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Monitoring and norms in sickness insurance: empirical evidence from a natural experiment

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Monitoring and norms in sickness insurance: empirical evidence from a natural experiment[*]

by

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Abstract

We test if social work norms are important for work absence due to self-perceived sickness. To this end, we use a randomized social experiment designed to estimate the effect of monitoring on work absence. The treated were exposed to less monitoring of their eligibility to use sickness insurance, which increased their non-monitored work absence. Based on a difference in differences analysis, we find that the not directly treated also increased their absence as a result of the experiment. By using an instrumental variables estimator, we find significant endogenous social interaction effects. A 10 per cent exogenous shock in work absence would lead to an immediate 5.7 per cent decrease in the hazard out of sickness absence: the long-run effect is calculated as a 13.3 per cent decrease in the corresponding hazard.

Keywords: Work norms, social insurance, social interactions, sickness absence.
JEL-codes: Z13, J22, C14, C23, C41.

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1 Introduction

> "Illness is not merely a state of the organism and/or personality, but comes to be an institutionalized role" Parsons (1978, p. 21)

Institutions and norms have for a long time been considered as an important factor for individuals' perception of sickness or health in medical sociology, which this quote of Talcott Parsons, the father of medical sociology, illustrates. It is hence reasonable to believe that norms are also important in determining the usage of any health or sickness insurance.

In this paper we study if and how the thresholds for individuals' self-perceived health are affected by others' usage of sickness insurance. We do this by using a randomised social experiment. The experiment was conducted in the Gothenburg municipality during the second half of 1988. In this experiment, half of the employees (born on an even day) in the Gothenburg municipality were exposed to less control of their eligibility to use sickness insurance.[1] The direct treatment was studied in Hesselius, Johansson & Larsson (2005) and the result was that relaxed monitoring substantially increased the duration of short-term work absence.

Here, we perform two different analyses: a difference in differences (DID) analysis, and a more structural instrumental variables analysis. In the DID analysis, the change in work absence between the first and second half of 1988 (i.e. before and during the experiment) between four groups is compared. Group one consists of the directly treated within Gothenburg municipality, group two of the non-directly treated within Gothenburg municipality, group three of individuals in the Gothenburg metropolitan statistical area (MSA) outside of Gothenburg municipality, and group four consists of individuals living in municipalities bordering Gothenburg MSA.[2] If there are no social interactions, we expect no difference in the half-year change between groups two, three and four.

[1] The treated were allowed to wait until day 15 (instead of day 8) of each work absence period before submitting a doctor's certificate.
[2] Gothenburg MSA and bordering municipalities are described more in Section 3.

The results of this analysis confirm the social interactions hypothesis: in comparison with group four, the work absence for the second half-year in 1988 increases more for group one but it also increases more for groups two and three. Furthermore, the work absence for group two increases more than for group three. Hence, those who live closer to the directly treated are more affected than those living further away. This pattern is expected if endogenous social interactions are present.

In order to test for endogenous effects on work absence, a theoretical model is specified. In this model, individuals' work absence depends on their network's work absence. The reflection problem (i.e. that the individual also affects the network work absence) is solved by using the intervention created by the social experiment as an instrument for network work absence. If the work absence depends on endogenous social interactions, then work absence among the non-treated would, *ceteris paribus*, increase more in networks with more treated than in networks with less treated.

In contrast to the DID analysis, the instrumental variables require a definition of networks. We base our definition of a network on previous work by Borjas (1992, 1995), who shows that ethnic capital is important. We thus define networks as immigrants with the same country of origin. The many ethnic associations and religious meeting places in Gothenburg, and in Sweden in general, provide good opportunities for individuals of the same ethnic origin to share information that would create common norms on e.g. the use of the Swedish social insurance system. This definition has advantages since it is not possible to change the country of origin and hence to sort into new "networks".

The main result from the structural analysis is that we find a large and statistically significant endogenous social interaction effect on the pre-monitoring (7 days) duration of work absence. A 10 per cent increase in the mean absence within the network would lead to a further immediate decrease in the hazard out of work absence by about 5.7 per cent on average. The long-run (equilibrium) effect on the hazard is estimated as 13.3 per cent on average.

The rest of the paper has the following organization. The Swedish sickness insurance system and the randomized controlled experiment conducted in Gothenburg are explained in *Section 2*. *Section 3* describes the data and provides results from the DID estimations. The theoretical framework for the instrumental variables estimation is outlined in *Section 4* and *Section 5* presents the empirical identification strategy. The empirical results for the instrumental variables estimator are presented in *Section 6*. Finally, *Section 7* concludes.

2 Institutions 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 work tasks.

Sickness benefits from the public insurance are and have been, in an international comparison, generous: in 1988 most workers received 90 per cent of their lost income from the first day. A benefit cap excluded workers at the very top of the income distribution from receiving the full 90 per cent. Most Swedish workers were, however, also covered by negotiated sickness insurance programmes regulated in agreements between the labour unions and the employers’ confederations. In general, these insurances replaced about 10 per-cent of the forgone earnings, which yielded that the actual replacement rate was 100 per cent for many workers.

The public insurance has no limit to how often or how long benefits are paid. Many sick spells continue for more than a year and there are examples of even longer durations. These spells end mostly in disability insurance, early retirement or in old age retirement.

The public insurance does not control claimants’ eligibility during the first benefit week. At the start of a sick spell, the worker has to call the public social insurance office (and her employer) to report sickness. Within a week, at the latest on the eighth day of sickness, the claimant should verify eligibility by showing a doctor’s certificate that proves reduced work 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.

Some exceptive rules make it possible for the public insurance offices to monitor more (or less) strictly. In a case which they suspect abuse, they can visit the claimant at home. Claimants who have been on sickness benefits ten times or more during the past year may be asked to show a doctor's 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 return '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 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]

The purpose of the experiment was to see whether and how work absence was changed when monitoring of the insurance claimants was 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. Everyone in Gothenburg municipality was exposed to the experiment, except central government employees.[5]

The experiment was a non-blind experiment in that all were informed about it in advance or at the 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 centres were informed in advance about the set-up of the experiment. Mass media were also an important channel to inform the insured.

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[3] The same experiment was conducted in Jämtland, a large county in the sparsely populated northern part of Sweden. There are few immigrants in the area; we therefore only use the experiment in Gothenburg in our empirical analysis.

[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.

