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ABSTRACT

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Based on administrative panel data from Norway, we examine how social insurance dependency spreads within neighborhoods, families, ethnic minorities, and among former schoolmates. We use a fixed effects methodology that accounts for endogenous group formation, contextual interactions, and time-constant as well as time-varying confounders. We report evidence that social insurance dependency is contagious. The estimated network effects are both quantitatively and statistically significant, and they rise rapidly with "relational closeness" in a way that establishes endogenous social interaction as a central causal mechanism. Social interactions do not cross ethnic borders.

JEL Classification: C31, H55, I38

Keywords: social interaction, social multiplier, work norms, peer effects

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1 Introduction

The purpose of this paper is to examine endogenous social interaction in social insurance (SI) claims. The paper is motivated by two observations. First, there has been a conspicuous – yet basically unexplained – rise in social security dependency in many countries, particularly related to health problems; see, e.g., Duggan and Imberman (2006), Bratsberg *et al.* (2010a), and Burkhauser and Daly (2011). And second, there tend to be correspondingly large and unexplained geographical disparities in dependency rates as well as in attitudes towards social insurance both within and across countries; see McCoy *et al.* (1994), OECD (2010), and Eugster *et al.* (2011). A potential explanation for these empirical patterns is that a person's probability of claiming social insurance benefits depends positively on the claimant rate among peers, implying that social insurance dependency becomes path dependent; see, e.g., Bertrand *et al.* (2000) and Durlauf (2004). A causal relationship of this kind could result from transmission of work norms or changes in the stigma attached to claiming social insurance (Moffitt, 1983; Lindbeck, 1995; Lindbeck *et al.*, 1999; 2003), or it could arise from the transfer of information regarding eligibility rules, application procedures, and acceptance probabilities (Aizer and Currie, 2004), or about job opportunities (Ioannides and Loury, 2004).

While social interaction effects have been extensively analyzed from a theoretical perspective, empirical analysis has been held back by methodological difficulties and lack of appropriate data. The fundamental empirical challenge is to disentangle endogenous interaction from other sources of correlation between individual and group behavior, such as endogenous group formation and unobserved confounders; see Manski (1993). As shown by our brief literature review in the next section, the existing empirical evidence on SI contagion is scant and, with a few important exceptions, limited to ethnic minorities. It is also confined to very specific SI programs (and peer groups), making it difficult to assess external validity and compare the results from different studies. But the few pieces of evidence that are available all point in the same direction: Social insurance claims are causally affected by peer group behavior, implying that there is a social multiplier associated with exogenous changes in SI rolls.

In the present paper, we examine social interaction effects within different kinds of networks – or peer groups – i.e., neighborhoods, schoolmates, families, and persons born in the same (foreign) country. The key research question we ask is whether – and to what extent – an agent's likelihood of claiming *any form of social insurance or welfare assistance* is causally affected by the level of SI claims recorded within the various types of networks the agent relates to, ceteris paribus. We use an extraordinary rich and detailed panel data set from Norway, covering the whole working-age population over age 17. We exploit the richness of the data to set up empirical models in which we control for the various confounding and sorting problems that often undermine the credibility of reported social interaction effects. In contrast to much of the existing literature, we do not rely on either instrumental variables or movements between networks, but instead use individual fixed effects to remove the influence of time-constant confounders and contextual interactions, and flexible time functions (including, e.g., separate year dummy variables for each travel-to-work area and separate age dummy variables for each of 35 different education groups) to control for network-specific shocks and sorting problems that are not eliminated by the individual fixed effects. A novel feature of our empirical approach is that we examine how SI interaction effects vary with *relational distance*, i.e., we are not only interested in effects of peer-group behavior per se, but also in the way the interaction effects vary as we move from "close" to more "distant" network members. While potentially interesting in its own right, we will argue that the interplay between estimated interaction effects and observed relational distance also contributes to ascertaining that the estimates really do reflect social interaction. To fix ideas, assume, for example, that a positive correlation has been established in the timings of social insurance claims within groups consisting of persons who at some point in time went to the same junior high-school. If this pattern reflects a genuine social interaction effect between former schoolmates we would expect the correlation to be larger if we restrict attention to schoolmates belonging to the same class (level) and/or of the same gender. If, on the other hand, the correlation reflects uncontrolled-for school-sorting or local shocks, we would expect the correlation pattern to be similar regardless of whether we use the actual classmates or schoolmates from different classes. With proper control functions, similar arguments can be established regarding the correlation pattern within geographical areas, families, and ethnic minorities – provided that it is possible to construct measures of relational distance that are unlikely to coincide with confounding shocks.

Our findings confirm the empirical relevance of endogenous social interaction. We present several empirical results indicating that individuals' own SI claim propensities are strongly affected by claim patterns among peers, and that the effects grow sharply with relational closeness. With direct reference to the example above, we find that a 1 percentage point increase in the SI dependency rate among junior high-school peers raises the typical person's own dependency rate by approximately 0.19 percentage points, ceteris paribus. But the effect is roughly twice as large for same-level schoolmates as it is for those 1-2 years above or below. It is also much larger for same-sex than for opposite-sex schoolmates. For neighborhood interactions, we find that a 1 percentage point increase in the SI dependency rate among *very close* and similarly aged neighbors raises own dependency by around 0.22 percentage points. The same increase among a matched group of slightly more geographically distant neighbors raises own dependency by 0.10 points. A more detailed analysis shows that similarly aged and similarly educated neighbors have much stronger influence than more dissimilar neighbors, and that same-sex neighbors have stronger influence than opposite-sex neighbors. It also shows that men are more responsive with respect to their neighbors' behavior than women are. This finding is consistent with the observation that the cross-sectional variation in neighborhood SI rates is significantly larger for men than for women. We find particularly strong interaction effects within ethnic networks. A 1 percentage point increase in SI dependency among same-country immigrants within a local area raises own dependency rate by 0.29 points. Social interaction effects do not cross ethnic boundaries, however; a rise in SI dependency among immigrants from *other* low-income countries has no – or even a small negative – effect. Within-family interactions are positive and significant, though the small sizes of family networks imply that their overall impacts on SI dependency are moderate. A one percentage point increase in SI dependency in the extended family (parents, siblings, cousins, aunts, and uncles) raises own SI dependency rate by approximately 0.06 percentage points, but the effects are much larger with respect to close (parents, siblings) than with respect to more distant family members.

