48%	

Notes: *** significant at the 1% level, * significant at the 10 % level

Table 7: Parameter estimates (estimates and standard errors (Std Error)) from the OLS with short term absence as the dependent variable. (excluding individuals from ethnic groups with less than 10 members)

Network Size		n_j > 10		n_j >30		10< n_j <1000	
Time period and Area	Parameter	Estimate	Std Error	Estimate	Std Error	Estimate	Std Error
1988	γ_1	0.161	**0.079	0.114	0.085	0.110	0.091
	γ_2	-0.053	0.085	0.003	0.099	-0.030	0.090
1987	γ_1	0.013	0.092	0.008	0.109	0.034	0.110

	γ_2	-0.048	0.085	-0.008	0.108	-0.070	0.090
1989	γ_1	-0.025	0.078	-0.024	0.082	-0.068	0.095
	γ_2	0.044	0.079	0.128	0.088	-0.050	0.086
Stockholm MSA	γ_1	-0.021	0.072	-0.031	0.075	0.015	0.127
	γ_2	0.551	***0.090	0.716	***0.105	0.292	***0.117

Notes: Robust standard errors. ** and *** denotes significantly different from zero at the 5 and 1 percent level.

Table 8: Parameter estimates (estimates and standard errors (Std Error)) from the stratified partial maximum likelihood estimator (using the exact method to handle ties) for 1988. The estimations made 1987, 1989 and Stockholm MSA 1988 are provided as sensitivity analyses.

Network Size		n_j > 10		n_j >30		10< n_j <1000	
Time period and Area	Parameter	Estimate	Std Error	Estimate	Std Error	Estimate	Std Error
1988	γ_1	-0.186	**0.072	-0.192	**0.084	-0.174	**0.084
	γ_2	0.102	0.078	0.054	0.096	0.108	0.084
1987	γ_1	-0.042	0.108	-0.022	0.120	-0.035	0.126
	γ_2	-0.114	0.084	-0.204	0.102	-0.015	0.090
1989	γ_1	0.060	0.072	0.051	0.078	0.021	0.090
	γ_2	0.036	0.078	-0.001	0.084	-0.006	0.084
Stockholm MSA	γ_1	-0.042	*0.024	-0.042	* 0.024	-0.066	* 0.042
	γ_2	0.042	0.030	0.030	0.036	0.030	0.036

Notes: *, ** and *** denotes significantly different from zero at the 10, 5 and 1 percent level.

Table 9: Parameter estimates (estimates and standard errors (Std Error)) from the OLS with incidence as the dependent variable.

Network Size		n_j > 10		n_j >30		10< n_j <1000	
Time period and Area	Parameter	Estimate	Std Error	Estimate	Std Error	Estimate	Std Error
1988	γ_1	0.022	0.016	0.023	0.018	0.017	0.018
	γ_2	0.012	0.017	0.014	0.020	0.010	0.019

Notes: *, ** and *** denotes significantly different from zero at the 10, 5 and 1 percent level.

Table 10: Parameter estimates from the stratified partial maximum likelihood estimator (using the exact method to handle ties). Sensitivity analysis not controlling for general trends.

Network Size		n_j > 10		n_j >30		10< n_j <1000	
Time period and Area	Parameter	Estimate	Std Error	Estimate	Std Error	Estimate	Std Error
1988	γ_1	-0.120	**0.053	-0.156	***0.058	-0.102	*0,060

Note: *** significant at the 1% level and * significant at the 10% level.

Table 11: Parameter estimates from the stratified partial maximum likelihood estimator (using the exact method to handle ties). Sensitivity analysis control for gender, age, age square, government employed, income and parish.

Network Size		$n_j > 10$	
Time period and Area	Parameter	Estimate	Std Error
1988	γ_1	-0.258	***0.072
	γ_2	0.156	**0.078

Note: *** significant at the 1% level and * significant at the 10% level.