[5] Government employees were exempted as they, by law, received their sick leave compensation from the employer instead of from the social security office. The employer, in turn, received the benefit from the social security office.

The direct effects of the experiment have been evaluated by Hesselius et al. (2005). 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 was well conducted. The result in Hesselius et al. (2005) is that the treated increase their duration of absence by 0.6 days on average. No significant effect on incidence into work absence could be found.

3 Data

Our data is taken from Statistics Sweden's RAMS database, to which we have matched data on work absence from the Swedish Social Insurance Agency (SSIA). The RAMS database is a population register and includes a set of socio-economic variables (e.g. age, sex, income, immigration status and employment status). It also includes information on country of birth, which we use to define ethnic groups. The work absence data covers all absence periods for which sickness benefits are paid. Because the SSIA at the time of the experiment replaced forgone earnings due to work absence from day one, it is a complete register of all absence due to sickness.

3.1 Sample selection

In the analysis, we use two different samples. In the DID estimation, presented in this section, we restrict the population to employed individuals living in the Gothenburg MSA and bordering municipalities in 1988. In the instrumental variables analysis, we only use employed immigrants. The immigrant data set includes immigrants from 83 countries, who had more than 10 network members in Gothenburg MSA in 1988. Information on the immigrant groups are found in *Table A 1* in Appendix A. The largest immigrant group is from Finland; other large immigrants groups are immigrants from the other Nordic countries, Hungary, former Yugoslavia[6], Poland, Germany, Iran, Estonia, Turkey and Chile.

[6] It should be noted that almost all immigrants from the former Yugoslavia were from Serbia (Magnusson, 1989). Therefore, no ethnic conflict should exist within this immigrant network. In addition, as sensitivity analysis we removed the former Yugoslavia from the analysis, it did not change our results.

The Gothenburg MSA is a homogenous local labour market, defined by Statistics Sweden, including 13 municipalities[7] in the area around Gothenburg municipality. In 1988, the MSA had a total population of 428,730 individuals between 20 and 60 years of age, and 59,152 of those were immigrants. The municipalities are of different sizes, from the smallest, Öckerö, with 5,487 individuals between 20 and 60 years of age to the largest, Gothenburg, with 242,447 individuals in the same age span.

We use the employment register (in the RAMS database) to identify working individuals. An individual is included in the analysis if he or she works for at least 5 months in the first and respectively second half of 1988 and is between 20 and 60 years old, excluding the self-employed, farmers and seamen.

3.2 A first look at the data

Only those born on an even date living in Gothenburg municipality were exposed to the randomized experiment presented in *Section 2.2*. If there are social interactions in the work absence, the work absence of the not treated in Gothenburg is also affected by the experiment. In addition, since Gothenburg MSA defines a local labour market, we expect that individuals in the whole Gothenburg MSA are also affected by the experiment. Hence, if the change in work absence between the first and second half of 1988, i.e. between before and during the experiment, is higher for non-treated individuals within the Gothenburg MSA as compared with individuals living outside this area, this is evidence of social interactions in work absence. A natural control group for this calculation is formed by individuals living in 27 municipalities bordering Gothenburg MSA[8]. The municipalities used in the estimations are described in *Figure A 1*, in Appendix A.

We have three different “treatment” groups: (1) the directly treated, (2) the non-directly treated living in Gothenburg municipality and (3) individuals living in Gothenburg MSA but not in the Gothenburg municipality. The total effect on group 1 consists

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

[8] Note that this area may also be affected by the experiment; however, for our purpose of identifying social interactions, this does not matter. All that matters is that individuals living in this area should be affected to a lesser extent than non-treated individuals living closer to the Gothenburg municipality.

of the direct effect and the potential social interaction effect, whereas groups 2 and 3 are only exposed to social interactions. Furthermore, we expect the social interactions to be stronger for group 2 compared with group 3 due to the increased distance from group 1 and therefore fewer contacts.

In *Table 1*, the results of the estimations are presented. The results are as expected. The directly treated increase their work absence by 10.7 per cent, the non-directly treated in Gothenburg municipality increase their absence by 4.8 per cent and the individuals in Gothenburg MSA excluding Gothenburg municipality increase their absence by 2.9 per cent, in comparison with the control group. This gives clear evidence of social interactions in the Swedish social insurance. In order to learn more about the social interactions, we develop a theoretical model that is estimated using subgroups of immigrants.

Table 1. The effect of the experiment using DID estimation.

Group	Estimate (%)	Standard error (%)
Gbg municipality – Treated	10.7 ***	0.9
Gbg municipality – Untreated	4.8 ***	0.9
Gbg MSA excluding Gbg municipality	2.9 ***	1.0

Note: Estimation is based on individuals differences (fall – spring) in work absence. The Gothenburg MSA consists of 13 municipalities. Individuals living in 27 municipalities bordering Gothenburg MSA are used as controls. Standard errors are estimated by clustering on municipality. *, ** and *** denote statistically significant results at the 10, 5 and 1 percentage levels.

4 Theoretical framework

The theoretical framework builds on the work by Brock and Durlauf (2001b). The starting point is a regular labour supply model, in which work implies increased monetary income as well as a utility loss in the form of forgone leisure. We also introduce social work norms through a deterministic social utility or a social cost. There is simply a social cost involved when deviating from the work norm. A rational individual will work if the utility from work is larger than the utility from being absent from work.

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. We denote the vector of work absence for this individual network, excluding the individual self, $\tilde{d}_{-ij}$, thus $\tilde{d}_{-ij} = (d_{1j}, \ldots, d_{i-1,j}, d_{i+1,j}, \ldots, d_{n_j,j})$.

The asset values for individual i in network j associated with work and work absence are

$$V_{ij}^{W} = w - \varepsilon_{ij} \tag{1}$$

and

$$V_{ij}^{S} = bw + a_j - g\left(E_i(\tilde{d}_{-ij})\right). \tag{2}$$

Here, $E_i(\tilde{d}_{-ij})$ is individual *i*'s beliefs about the work absence of the network members and $g(.)$ is the deterministic social cost function. If the individual is present at work, she receives the wage, w, but faces a cost ε_{ij}, that is, the effort of working. ε_{ij} is assumed to be a function of individual health shocks.