2 Related literature

There is by now a large and rapidly expanding empirical literature on social interactions within economics, covering a wide range of topics; see, e.g. Durlauf (2004) or Ioannides and Loury (2004) for recent reviews and Blume *et al.* (2010) for a comprehensive overview of the various identification strategies that have been applied in the literature. The latter paper concludes that the current research frontier still involves efforts to achieve identification in the presence of the three challenges originally highlighted by Manski (1993): i) to differentiate between social interactions that derive from direct interdependences between choices (endogenous interactions) and social interactions that derive from predetermined social factors (contextual interactions), ii) to deal with the presence of group-level unobserved heterogeneity (confounding factors), and iii) to deal with the presence of endogenous formation of the groups that act as carriers of social interactions.

There is also a small empirical literature on peer-effects in the utilization of public transfers. Bertrand *et al.* (2000) examine the role of welfare participation within local networks in the U.S., defined by language spoken. Their empirical strategy is to investigate whether belonging to a language group with high welfare use have larger effects on own welfare use the more a person is surrounded by people speaking one's own language. They find that this is indeed the case, and conclude that networks are important for welfare participation. Aizer and Currie (2004) use a similar approach to study network effects in the utilization of publicly funded prenatal care in California, with groups defined by race/ethnicity and neighborhoods. They conclude that group behavior does affect individual behavior. Furthermore, they show that the identified network effects cannot be explained by information-sharing, since the effects persist even for women who had used the program before. Conley and Topa (2002) examine the spatial patterns of unemployment in Chicago, and find that local variations are consistent with network effects operating along the dimensions of race and geographical and occupational proximity. Hesselius *et al.* (2009) use experimental data from Sweden to examine the extent to which co-workers affect each other's use of sick-pay. The experiment they use implied that a randomly selected group of workers were subject to more liberal rules regarding the need for obtaining a physician's certificate to prove that their absence from work was really caused by sickness. Hesselius *et al.* (2009) show that the reform caused absenteeism to rise both among the treated and the non-treated workers, and that the latter effect was larger the larger was the fraction of treated workers at the workplace. Peer effects in absenteeism are also examined by Ichino and Maggi (2000). Their empirical strategy is to study how workers who move between branches in a large Italian bank adapt to the prevailing absence cultures in the destination branches. The key finding is that workers adjust own absence behavior in response to the absence level among their new colleagues. A similar approach has been used by Bradley *et al.* (2007) to study absenteeism among school teachers in Queensland, Australia. And again, the finding is that the absenteeism of movers to some extent adapts to the prevailing absence culture at their new school. Åslund and Fredriksson (2009) examine peer effects in welfare use among refugees in Sweden, exploiting a refugee placement policy which generates the rarity of exogenous variation in peer group composition. A key finding of the paper is that long-term welfare dependency among refugees is indeed higher the more welfare-dependent the community is in the first place.

Empirical evidence from Norway is provided by Rege *et al.* (2012). They investigate network effects in disability program participation by means of an instrumental variables strategy. Their key idea is that since the probability of disability program entry in Norway has been shown to be strongly affected by job loss (Rege *et al.*. 2009; Bratsberg *et al.*, 2010a), exogenous events of layoff in a person's neighborhood, e.g., caused by firm closure, can be used to instrument the neighbors' disability program participation (with proper controls for local variations in labor demand). Based on this strategy, Rege *et al.* (2012) estimate a sizable network effect implying that a 1 percentage point exogenous increase in similarly aged neighbors' disability program participation rate generates an additional increase of 0.3-0.4 percentage points as a result of network effects.

3 Theoretical Considerations

Social interaction models start from the idea that the preferences of individuals over alternative courses of action depend directly on the actions taken by other individuals to whom the individuals relate; see, e.g., Brock and Durlauf (2000) and Cont and Löwe (2010) for overviews. The purpose of these models is typically to characterize or to provide an explanation for group behavior which emerges from interdependencies between individuals. To illustrate, let a_i indicate individual *i's* use of social insurance, and assume that the payoff function associated with this action can be decomposed into a sum of a private and a social component. Let

a_i^0 denote the optimal choice in the absence of social interaction and let $j \in J$ be the set of agents that *i* relates to. With quadratic utility, we can write

$$U_i\left(a_i;\left\{a_j, j \neq i\right\}\right) = -\pi(a_i^0 - a_i)^2 - \sum_{j \neq i} \gamma_{ij}(a_i - a_j)^2, \tag{1}$$

with the optimal SI claim characterized by

$$a_i^* = \frac{1}{\pi + \sum_{j \neq i} \gamma_{ij}} \left(\pi a_i^0 + \sum_{j \neq i} \gamma_{ij} a_j \right). \tag{2}$$

In this specification, π reflects the marginal disutility of deviating from the private optimum and γ_{ij} measures the marginal gain in *i's* utility of conforming to the action of *j*. Note that it is the *actual behavior* of *j* that *i* conforms to, and not the norms/attitudes that motivate *j's* behavior; hence γ_{ij} represents what Manski (1993) refers to as *endogenous interaction*. While endogenous and contextual interactions both represent important social propagation mechanisms, it may be important from a policy perspective to discriminate between them, since only endogenous interactions are able to create spill-over or multiplier effects of policy interventions targeted at changing actual behavior. Formally, endogenous interactions imply that optimal choices are determined in a large simultaneous equations system, with as many equations as there are individuals.