The value if on sick leave is $bw + a_j$, where b is the replacement rate when absent from work and a_j is the baseline value of leisure for individuals in network *j*. For simplicity, we assume that ε_{ij} is independent of the individual's beliefs about the network members' choices, as well as independent between the individuals in the network conditional on a_j. Our basic assumption regarding the deterministic social cost, $g\left(E_i(\tilde{d}_{-ij})\right)$, is that individuals prefer to behave in the same way as those in their network, that is, the social cost from being absent is low for individuals in high work absence networks.

Individual i will be absent from work if $V_{ij}^{S} - V_{ij}^{W} \geq 0$, that is, using equations (1) and (2), if

$$\varepsilon_{ij} \geq (1-b)w - a_j + g\left(E_i(\tilde{d}_{-ij})\right). \tag{3}$$

In this simplified model, each individual in network *j* is assumed to have the same wage and, since the replacement rate is the same for all individuals, this implies that the cut-off value will be the same for all individuals in the network. The cut-off value is increasing in wages, but decreasing in the replacement rate, network *j*'s value of leisure, and in the social cost of work absence. The probability that a randomly drawn individual in network *j* is absent is then equal to

$$\begin{aligned}\Pr(d_{ij}=1) &= \pi\left(w,b,E_i(\tilde{d}_{-ij})\right) \\ &= \Pr\left(\varepsilon_{ij} \geq (1-b)w - a_j + g\left(E_i(\tilde{d}_{-ij})\right)\right)\end{aligned} \tag{4}$$

In order to test empirically for endogenous social interactions, we need assumptions on: (i) how the interactions are formed, (ii) the networks, (iii) how the individuals make their predictions and (iv) the distribution of ε_{ij}. Assumptions (i)–(iii) are discussed in the next subsection. The distribution assumption of ε_{ij} is given in *Section 5.2*.

4.1 Social work norms

To get a closed form expression for the social cost, we follow Brock and Durlauf (2001b) and assume (i) quadratic conformity effects. That is, the social cost is specified as

$$g\left(E_i(\tilde{d}_{-ij})\right) = E_i\left(\sum_{k\neq i}^{n_j} \frac{J_{ik}}{2}(d_{ij}-d_{kj})^2\right), \tag{5}$$

where J_{ik} represents the weights individual *i* gives the interaction between individual *i* and individual *k* in the same network. If J_{ik} = 0, then individual *i* disregards the actions of individual *k*. We furthermore assume (ii) that all individuals have the same weights when forming the expectation and that the social interaction parameter is constant across the population, thus $J_{ik} = J_i / n_j = J / n_j$ for all *j*, *k*. We also assume (iii) that the individuals have rational expectations (this means that $E_i = E$, for all *i*s, where *E* is the mathematical expectation). A specific set of actions by the individuals then constitutes an equilibrium if the individuals correctly anticipate the actions by their network members, thus $E(d_{ij}) = \pi_j$, where π_j is the mean absence rate in network *j*.

Under these three assumptions, we obtain the following expression for the social cost:

$$g\left(E(\tilde{d}_{-ij})\right) = J\left(1 - d_{ij}\pi_j\right).^{9} \qquad (6)$$

The social cost of being absent is, hence, proportional to the mean absence level in the network. This gives us a closed expression for the probability to be absent (i.e. equation (4)):

$$\begin{aligned}\Pr\left(d_{ij} = 1\right) &= \pi\left(w,b,\pi_j\right) \\ &= \Pr\left(\varepsilon_{ij} \geq J + (1-b)w - a_j - J\pi_j\right).\end{aligned} \qquad (7)$$

Note that the marginal endogenous effect is

$$\frac{\partial \Pr(d_{ij} = 1)}{\partial \pi_j} = J\,\frac{\partial \pi(w,b,\pi_j)}{\partial \pi_j}. \qquad (8)$$

This implies that the marginal effects of an exogenous shift in the mean level, in general, depend on the level of sickness absence.

5 Identification and estimation

In this section, we discuss the empirical identification[10] of our structural model, i.e. of J, and present the model used to test the hypothesis of endogenous social interactions in short-term work absence. Our interest is in the incidence (into work absence) and duration of work absence.

The empirical identification problem of J consists in that: (i) π_j and a_j are dependent and (ii) d_{ij} affects π_j. Since we have data on work absence before the experiment, this enables us to control for the network heterogeneity (i) using fixed effects. The second problem, referred to as the reflection problem in the literature, is solved by esti-

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

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

Further, using $J_{ik} = J / n_j$, we obtain

$$g\left(E_i(\tilde{d}_{-ij})\right) = J(1 - d_{ij}E_i(d_{ij})).$$

Then, imposing the self-consistency condition leaves us with equation (6).

[10] See e.g. Manski (1993, 2000), Brock & Durlauf (2001a, 2001b, 2007), Graham (2005), Graham & Hahn (2005) and Moffitt (2001) regarding the requirements for empirical identification in linear and non-linear models.

mating the effects on the non-treated work absence, where the experiment is used as an instrument. Hence, we estimate our models using 2SLS only on non-treated individuals. The first stage amounts to regressing the network work absence rates on the fraction treated in the non-treated individuals' network.

5.1 The experiment as an instrument

The experiment was conducted in the Gothenburg municipality and not in the Gothenburg MSA; this means that the immigrants will have different fractions of directly treated in their network, depending on where they live in the MSA. Since the central government employees were exempted from the experiment, this, in addition, creates some small variation in the fraction treated between the networks. The results in *Section 3* showed that there is a large direct effect on work absence of the experiment. It is also important that there is a large variation in the proportion of treated, ranging from 14 to 59 per cent (for details see *Table A 1* in Appendix A).

The work absence rate in network *j* should thus be affected by the proportion treated in network *j*, P_j. However, from (8) it is evident (because of the non-linearity) that the effects from P_j should depend on the level of sickness. Taking the work absence before the experiment as a proxy for the level, the first step linear projection is specified as

$$\pi_{jc} = \beta_j + \beta_c + \beta P_j \pi_{jc-1} D_c + \eta_{jc}, c = 1,2, \qquad (9)$$

where c = 1 in the first half-year and c = 2 in the second half-year of 1988. Further, D_c is an indicator variable taking the value of one in the second period. In sensitivity analyses, we also use only the proportion of treated as an instrument, and both these as instruments. Similar results are obtained with these instruments.

There are, though, some threats to this instrumental variable approach. By utilizing data before the reform, we control for general network heterogeneity. It may, though, be the case that there are trends in η_{jc} correlated with our instrument which would violate the exclusion restriction. However, informal tests of the exclusion restriction are performed by estimating placebo effects in *Section 6.1*. This is done by assuming that the same intervention as in 1988 took place in July–December 1987, July–December 1989 and in the Stockholm[11] MSA in July 1988. It means that the same regression equations as for 1988 are specified and estimated. Since there were no experiments in these periods, we expect the estimated "effects" to be zero for these years/regions unless there are trends in unobservables correlated with P_j. These informal placebo estimations suggest that the exclusion restriction is indeed satisfied.

Another concern may be that the networks are incorrectly specified: for example, one could argue that it is not plausible to assume that the immigrants interact with all persons in the ethnic group. It is then important to note that within each specified network there is a probability, P_j, to have a treated network member. We can, basically, think of the treated as randomly chosen (with inclusion probability P_j) within each network. It is worth emphasizing that this feature (of our instrument) provides us with a better situation for identification than if certain groups had been targeted for the experiment (e.g. if old age workers or females had been targeted). Under *randomization* within the network (inclusion probability P_j), then the social interaction effect is identified if: (1) the true network is a (*s*)ubgroup of the specified network and (2) the level of work absence is the same for these subgroups (i.e. $\pi_{js} = \pi_j$ for all *s*).

Assumptions (1) and (2) imply that the marginal endogenous effects on individual work absence are the same for all subgroups (see equation (8)). Under randomization within specified networks each non-treated individual will in expectation have the same fraction of treated individuals in his or her subgroup and this enables identification of the (common) endogenous treatment effect even though we mis-specified the true network of the individuals.

5.2 Modelling duration and incidence

When studying norm effects on duration of work absence and incidence into work absence, then potential duration dependence needs to be addressed. Such correlation is highly likely considering that disutility of work is mainly driven by variation in health, which is arguably correlated over time.

[11] Stockholm is the capital of Sweden and the largest city in Sweden.

The individual starts an absence spell as a result of the health shock $\varepsilon_{ij,t=1}$, i.e. the health shock of the first day of absence. In order to allow for duration dependency during work absence, we specify the unobserved (health) shock at day *t* in a work absence spell as: $\varepsilon_{ij,t} = \varepsilon_{ij,t=1} + \delta_t \ \ (t = 2,\ldots,7)$, where δ_t are parameters that allow for (deterministic) health changes over the duration. We continue to assume that the $\varepsilon_{ij,t=1}$ are independent across individuals.

The specification of $\varepsilon_{ij,t}$ implies that the previous day's attendance or absence contains information about the absence decision (*t*)oday, and we obtain the hazard out of a work absence spell as

$$\Pr(d_{ij,t} = -1 \mid d_{ij,t-1} = 1) = \Pr(\varepsilon_{ij,t=1} + \delta_t < (1-b)w - a_j - J\pi_j). \tag{10}$$

If we, furthermore, assume $\varepsilon_{ij,t=1}$ to be independent and logistically distributed, we obtain the logit hazard regression model:

$$\Pr(d_{ij,t} = -1 \mid d_{ij,t-1} = 1) = (1 + \exp(-(\alpha_j - \gamma_1\pi_j - \delta^*{}_t)))^{-1}, \tag{11}$$

where $\alpha_j \simeq (\overline{w}_j - a_j)/\sigma$, $\delta_t^* \simeq \delta_t/\sigma$, $\overline{w}_j$ is the average income replacement from work absence in network *j* and σ is the standard deviation of the logistic distribution. In this model, the complete set of parameters is not identified. *J* is only identified up to scale and hence only $\gamma_1 = J/\sigma$ is identified.

By aggregating the hazards over the individuals in each network, we obtain

$$\ln\left(\frac{h_{jc}(t)}{1-h_{jc}(t)}\right) = \alpha_j + \alpha_c + \delta_t^* + \gamma_1\pi_{jc}, c = 1,2, j = 1,\ldots,N, \tag{12}$$

where $h_{jc}(t)$ is the population average hazard rate out of work absence at day *t* in network *j* in half-year *c* and *N* is the number of networks. Thus, α_c reflects common time trends.

The aggregated incidence of work absence can be formulated in a similar fashion. Here, we (for good reasons) neglect the duration dependence (i.e. $\delta_t = 0$ for all *t*), which leaves us with:

$$\ln\left(\frac{p_{jc}}{1-p_{jc}}\right) = \alpha_j + \alpha_c + \alpha_1\pi_{jc}, \tag{13}$$

where p_{jc} is the fraction of the individuals in network *j* in calendar time *c* entering work absence due to sickness each day.

5.3 Estimation

The population hazard rates and incidence, specified in (12) and (13), are estimated from the networks' mean hazard rates and the incidence in each network. In order to separate fully norm effects from the direct monitoring effect, we focus on non-monitored sickness absence. We therefore censor each work absence spell at day eight, i.e. the day when the non-treated have to submit a doctor's certificate.[12] Hence, for the hazard, we have fourteen outcome values for each network: seven before and seven during the experiment. Since we neglect any duration dependence for the incidence, we only have two observations on incidence for each network: one before and one during the experiment. Descriptive statistics for the duration and incidence data set are displayed in *Table 2* and *Table 3.*

From *Table 2*, we can see that the hazard from work absence is quite constant and around 15 per cent for the first six days in an absence spell. At day seven (when a doctor's certificate is normally required), there is a sharp increase in the mean hazard. This suggests that moral hazard is present in the insurance. There are neither systematic differences with respect to populations (size of networks) and city (Gothenburg/Stockholm) nor any large differences over time within the Gothenburg MSA.

From *Table 2*, we can see that the incidence into work absence is around 0.8 per cent in 1988 for both half-years, and for both Gothenburg and Stockholm in 1987 and 1989 the incidence is somewhat lower for the second half-year. The differences are, however, not statistically significant.

[12] We have also estimated the models with 14, 21 and 28 days before censoring. The parameter estimates change; however, the elasticity estimates are robust to the day of censoring.

Table 2. Average hazard rates from work absence

	1988		1987	1989	1988
	NS >10	*NS* >30	*NS* >10	*NS* >10	Stockholm *NS* >10
Mean hazard					
Day 1	0.16	0.15	0.10	0.17	0.17
Day 2	0.18	0.17	0.13	0.19	0.20
Day 3	0.15	0.15	0.13	0.16	0.17
Day 4	0.15	0.14	0.14	0.14	0.16
Day 5	0.21	0.20	0.14	0.25	0.22
Day 6	0.13	0.13	0.16	0.13	0.15
Day 7	0.32	0.33	0.42	0.36	0.37

Note: *NS* is the network size. Groups with group sizes outside the limit are excluded from the sample.