Different classes of models are obtained from Equation (1) by parameterizing γ_{ij} in different ways. For example, the choice $\gamma_{ij} = \gamma / N$, where *N* is the size of the population (excluding *i*), leads to the global interaction model, where each agent's preferences are affected by the average action of all others, as in Lindbeck *et al.* (1999) and Glaeser *et al.* (2003). By contrast, local interaction models assume that social influences are mediated within confined groups, potentially differentiated by some notion of "distance" such that $\gamma_{ij} = \gamma(d_{ij})$, where

d_{ij} is a measure of relational distance between *i* and *j*. In this setting, the concept of distance may be given a geographical as well as a social interpretation. Studies on the structure of social groups show that individuals tend to interact most with other individuals who are similar to themselves; see, e.g., Marsden (1982). In empirical applications, social interactions are thus typically assumed to take place within peer groups, defined in terms of, e.g., neighborhoods, workplaces, school-classes, families, or races, often in combination with demographic factors (gender, age) and measures of “social distance” (e.g., educational attainment or “class”). But social influences can of course also be mediated without any form of direct interaction, for example if the stigma associated with claiming SI declines with the national rate of SI dependency or with the aggregate rate recorded for persons that are similar to *i*, e.g., in terms of gender, age, and education/class; see Lindbeck *et al.* (1999).

In the present paper, we focus on local interactions; i.e., it is the *idiosyncratic* across-groups variations in social insurance take-up that identify the effects of interest. Endogenous interaction effects are examined at group-levels, and group-averages are used as the central explanatory variables. This implies that the bivariate interaction effects – the direct influence of one person on another – are modeled as homogeneous within (narrowly defined) groups and inversely related to group size; i.e., $\gamma_{ij} = \gamma_g / N_g$, where *g* denotes the group in question and N_g is the number of group members apart from *i*. An important assumption embedded in this framework is that average distance increases with group size, ceteris paribus, such that the larger the number of peers in a particular group, the smaller is the influence exercised by each and one of them. Equation (2) can then be reformulated as

$$a_i^* = \frac{1}{\pi + \sum_g \gamma_g} \left(\pi a_i^0 + \sum_g \gamma_g \overline{a}_{g,-i} \right), , \qquad (3)$$

where γ_g is the utility of conforming to the average behavior in group *g* ($\bar{a}_{g,-i}$). This parameter clearly depends on the weight attributed by individual *i* to the behavior of group *g*, which is again a reflection of its physical or relational closeness, its sameness (similarity), and potentially also its size. The assumption that average distance increases with group size is not always appropriate. For example, in cases where we split a particular group (e.g., schoolmates) into subgroups (e.g., by level), it would be meaningless to assume that a given schoolmate become more influential simply because we have constructed multiple smaller peer groups instead of a single large one. In this case, it would be more natural to normalize by the total number of schoolmates, such that $\gamma_{ij} = \gamma_g / \sum_g N_g$, where *g* now indicates the level to which *i's* schoolmates belonged. This is equivalent to weighting the groups-specific averages by relative group-size in Equation (3).

We typically expect $\gamma_g \geq 0$, but $\gamma_g < 0$ can of course not be ruled out. Negative interaction effects may occur when agents derive utility from displaying novelty, as in fashion and fads, or from signaling a distance to groups one do not wish to be associated with.

4 Institutional Setting and Data

The Norwegian public system of social insurance is comprehensive. In the present paper, we examine all the major social insurance programs relevant for the working age population in Norway; i.e.:

- Unemployment insurance
- Sick-leave benefits (spells exceeding 16 days only)
- Rehabilitation benefits (medical or vocational rehabilitation)
- Disability pension (temporary or permanent)
- Subsidized early retirement (starting at age 62)
- Social assistance

Entitlement to unemployment insurance, sick-leave benefits and subsidized early retirement is obtained through regular employment, whereas rehabilitation benefits, disability pension, and social assistance in principle can be obtained without such experience. The replacement ratios for unemployment insurance, rehabilitation, disability, and subsidized early retirement all typically lie around 60-65 % of previous earnings, but with minimum and maximum levels. For sick-leave, the replacement ratio is 100 %, but these benefits can only be maintained for one year (persons who are still unable to work after one year of sickness will have to apply for rehabilitation or disability benefits). All health related benefits need to be certified by a physician. Social assistance constitutes the last layer of social insurance and is primarily targeted at individuals with no other income sources. In contrast to the other benefits, it is means tested against family income.[1]

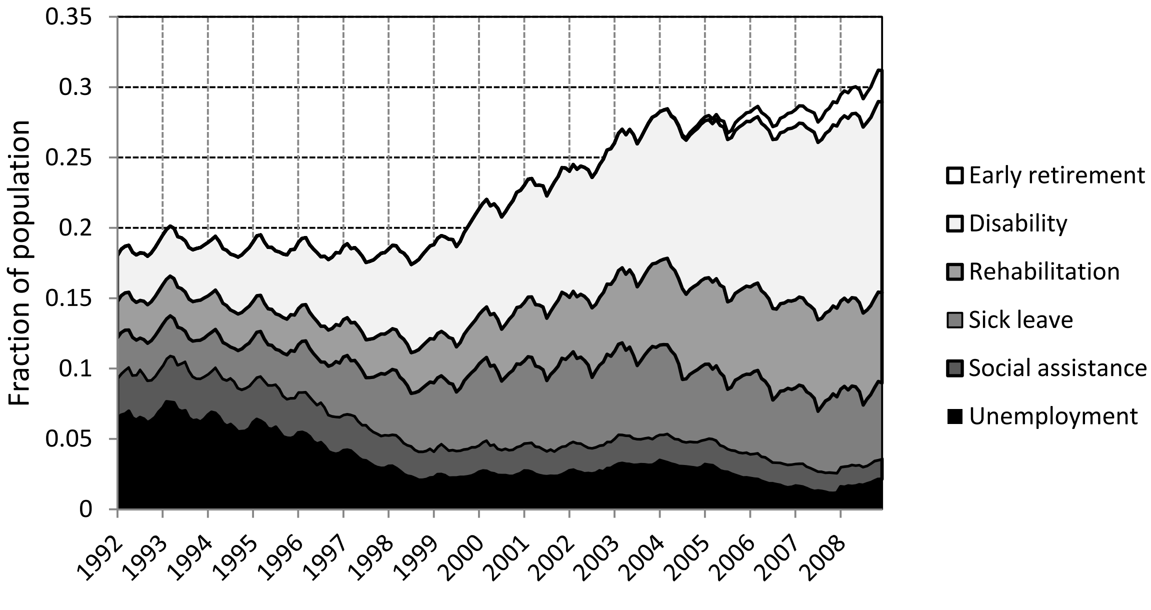

Figure 1. Social insurance claims for the 1942-1974 birth cohorts from 1992.1 to 2008.12
Note: Data include all persons who resided in Norway from 1992 to 2008 and who were born between 1942 and 1974 (1,867,662 individuals).