Table 3. Mean work absence incidence for the 1988 population and the populations used in the sensitivity analyses.

	1988		1987	1989	1988
	NS >10	*NS* >30	*NS* >10	*NS* >10	Stockholm *NS* >10
Mean incidence					
Jan.–June	0.0077	0.0074	0.0067	0.0079	0.0089
July–Dec.	0.0079	0.0078	0.0059	0.0050	0.0093

Note: *NS* is the network size. Groups with group sizes outside the limit are excluded from the sample.

Table 4. First step estimates (WLS) (proportion of treated times lagged the group mean level of sickness as an instrument for mean absence.)

	NS >10		*NS* >30	
	Estimate	Std error	Estimate	Std error
1988	0.500***	0.112	0.492***	0.114
1987	-0.164*	0.086	-0.110	0.090
1989	-0.044	0.157	-0.037	0.163
Stockholm 1988	-0.268	0.315	-0.233	0.334

Note: Includes network fixed effects and a calendar time effect. Robust standard errors. *, ** and *** denote significantly different from zero at the 10, 5 and 1 percentage levels. Group sizes outside the limit are excluded from the sample.

6 Results

Since the same first step is used for both the hazard and incidence, we start with a discussion of the results from estimating equation (9) using a weighted least squares (WLS) estimator with the population size as weights.[13] Thereafter, we turn to the instrumental variables estimations of the hazard rates and incidence.

6.1 First step least squares

π_j is chosen to be the mean short-term absence (until day 15 of each spell) as a percentage. The reason for this is that the experiment only affected spells shorter than 15 days (Hesselius et al., 2005). The parameter estimates from the estimation of equation (9) are given in *Table 4*. The main results are given in the first row. In columns 2 and 3 we present the results when we restrict the network size to be larger than 10 individuals and columns 4 and 5 restrict the analysis to networks larger than 30 individuals. In addition, the results from three sensitivity analyses are provided in rows 2 to 4. In this sensitivity analysis, we assume that the same intervention as in 1988 took place in July–December 1987, July–December 1989 and in the Stockholm MSA in July 1988. It means that the same regression equations as for 1988 are specified and estimated using fictive proportion treated (fictive treatment assignment by day of birth).

We conclude that the work absence is, as expected, positively affected by the instrument and the effect is also statistically significant. This result is insensitive to the network specification. In our sensitivity analyses, we find no positive associations. However, we find one negative and marginally (at the 10 per cent level) statistical significant association (see row 2 for 1987). Based on this sensitivity analysis we believe that the instrument is excludable from the non-treated individuals' outcome equation in the absence of the intervention.

[13] With this aggregated data, it is straightforward to estimate (12) and (13) using 2SLS estimators. In the estimation stage, it is instructive to consider $\alpha_j = \alpha_{j0} + v_{jtc}$, with $E(v_{jtc}) = 0$, i.e. that there is some random variation, both over calendar time and spell duration, in the group term. Following this specification, it is obvious that we can increase the efficiency of the estimation by weighting with the network sizes. In addition, the inference is made robust with respect to heteroscedasticity and correlation between the hazard rates within each network.

6.2 Hazard rates

The results from the instrumental variables (IV) estimation are presented in columns 2 and 3 in *Table 5*, together with the reduced form estimate. The IV estimate is negative and statistically significant at the 5 per cent level. According to the marginal effect displayed in the table, the effect from an exogenous shift in work absence level by one percentage point would lead to an immediate reduction on the hazard from a work absence spell of 0.035 on average. Evaluated at the mean work absence (2.68 per cent) and mean hazard (0.165) this implies an elasticity of -0.57. Hence, an exogenous shift in the mean absence by one per cent would lead to a shift in the hazard rate by 0.57 per cent, on average.

Table 5. Hazard and incidence regressions, reduced forms and IV estimates for 1988. Excluding individuals in ethnic groups with ten members or fewer.

	Hazard		Incidence	
	Estimate	Std error	Estimate	Std error
Reduced	-0.138***	0.046	0.011	0.031
IV	-0.275***	0.103	0.026	0.838
IV marginal	-0.035		0.0002	

Note: Includes network fixed effects and a calendar time effect. Robust standard errors. *, ** and *** denote significantly different from zero at the 10, 5 and 1 percentage levels. Group sizes outside the limit are excluded from the sample.

6.3 Incidence

The results from the reduced form and instrumental variables (IV) estimation of equation (13) are given in *Table 5*. As expected, we find a positive effect on the incidence, however statistically insignificant. One potential explanation is that individuals on sick leave interact mainly with each other.

6.4 Sensitivity analyses

To provide some further evidence about the validity of our results, we have performed extensive sensitivity checks. Here, we present the results in which we elaborate on (i) the specifications of the first step linear projection, (ii) the network definitions and, finally, (iii) the results from reduced form Cox regression models based on individual data.

The results from the different IV estimations are presented in *Table 6*. The estimates in the first row are the results already presented in *Table 5*. The IV estimates for one other network size restriction are presented in row 2. The precision of the estimation is, as expected, lower when we restrict the size of the networks; however, the inference stays the same. In row 4, we present the results when the proportion treated is used as an instrument. We can see that the results are robust with respect to the specifications of the first step.

We also present results in which we estimate equation (12) but the networks consist of immigrants from the same country arriving in Sweden close in time (five-year periods). This means that we form subgroups within the former networks, all with their unique proportion of treated. The results displayed in *Table 7* are very similar to our baseline results.

Table 6. IV estimates using different instruments and for different samples.

	Hazard		Incidence	
	Estimate	Std error	Estimate	Std error
NS >10	-0.275***	0.103	0.026	0.084
NS >30	-0.268**	0.110	0.022	0.089
Prop. treated as instrument	-0.221**	0.091	0.051	0.072

Note: Includes network fixed effects and a calendar time effect. Robust standard errors. *, ** and *** denote significantly different from zero at the 10, 5 and 1 percentage levels. Group sizes outside the limit are excluded from the sample. *NS* is the size of the network.