[1] Due to space considerations, we do not give any detailed description of Norwegian social insurance institutions here. More thorough descriptions (in English) are provided by Halvorsen and Stjernø (2008) and by the European Commission (2011).

Our data cover social insurance claims for the whole Norwegian population from 1992 through 2008. Since we have chosen to use a balanced panel (see next section), we limit the analysis to individuals who were between 18 and 66 years throughout this period, implying that they were born between 1942 and 1974. This implies that our analysis comprises 33 complete birth cohorts, conditioned on being alive and residing in Norway in 1992-2008. Figure 1 gives an overview of these cohorts' social insurance claims – month by month – by SI program. Since we follow the same individuals in this graph, the changes over time are clearly related to ageing as well as calendar time fluctuations. While unemployment insurance and social assistance claims declined significantly during our observation window, the use of health-related social insurance benefits increased sharply. Our primary interest in social insurance exploitation does not lie in the many short-term spells of, e.g., sick pay or unemployment – which to a large extent are dominated by seasonal fluctuations – but rather in longer-term SI *dependency*. Hence, for the statistical analysis, we aggregate the observed social insurance outcomes into two annual dependent variables:

i) **Long-term social insurance dependency**: An indicator variable taking the value 1 if a person during a year claimed any of the social insurance benefits referred to above for at least four months altogether (0 otherwise).

ii) **Overall benefit claims**: A scalar variable taking the values 0,1,…,12, reporting the number of months during a year that a person received social insurance benefits.

The aggregation of all types of social insurance claims into broader outcome measures is partly motivated by the fact that the distinction between them is blurred (Bratsberg *et al.*, 2010a), with large flows between the different programs (Fevang *et al.*, 2004), and partly by our ambition to identify patterns of interest beyond a narrow program-specific Norwegian setting. It is the overall exploitation of social insurance programs – and how this is affected by social interaction processes – that we seek to illuminate.

Figure 2 illustrates some key descriptive features of the two dependent variables. Panels (a) and (c) show how their averages developed within our analysis population from 1992 through 2008 (solid lines), whereas panels (b) and (e) illustrate how they vary by age within the whole data period. Since we follow the same group of people over time in this analysis, it is clear that the strong age gradient shown in panels (b) and (d) is an important factor behind the observed increase in social insurance dependency shown in panels (a) and (c). Now, to the extent that we wish to describe a particular group's overall social insurance propensity in order to investigate its potential effect on the prevailing work norm, we may wish to eliminate the pure age-composition effect. Hence, in the statistical analyses we will use *age-adjusted* social insurance dependency observations to compute the average SI propensities within groups. These are obtained by subtracting from each observed individual outcome the mean outcome for the corresponding age group and then adding the mean outcome for 40-year-olds. The adjustment is made separately for each year.[2] As a result, we obtain age-adjusted observations normalized to a person aged 40. To illustrate the pure calendar-time trends, the dashed lines in panels (a) and (c) illustrate how the outcome variables developed for 40-year-olds, and the dotted lines show corresponding development for 50-year-olds. It is evident that overall social insurance dependency increased quite sharply from around 1997 to 2003, conditional on age. While this rise may – or may not – have been caused by changes in work-norms, the small decline afterwards was at least to some extent related to a reform in the sickness insurance system implemented in July 2004 (see Markussen, 2009) and to the prevalence of an extremely tight labor market until the financial crisis in the Autumn of 2008, with registered unemployment rates hitting a low of 1.5 % just before the crisis.

[2] It turns out the age-adjustment is empirically unimportant for the findings reported in this paper; the results would have changed only marginally had we chosen to use age-*un*adjusted SI propensities.

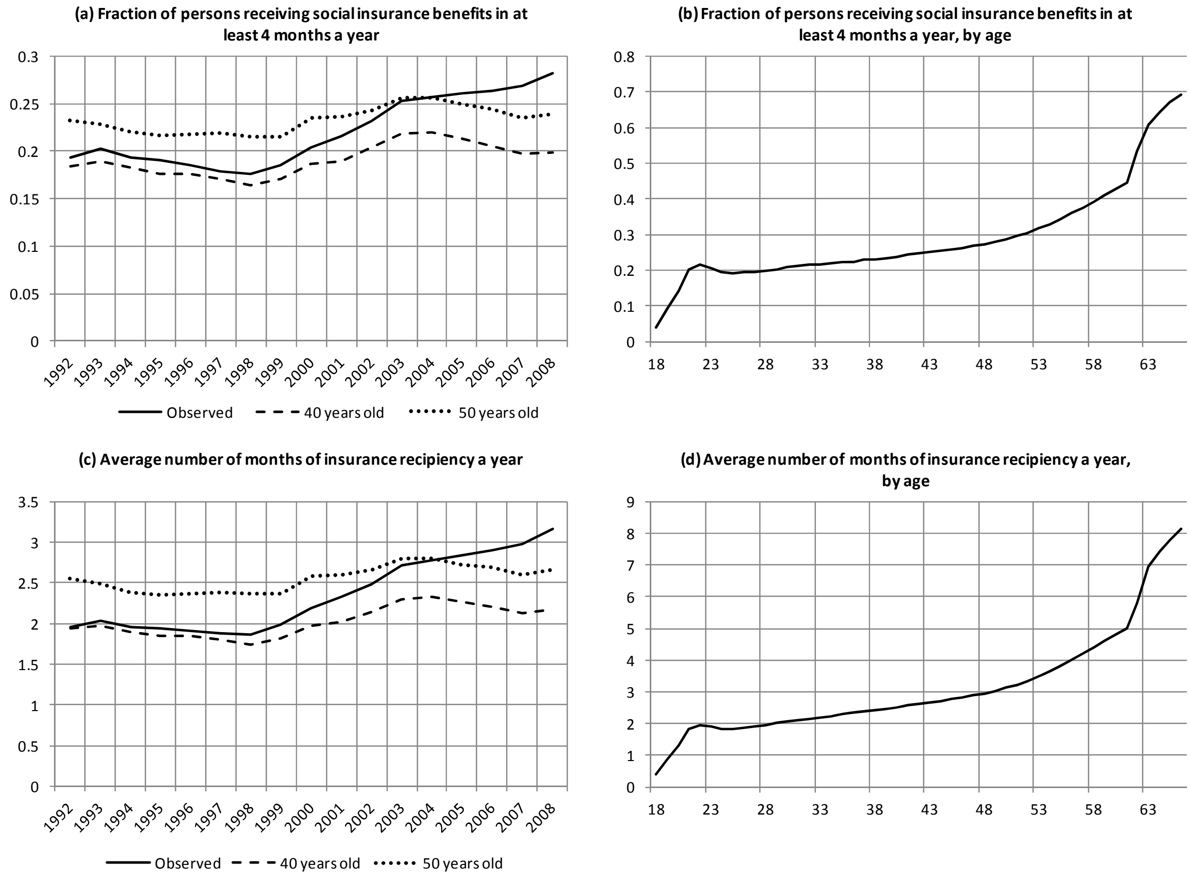

Figure 2. Long-term social insurance dependency and overall benefit claims in Norway, by year (1992-2008) and age (18-66).
Note: Data include all persons who resided in Norway from 1992 to 2008 and who were born between 1942 and 1974 (1,867,662 individuals).

5 Empirical Analysis

Our research question is whether – and to what extent – an individual's use of social insurance benefits is causally affected by the (age-adjusted) use within networks/groups that the individual is closely – or more vaguely – attached to. As noted above, identification of these effects is potentially complicated by endogenous group sorting, social interaction through individual characteristics/attitudes (contextual effects), confounding (unobserved) factors, and simultaneity. Our identification strategy can be summarized as consisting of four elements. First, we circumvent the problem of *dynamic* endogenous group-formation by focusing on groups that – by definition – are stable, such as the families that persons were born into and the neighborhoods to which they belonged at a particular point in time. The price we pay for this is that our "networks" in some cases will serve as imperfect proxies for the various groups of people that agents actually interact with. Hence, compared to analyses based on positively identified and closely tied networks, we expect that interaction effects identified in our analysis will be attenuated. Second, we handle the problems of *initial* group sorting and time-invariant contextual effects and confounding factors by using individual fixed effects. This implies that it is the *timing* (not the occurrence) of SI dependency within networks that identifies the effects of interest. Third, we handle the problems of time-varying confounders by including separate year-dummies for each travel-to-work area (TWA) in Norway, by letting the individuals' *time profiles* of SI claims vary according to some key individual characteristics (birth-cohort, gender, and educational attainment) and by examining the results' robustness with respect to the inclusion of additional time-varying network-specific controls. And fourth, to avoid simultaneity and ensure that the presumed cause actually precedes its effect, we let interaction effects operate with a one-year time lag.

Let $\tilde{y}_{i,t}^{S}$ be an age-adjusted social insurance outcome for individual *i* in year *t*, with *S=LT* (long-term SI dependency) or *S=NM* (number of SI months), see Section 4, and let $\overline{y}_{g,-i,t}^{S} = \frac{1}{N_g}\sum_{j\neq i\in g}(\tilde{y}_{j,t}^{S})$ be the age-adjusted average outcome for persons belonging to a group *g* in year *t*, excluding individual *i*. For the dichotomous outcome variable (long-term dependency), we set up fixed effects (conditional) logit models (CLM) of the following form:

$$\ln\frac{\Pr\left[y_{it}^{LT}=1\right]}{\Pr\left[y_{it}^{LT}=0\right]}=\alpha_i^{LT}+\lambda_t^{LT}\left(x_i\right)+\phi_{rt}^{LT}+\sum_{g\in G}\theta_g^{LT}\overline{y}_{g,-i,t-1}^{LT}, \qquad (4)$$

where α_i^{LT} is an individual fixed effect, $\lambda_t^{LT}\left(x_i\right)$ is a time (year) effect specified separately for different combinations of individual covariates x_i, ϕ_{rt}^{LT} is a TWA-year fixed effect, and *G* is the set of groups/networks potentially influencing the behavior of *i*.[3] The reason why we have removed individual *i* from the network's aggregate is that if there is autocorrelation in individuals' SI dependency – which appears plausible – the inclusion of individual *i* would cause a positive bias in the estimated interaction effect.

For the scalar outcome variable (number of months with benefits), we use the same vector of explanatory variables, estimated with ordinary least squares (OLS):

$$E\left[y_{it}^{NM}\right]=\alpha_i^{NM}+\lambda_t^{NM}\left(x_i\right)+\phi_{rt}^{NM}+\sum_{g\in G}\theta_g^{NM}\,\overline{y}_{g,-i,t-1}^{NM}. \tag{5}$$

The parameters of interest are in both cases θ_g^S, and the corresponding long-run "social multipliers" are equal to $1+\theta_g^S+\left(\theta_g^S\right)^2+...=(1-\theta_g^S)^{-1}$.

As noted above, the individual fixed effect (α_i^S) is included to control for sorting on overall SI-propensity into networks and for time-constant confounders. For the conditional logit model, it implies that only individuals with variation in the outcome variable can be used to estimate the parameters of interest. At first sight, it may appear unnecessary to use individual fixed effects in this setting, since it is confounding factors at the network level that we primarily worry about. However, the removal of individual *i* from the network aggregate implies that we introduce a deterministic source of negative within-network correlation between the network aggregate and the individual outcome; each time a particularly SI-dependent individual is removed from the aggregate, the aggregate falls by construction (and when a non-

[3] In comparison to alternative probability models, the logit model in (4) entails the significant practical advantage that the parameters of interest can be consistently estimated without having to estimate the individual-fixed effects. The model is described and discussed in, e.g., Baltagi (2008, Section 11.1), and Hilbe (2009, Section 13.4.1).

claimant is removed, it rises). As a result, the use of network fixed effects would yield large biases in the interaction effects of primary interest.[4]

TWA-year fixed effects (ϕ_{rt}^{S}) are included to control for time-varying confounding factors with a geographical dimension, such as local business cycle fluctuations. The 90 travel-to-work areas in Norway are defined by Statistics Norway to ensure that persons living within each of these areas operate in a common labor market and have, hence, been subject to exactly the same geographical fluctuations in labor market tightness over time. Note that TWA-year fixed effects are defined on the basis of persons' *initial* residential area; i.e., the area they lived in at the start of our analysis period and at which point we construct the various networks/groups used in the analysis. We do not exploit information on subsequent migrations, as we expect that migration decisions to some extent are endogenous responses to changes in labor market status (including transitions to social insurance dependency).