Table 7. Hazard regressions when networks are defined by ethnic group and time of arrival. Excluding groups with ten members or fewer.

	Estimate	Std error
First step	0.323***	0.022
Reduced	-0.135***	0.052
IV	-0.419**	0.208

Note: Includes network fixed effects and a calendar time effect. Robust standard errors. *, ** and *** denote significantly different from zero at the 10, 5 and 1 percentage levels.

Table 8. Reduced form Cox regressions using individual data.

	Estimate	Std error
No controls	-0.099***	0.037
With controls	-0.101***	0.037

Note: Includes network fixed effects. Robust standard errors. *, ** and *** denote significantly different from zero at the 10, 5 and 1 percentage levels. The set of controls include gender, age, age squared, income, type of employment, parish, type of education and level of education.

In *Table 8*, we present the reduced form effects of our instrument on the hazard from work absence using individual spell data. In the first row, we do not include any control variables, while the second row presents the effects when we control for gender, age, age squared, income, type of employment, parish, type of education and level of education. These estimations are performed with Cox regressions using the exact maximum likelihood (ML) estimator. The results in *Table 8* clearly show that the estimates are the same when control variables are included. In addition the reduced form estimates are close to the reduced form estimates using the aggregated data (see row 1 in *Table 5*)

6.5 Alternative explanaitions

The above results clearly show that individuals' work absence behaviour is affected by the behaviour of the individuals in their network. The theory outlined in Section 4 assumes that the individuals in the network like to behave according to the norm in their network. Under a quadratic loss function, rational expectations and a common weight, we obtained a closed form expression in which the individual work absence depends on the mean work absence in the network. In addition to this explanation of a norm effect, there are, potentially, at least two other reasons to expect a relation between the mean absence in network and individual behaviour: health spillovers and information effects.

Changed work absence in the network group due to health changes may influence individuals' absence through health spillovers. This would create a relation between the mean absence behaviour and individual behaviour as observed in the estimations above. The present intervention, though, decreases the control of an individual's eligibility, which should hardly affect the individual's health on a short-term basis. We therefore disregard health spillovers as an explanation for the observed relation between network absence and individual absence. Health spillovers are, of course, likely in other situations.

Another possibility may be that immigrants are not informed about the rules and the relatively generous replacement rates in the Swedish sickness insurance system. The implementation of the experiment in itself may then have increased the information about the social insurance system which may then also have increased the take-up rates among the non-treated. If this hypothesis is correct, one would observe that the networks with a high proportion of treated would continue to have high absence in 1989 after the experiment had finished. However, from *Table 9*, we cannot see any larger increase in work absence in 1989 for the networks with more treated than for networks with fewer treated. Hence, we cannot find any other explanation than social work norms as the cause for the estimated effect.

Table 9. Hazard regressions when extending the study period into the post-experiment period in 1989, first step and reduced forms.

	88:2	89:1
First step	0.492*** (0.038)	-0.024 (0.037)
Reduced	-0.142*** (0.054)	-0.076 (0.054)

Note: Includes network fixed effects and a calendar time effect. Robust standard errors in parenthesis. *, ** and *** denote significantly different from zero at the 10, 5 and 1 percentage levels.

6.6 Dynamic multiplier

The calculation of the long-run effects of social interactions is made under the simplifying assumptions of a constant incidence and a constant hazard. Then, in steady-state, the prevalence (mean absence) is simply the ratio of the incidence to the hazard rate and, since no significant effect on the incidence was found, we only need to perform the calculations for the hazard of leaving work absence.

Under these assumptions, it is quite easy to show (see *Appendix B* for the derivation) that the long-run elasticity of an exogenous shift in the mean absence (prevalence) is equal to

$$\varpi_{\pi} = \frac{-\varepsilon_{\pi}}{\left(1+\varepsilon_{\pi}\right)}, \tag{14}$$

where ε_{π} is the short-run elasticity on the hazard from an exogenous shift in the prevalence. Using the short-run estimate of ε_{π} = -0.57, the long-run elasticity is estimated as -1.33 per cent.

7 Conclusion and discussion

Our study adds evidence on the importance of social work norms in the usage of social insurance (cf. Aberg, Hedström & Kolm (2003), Clark (2003), Conley & Topa (2002) and Topa (2001) regarding unemployment insurance and Ichino & Maggi (2000) and Lindbeck, Palme & Persson (2007) regarding sickness insurance). In addition to extending this rather short list of studies, we use an exogenous variation in the network insurance usage that previous studies have lacked.

We find evidence of endogenous social interaction effects on short-term (unmonitored) sickness insurance and that a one per cent exogenous increase in mean absence within the network would lead to an immediate decrease in the individual hazard from work absence to work by 0.57 per cent because of endogenous interactions. This effect is large and in the same order as the effect as one per cent decreases in the replacement rate (according to Johansson & Palme, 2005), and more than three times as large as the estimates of endogenous effects found in Ichino & Maggi (2000). They used mobility between workplaces within an Italian bank to identify the effects and found that a one per cent increase in the mean absence would further increase the absence by 0.16 per cent due to social interactions.

In addition to the short-run (direct) endogenous effect, we have also calculated the long-run (or equilibrium) effect as 1.33 per cent, which suggests that norms are very important for unmonitored work absence.

It is difficult to speculate on the causes of this rather large difference in comparison with Ichino & Maggi. However, we can think of three aspects of our study that are important for explaining the difference in point estimates. The first one is that we focus on non-monitored sickness absence, whereas they study sickness absence in general. The second explanation is that the replacement rate was higher in Sweden than in the Italian social insurance system. The monitoring is low both from the provider of the insurance (the government) but also from the employer since the direct cost of an absent worker is not taken by the employer. This explanation is supported by Lindbeck et al. (2007), who also study the effects of norms in Swedish sickness insurance and find large effects: an increase in mean absence by one day would on average lead to a further increase of about 0.6 days.[14] The third explanation is that the identification strategies are different: we use an intervention while Ichino and Maggi use movers.

From a policy perspective, these results are of great interest, primarily because individuals' health is not observed, which suggests problems with moral hazard. Previous research has shown that the problems with moral hazard will be high if the monitoring

[14] Their analysis is based on four different strategies and the results and interpretation differ depending on the strategy. This result is from their two preferred specifications.

is low and/or when the replacement rate is high. Our results add another important factor: social work norms. The health level that motivates an unmonitored work absence is simply to a large extent determined by the norms in the society. If we change the monitoring (from the authorities and insurance companies etc.), then there is a spillover. That is, what matters for the usage is not only the monitoring per se but also what is considered fair usage of public insurance.