The time function $\lambda_t^S\left(x_i\right)$ is included to control for sorting into networks on individual SI *trends*. This is required if persons are sorted into networks not only on the basis of their unobserved SI risks (which are accounted for by the fixed effects), but also on the basis of the way these risks change over time. It is of course impossible to estimate separate year effects for each individual. We do, however, estimate separate year effects for each annual birth cohort (the model is saturated in the age-year space).[5] In addition we include *gender*×*year* and *gender*×*age* dummy variables. In some specifications, we include *education*×*year* or (alternatively) *education*×*age* dummy variables to take into account that different education groups may have different SI time profiles. As part of the robustness exercise, we also estimate models where the education-specific year-effects are allowed to vary by birth-year, gender, and

[4] In the next subsection, we report an example illustrating that this bias would be completely devastating in the present context.

[5] With this specification, we can obviously not distinguish age from time effects, since age and time is perfectly correlated at the individual level; see Biørn *et al.* (2012).

travel-to-work area (yielding more than 1 million time-varying dummy-variables). Educational attainment is in most specifications represented by a vector of education dummy variables that reflect the level of education (number of years) as well as its type (35 categories). As with residential area, we measure education in 1992 to ensure that it does not incorporate endogenous responses to social insurance outcomes.[6]

In the following subsections, we examine interaction effects within four different types of networks separately; i.e., neighborhoods, schoolmates, nationalities, and families. In all these exercises, we distinguish between peer groups according to their presumed relational distance to *i*. In principle, we could have examined all types of networks simultaneously. However, as we explain below, the analysis of each network type requires different cuts and adaptations of the data and the models.

5.1 Neighborhoods

We start out examining the impacts of social insurance dependency within small geographical areas. The purpose is to examine the degree to which SI claim propensities spread endogenously within small communities and to which extent such interaction effects depend on relational distance. The latter is measured by differences in age, gender, and educational attainment. The central geographical entity we focus on is a person's "neighborhood". Our definition of neighborhoods correspond to the so-called "basic statistical units" ("grunnkretser") used by Statistics Norway. They are designed to resemble genuine neighborhoods, and contain residences that are homogeneous with respect to location and type of housing.[7] There are

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[6] Due to the large number of observations (up to around 16 million person-years, see next section) and the large number of dummy variables (2,163 in the most general specification) in addition to the person-fixed effects, estimation raises some computational challenges. For the conditional logit estimation we have used a standard recursive algorithm like the one used by Stata's clogit-command, but keeping each set of dummies as a single ordinal variable during the computations to avoid excessive and unnecessary multiplication by zero. For the OLS, we have used a novel algorithm based on The Method of Alternating Projections as described in Gaure (2012) and implemented in the R-package "lfe"; see http://cran.r-project.org/web/packages/lfe/citation.html.

[7] For a more thorough description of the neighborhood concept and other geographical entities used in this paper, see Statistics Norway (1999).

13,700 basic statistical units in Norway, each populated by around 350 individuals on average. To avoid endogenous geographical sorting, our analysis is based on recorded address at the start of our analysis period; i.e., in 1992. To reduce the potential attenuation bias caused by subsequent out-migration, we limit the analysis in this subsection to persons belonging to the 1942-1960 birth cohorts, implying that they were between 32 and 50 years old – and hence reasonably settled – at the time of peer group construction in 1992.[8]

In total, there are around 1 million individuals included in our analysis population, each of them contributing 16 annual observations (the 1992-observations are lost due the inclusion of the lagged SI dependency rate), see Table 1. This leaves us with a total number of 16.4 million person-year observations. However, in the conditional logit model, only individuals with variation in the outcome contribute to identification of the parameters of interest. This leaves us with 551,000 individuals and around 8.8 million annual observations. On average, the persons in our dataset claim social insurance benefits in around two months each year. Around 25 % of the persons are long-term claimants in a typical year; i.e., they claim benefits for at least four months.

Table 1. The two outcome measures – Descriptive statistics – Neighborhoods (1942-1960 cohorts)

Number of individuals	1,027,253
Average size of the neighborhood (individuals included in the data)	92.5
Long-term dependency (at least 4 months)	
Number of individuals with long-term dependency in all years	75,898 (5.1 %)
Number of individuals with no long-term dependency in any of the years	395,362 (38.5 %)
Number of individuals with variation in long-term dependency	550,982 (55.0 %)
Mean fraction long-term dependent all individuals	0.250
Mean fraction long-term dependent for individuals with variation only	0.327
Number of months with benefits	
Mean annual number of benefit months all individuals	2.75
Number of individuals with 0 benefit months all years	199,498 (19.4%)
Number of individuals with 12 benefit months all years	52,212 (5.1%)

[8] In our data, 58 % of the individuals lived in exactly the same neighborhood in 2008 as they did in 1992.

With respect to the identification of interaction effects within neighborhoods, we see two principal threats to the validity of our research design. The first is the possibility of local labor market shocks that occur below the travel-to-work area level. An example would be downsizing or closure of an important local workplace. The second concern is the occurrence of shocks that are not necessarily specific to a particular neighborhood, but rather to the types of people that are concentrated within it. An example would be a significant decline in an industry that happens to employ a disproportionally large fraction of a neighborhood's workforce. To address these concerns, we compare estimated neighborhood effects with the corresponding estimates associated with similar "artificial" peer-groups from neighboring neighborhoods and from different parts of the country, respectively. Furthermore, to assess the robustness of our findings, we add alternative sets of time-varying controls, including variables intended to proxy local or industry-specific shocks.