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Appendix A

Table A 1 Descriptive statistics for the countries of origin included in the analysis. Absence rates for group sizes smaller than 50 are not presented due to data protection reasons.

	Group size	Prop. treated	Prop. males	Mean age	Mean abs. 88:1	Mean abs. 88:2
Finland	10755	0.29	0.45	40.9	2.9	3.5
Denmark	2085	0.25	0.52	45	2.4	2.7
Iceland	214	0.35	0.43	36.1	2.3	3.0
Norway	2326	0.29	0.45	43.3	2.3	2.6
Former Yugoslavia	3746	0.38	0.58	39.8	2.4	2.9
Poland	1753	0.33	0.34	39.5	2.8	3.4
Ireland	36	0.33	0.34	39.5	-	-
Great Britain	728	0.28	0.58	38.6	1.9	2.3
DDR	38	0.26	0.23	35.2	-	-
West Germany	1907	0.24	0.48	47.2	1.9	2.1
Greece	416	0.33	0.65	38.6	1.9	2.5
Italy	394	0.31	0.73	45.3	2.1	2.6
Portugal	484	0.39	0.54	36.7	2.7	3.1
Spain	307	0.30	0.64	42.6	2.0	2.4
Estonia	543	0.19	0.51	51.8	1.4	1.6
Latvia	66	0.27	0.5	51.6	1.9	2.3
Albania	12	0.17	0.92	45.4	-	-
Bulgaria	66	0.41	0.58	42.5	2.6	1.7
Romania	214	0.36	0.55	39.6	2.8	3.6
Former USSR	231	0.22	0.45	48.3	2.3	2.2
-”- Czechoslovakia	426	0.26	0.51	42.2	1.9	2.4
Hungary	990	0.32	0.6	45.1	2.5	2.7
Belgium	51	0.30	0.51	41.1	2.6	3.0
France	190	0.31	0.57	41	1.7	1.9
Netherlands	247	0.27	0.62	43.5	1.7	2.0
Switzerland	71	0.27	0.58	42.9	1.3	2.0
Austria	302	0.25	0.61	44.3	2.0	2.0
Canada	55	0.22	0.49	36.4	2.2	2.5
USA	490	0.28	0.51	40.6	1.8	2.4
El Salvador	20	0.2	0.6	30.2	-	-
Mexico	25	0.36	0.52	34.1	-	-
Trinidad	18	0.33	0.5	37.5	-	-
Chile	548	0.42	0.46	36.7	3.3	4.1
Argentina	113	0.37	0.44	39.8	2.2	2.4
Bolivia	158	0.44	0.54	34.2	3.3	4.6
Brazil	82	0.29	0.33	36.4	2.6	3.5
Colombia	59	0.32	0.54	36.2	2.0	3.3
Ecuador	11	0.36	0.54	38.1	-	-
Peru	49	0.31	0.45	35.2	-	-
Uruguay	209	0.36	0.53	38.6	3.5	4.1

	Group size	Prop. treated	Prop. males	Mean age	Mean abs. 88:1	Mean abs. 88:2
Venezuela	17	0.24	0.65	37.4	-	-
Ethiopia	161	0.34	0.67	31.7	2.8	3.7
Somalia	28	0.29	0.79	32.1	-	-
Algeria	43	0.28	0.65	39.9	-	-
Cyprus	18	0.39	0.5	36.3	-	-
Egypt	29	0.14	0.62	41.7	-	-
Israel	41	0.2	0.83	37.4	-	-
Jordan	21	0.29	0.67	37.9	-	-
Lebanon	181	0.34	0.83	30.8	4.8	5.5
Morocco	146	0.29	0.73	37.3	3.6	3.8
Palestine	21	0.24	0.91	44.3	-	-
Syria	55	0.4	0.6	34.7	2.9	4.4
Tunisia	62	0.31	0.87	36.7	3.6	4.0
Gambia	53	0.32	0.81	36.8	4.3	4.1
Ghana	35	0.51	0.66	37.9	-	-
Cap Verde	22	0.59	0.23	34	-	-
Kenya	24	0.42	0.54	35.9	-	-
Liberia	15	0.4	0.27	24.3	-	-
Nigeria	22	0.27	0.68	33.3	-	-
South Africa	49	0.31	0.65	39.7	-	-
Tanzania	20	0.45	0.6	35.9	-	-
Uganda	80	0.35	0.61	33.8	3.3	5.0
Iran	922	0.38	0.77	30.5	3.0	3.9
Iraq	214	0.2	0.82	32.8	4.9	5.1
Turkey	820	0.36	0.55	32.8	3.0	3.4
Japan	71	0.28	0.28	42.1	1.4	1.7
China	171	0.37	0.59	41.8	1.5	2.1
Taiwan	22	0.59	0.5	32.7	-	-
Korea	98	0.26	0.15	24.1	1.9	2.1
Philippines	142	0.4	0.27	34.8	3.6	4.0
Indonesia	36	0.42	0.64	42.8	-	-
Malaysia	48	0.48	0.69	37	-	-
Singapore	11	0.36	0.55	36.8	-	-
Thailand	121	0.26	0.1	33.4	3.1	4.0
Vietnam, rep	141	0.45	0.65	31.2	4.2	4.5
Vietnam	45	0.15	0.92	32.8	-	-
Bangladesh	13	0.15	0.92	32.8	-	-
India	211	0.26	0.58	37.2	2.4	3.0
Kampuchea	25	0.36	0.56	30.8	-	-
Pakistan	97	0.36	0.69	36.5	2.6	3.1
Sri Lanka	34	0.44	0.53	37.6	-	-
Australia	59	0.37	0.47	33.5	2.6	2.9
New Zealand	12	0.42	0.58	36.3	-	-

Note: The table includes all countries of origin, i.e. the countries with more than 10 working individuals in the Gothenburg MSA. Mean absence refers to average short-term absence as a percentage (spells of 15 days or shorter) during the first and the second half-years of 1988, respectively.

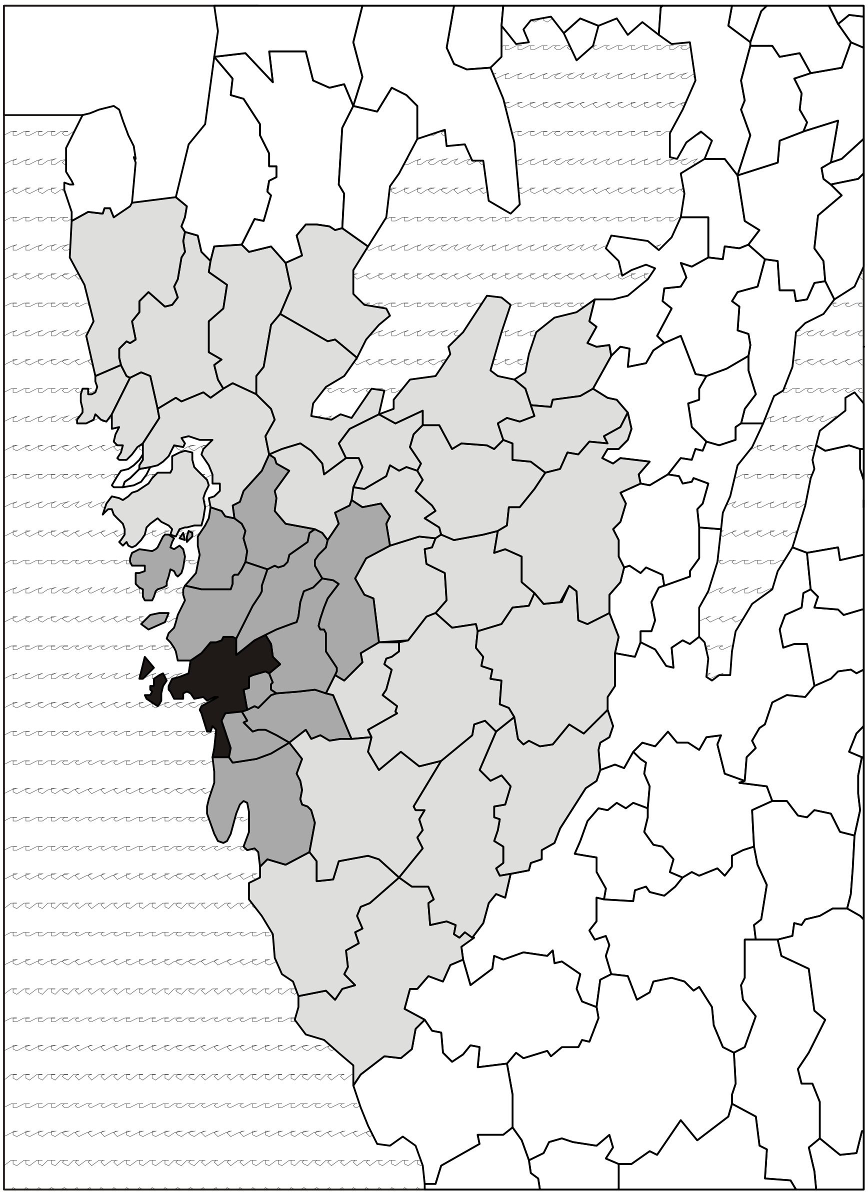

Figure A 1 Map of Gothenburg MSA and the bordering municipalities. The black area is Gothenburg municipality (group 1 for directly treated and group 2 for non-directly treated). The dark grey area shows the other municipalities in Gothenburg MSA (group 3). The light grey area shows the reference group municipalities

Appendix B

The short-run hazard rate h_{t+1} elasticity from social interaction from an exogenous shift in the mean absence from π_t at time period t to π_{t+1} one time period ahead is defined as

$$\varepsilon_\pi = \left(\frac{h_{t+2} - h_{t+1}}{h_{t+1}}\right) / \left(\frac{\pi_{t+1} - \pi_t}{\pi_t}\right). \tag{B1}$$

The initial social interaction effect on the hazard is hence assumed to occur between $t+1$ and $t+2$. The hazard at time period t + 2 can be written as

$$h_{t+2} = \left(1 + \varepsilon_\pi \frac{(\pi_{t+1} - \pi_t)}{\pi_t}\right) h_t \tag{B2}$$

where we use $h_{t+1} = h_t$. In the next time period, the mean $\pi_{t+2} = I_t / h_{t+2}$ increases. Here we use the assumption that the incidence, I_t, is not affected by the social interactions, hence $I_t = I_{t+1} = \ldots$. Now, in t+3, there is a further decrease in the hazard:

$$\begin{aligned} h_{t+3} &= \left(1 + \varepsilon_\pi \frac{(\pi_{t+2} - \pi_{t+1})}{\pi_{t+1}}\right) h_{t+2} \\ &= h_{t+2} - \varepsilon_\pi (h_{t+2} - h_{t+1}) \end{aligned} \tag{B3}$$

The last row is obtained from

$$\frac{\pi_{t+1} - \pi_t}{\pi_t} = \frac{\dfrac{I_t}{h_{t+1}} - \dfrac{I_t}{h_t}}{\dfrac{I_t}{h_t}} = -\left(\frac{h_{t+1} - h_t}{h_{t+1}}\right) \tag{B4}$$

By using recursive substitution, we obtain

$$h_\infty = h_{t+2} + (h_{t+2} - h_{t+1}) \cdot \sum_{k=1}^{\infty} (-\varepsilon_\pi)^k , \tag{B5}$$

and by subtracting each side with h_{t+1} and using that $h_{t+1} = h_t$ we obtain

$$\frac{h_\infty - h_t}{h_t} = \frac{(h_{t+2} - h_{t+1})}{h_{t+1}} \cdot \sum_{k=0}^{\infty} (-\varepsilon_\pi)^k . \tag{B6}$$

The long-run elasticity on the hazard rate of a one per cent exogenous increase in the prevalence is then equal to:

$$\varpi_{\pi} = \frac{\left(h_{\infty} - h_{t}\right) / h_{t}}{\left(\pi_{t+1} - \pi_{t}\right) / \pi_{t}} = \sum_{k=1}^{\infty} (-\varepsilon_{\pi})^{k} . \tag{B7}$$

Under the assumption that $-1 < \varepsilon_{\pi} < 1$, we obtain:

$$\varpi_{\pi} = \frac{-\varepsilon_{\pi}}{\left(1 + \varepsilon_{\pi}\right)} . \tag{B8}$$

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