To construct peer groups in neighboring neighborhoods, we draw persons from the local area outside the reference person's own neighborhood. Our local areas correspond to the so-called "statistical tracts" ("delområder"), drawn up by Statistics Norway. They are designed to encompass neighborhoods that naturally interact, e.g., by sharing common service/shopping centre facilities. A typical local area comprises around 8-9 neighborhoods and 3,100 inhabitants. We construct our neighboring neighborhood peer groups by conducting a one-to-one exact-match sampling; i.e., for each person in *i's* own neighborhood, we draw one person from the neighboring neighborhoods who is of the same gender, has the same age, and has exactly the same education (in terms of both level and type).[9] Given the geographical proximity of neighboring neighborhoods, we would expect there to be some room for social interaction with *i*, although not to the same extent as for the closest neighbors in *i's* own

[9] We use 35 different education categories in this matching process. We obtain an exact match on gender/age/education in 98 % of the cases. For the remaining two percent, we chose a person with slightly different age and/or slightly different education.

neighborhood. Moreover, it is hard to envisage shocks that affect *i's* neighborhood, without affecting the other neighborhoods in the same local area also. Hence, if our estimates primarily reflect uncontrolled-for local shocks, we would expect the estimated effects to be similar for true neighbors and for persons living in neighboring neighborhoods. If they reflect social interaction, on the other hand, we would expect the effect to be significantly larger for the true neighbors. To further examine and control for shocks that are specific to the type of persons who have sorted into particular neighborhoods (and local areas), we also construct artificial peer groups of presumed strangers, i.e., of persons living in another part of the country, but who share exactly the same observed characteristics as the true neighbors (based on the same exact-matching-procedure). Finally, as an additional robustness check, we add to the model proxies for observed neighborhood-specific downsizings and economic fluctuations. A downsizing is assumed to have occurred if at least two persons living in the same neighborhood and working in the same firm register as unemployed in the same year. To represent economic fluctuations that are of relevance for each neighborhood, we first compute industry-specific annual transition rates from employment to unemployment for all Norwegian employees.[10] We then use the initial (1992) employment structure in each neighborhood to compute neighborhood-specific weights. Finally, we use these weights, multiplied with the time-varying industry specific unemployment risks to compute the annual unemployment risks for each neighborhood.

[10] We use 12 different industries, based on ISIC codes: i) Farming and fishing, ii) Oil, gas and mining, iii) Manufacturing, iv) Electricity and water supply, v) Construction, vi) Wholesale and retail trade, hotels and restaurants, vii) Transport, storage and communication, viii) Finance, insurance and real estate, ix) Public administration and defense, x) Schools and education, xi) Health services, and xii) Other.

Table 2. Estimated interaction effects within neighborhoods

	Long-term dependency (CLM)					Number of months with benefits (OLS)						
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
Own neighborhood	1.250*** (0.026) [0.235]	1.211*** (0.026) [0.228]	1.199*** (0.026) [0.225]	1.184*** (0.026) [0.223]	1.164*** (0.026) [0.219]	0.158*** (0.003)	0.153*** (0.003)	0.150*** (0.003)	0.142*** (0.003)	0.142*** (0.003)	0.139*** (0.003)	0.140*** (0.003)
Similar group (matched on education, age and gender) in same local area, but different neighborhoods		0.545*** (0.025) [0.102]	0.536*** (0.025) [0.101]	0.534*** (0.025) [0.100]	0.532*** (0.025) [0.100]		0.070*** (0.003)	0.069*** (0.003)	0.061*** (0.003)	0.061*** (0.003)	0.058*** (0.003)	0.047*** (0.003)
Similar group (matched on education, age and gender) in a different part of the country			0.293*** (0.027) [0.055]	0.284*** (0.027) [0.053]	0.280*** (0.027) [0.053]			0.045*** (0.003)	0.037*** (0.003)	0.038*** (0.003)	0.034*** (0.003)	0.013*** (0.003)
Model specification - Number of variables included												
Individual fixed effect (N)	550,982	550,982	550,982	550,982	550,982	1,027,253	1,027,253	1,027,253	1,027,253	1,027,253	1,027,253	1,027,253
TWA-year fixed effect	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321	
Included in individual trend												
Cohort×age	271	271	271	271	271	271	271	271	271	271		
Gender×age	29	29	29	29	29	29	29	29	29	29		
Gender×year	16	16	16	16	16	16	16	16	16	16		
Education×age				981	981				981	981		
Gender×cohort×year×education											20,311	
Geder×cohort×year×education×TWA												1,025,387
Controls for neighborhood shocks *t-1*												
Downsizing					Yes					Yes	Yes	Yes
Unemployment risk					Yes					Yes	Yes	Yes

Notes: Standard errors in parentheses. Marginal impacts of a 1 percentage point increase in long-term SI dependency in brackets, evaluated at average rate (0.25). "Similar groups" are matched on education (2 digit codes for level and field), birth year and gender, and the groups are of exactly same size as a person's own neighborhood.
*(**)(***) Significant at the 10(5)(1) percent level.

Estimation results are provided in Table 2; for long-term dependency (CLM) in Columns I-V, and for the number of benefit months (OLS) in Columns VI-XII. The estimated neighborhood effects are positive and significant in all specifications, but decline slightly as we include matched artificial peer groups from the local area and from the rest of the country. Apart from this, the estimated coefficients are remarkably stable across widely different model specifications. Evaluated at the mean long-term social insurance dependency rate (25.0 %), the estimated logit coefficients all imply that a 1 percentage point increase in the (age-adjusted) long-term dependency rate among the closest 1992-neighbors causes the dependency-risk of a typical agent to rise by 0.22-0.23 percentage points, ceteris paribus, implying a long-run social multiplier around 1.28. Similarly, according to the fixed effects OLS model, a one-month rise in annual SI claims among the closest neighbors causes the number of expected claimant months to rise by 0.14-0.15, implying a long-run multiplier of 1.16.[11] It is notable that the estimated neighborhood effects change little when we expand the set of time-varying controls. Adding 981 education-age dummy variables has little impact on the coefficients of interest (Columns IV and IX). Adding indicators for local downsizings and unemployment risks (Columns V and X) also does little to modify the estimated neighborhood effects. And even when we add more than 20,000 gender-cohort-education-year dummy variables (Column XI) or more than 1 million gender-cohort-education-year-TWA dummy varia-

[11] To illustrate the importance of using individual rather than neighborhood fixed effects, we have re-estimated the OLS model reported in Table 2, Column IV, using fixed effects for own neighborhood instead of individual fixed effects. We then obtained an estimate for the interaction effect of own neighborhood of -1.404 (standard error 0.005); i.e., way off our preferred estimate of 0.150. The reason for this is that when we only use neighborhood fixed effects, the estimate is negatively biased by the mechanical within-network correlation arising from the fact that when we remove a person with high (low) SI propensity from the group average, the average declines (increases).

bles (Column XII) to the OLS model, the estimated neighborhood effects are hardly affected at all.[12]

When we move on to the neighboring neighborhoods in the local area, the estimated interaction effects are cut by more than half. As pointed out above, this is consistent with a social interaction interpretation, and correspondingly hard to explain with reference to unobserved local shocks. As we move out of the local area, the effect is cut by half again. The latter effect is still statistically significant though, apparently indicating that there might have been some common SI shocks related to the interaction of gender, age, and educational attainment. Alternatively, we may speculate that the dependency rates of persons who are similar to *i's* neighbors do affect *i's* own claim propensity even when they live too far away to interact directly with *i*, i.e., that agents are responsive with respect to the *aggregate* dependency rates among people who are similar to themselves. Given the fine-grained exact matching procedure we have used to construct the artificial peer groups, it is also likely that *i's* neighbors actually interact with persons in the other-part-of-the-country peer group. Some of the education-groups used in the statistical matching are quite small, implying that persons who are born in the same year and have taken exactly the same education at some point may have studied together.

The importance of "similarity" implies that we would expect to find differences in social interaction effects even *within* genuine neighborhoods. In particular, we may hypothesize that persons are more strongly influenced by persons of same sex and similar age and education than by more dissimilar neighbors. To examine the empirical relevance of this hypothesis, we have re-estimated the models using a multiple of group-specific averages within own neighborhoods as explanatory variables. To ascertain direct comparability, we weight each

[12] For computational reasons, we were not able to do this exercise for the conditional logit model. It may be noted, however, that the inclusion of 981 education-age-dummy variables does not noticeably affect the estimated interaction effects.

group mean by its size relative to the whole neighborhood (these weights are computed separately for each individual), such that each coefficient is directly comparable to the overall neighborhood effect; see Section 3. Since the alternative formulations of individual trends produced almost exactly the same results in Table 2, we use the more parsimonious versions of the model for these exercises, but maintain a vector of education-year dummy variables when we examine the impacts of education-specific SI rates. Table 3 presents the estimated gender-differentiated neighborhood effects separately for men and women. Particularly for men, we find that the behavior of same-sex neighbors is more important than the behavior of opposite-sex neighbors. Another message coming out of Table 3 is that men's propensity to claim SI is in general more strongly influenced by their neighbors' behavior than women's propensity.[13] If interaction effects really are larger for men than for women, we would expect to find larger variation in men's than in women's average SI dependency rates across neighborhoods, and also larger variation in men's within-neighborhood changes over time. These predictions are confirmed by the data (not shown in tables). Using age-adjusted outcomes, we find, for example, that the coefficients of variation for both the two neighborhood-averaged outcome measures in 2008 are around 0.41 for women and 0.58 for men (although the coefficients vary somewhat from year to year, they are larger for men in all years). Looking at absolute relative changes in SI-dependency within neighborhoods from 1993 to 2008, we find, for the long-term dependency outcome, that the coefficients of variation are 0.88 for women and 0.92 for men. For the number-of-months outcome, the corresponding numbers are 0.84 for women and 0.94 for men.

[13] The finding that peer effects are larger for men than for women has also been reported in studies of sickness absence (Hesselius *et al.*, 2009), schooling choices (Lalive and Cattaneo, 2009), and immigrant student achievement (Åslund *et al.*, 2011).

Table 3. Estimated effects of weighted group-specific neighborhood averages on own outcomes by gender

	Long-term dependency (CLM)			Number of months with benefits (OLS)		
	All	Men	Women	All	Men	Women
Own sex	1.723*** (0.037) [0.324]	1.868*** (0.055) [0.352]	1.140*** (0.053) [0.214]	0.205*** (0.005)	0.197*** (0.006)	0.166*** (0.007)
Opposite sex	0.789 (0.038) [0.148]	0.960*** (0.055) [0.180]	1.066*** (0.055) [0.200]	0.114*** (0.005)	0.128*** (0.006)	0.150*** (0.007)
Model specification - Number of variables included						
Individual fixed effect (N)	550,982	259,416	291,566	1,027,253	524,868	502,385
TWA-year fixed effect	1,321	1,321	1,321	1,321	1,321	1,321
Included in individual trend						
Birth cohort×age	271	271	271	271	271	271
Gender×age	29			29		
Gender×year	16			16		

Notes: Standard errors in parentheses. Marginal impacts of a 1 percentage point increase in long-term SI dependency in brackets, evaluated at average rate (0.25). *(**)(***) Significant at the 10(5)(1) percent level.

Table 4. Estimated interaction effect for neighborhoods by peer group's age relative to own age

	Long-term dependency (CLM)	Number of months with benefits (OLS)
Younger neighbors (more than 5 years younger)	0.922*** (0.071) [0.173]	0.047*** (0.008)
Same age neighbors (+/- 5 years)	2.214*** (0.053) [0.417]	0.283*** (0.006)
Older neighbors (more than 5 years older)	0.771*** (0.037) [0.145]	0.098*** (0.005)
Model specification - Number of variables included		
Individual fixed effect (N)	550,982	1,027,253
TWA-year fixed effect	1,321	1,321
Included in individual trend		
Birth cohort×age	271	271
Gender×age	29	29
Gender×year	16	16

Notes: Standard errors in parentheses. Marginal impacts of a 1 percentage point increase in long-term SI dependency in brackets, evaluated at average rate (0.25). The three neighborhood groups are for each individual weighted by size. *(**)(***) Significant at the 10(5)(1) percent level.

Table 5. Estimated interaction effect for neighborhoods by peer group's education relative to own

	Long-term dependency (CLM)	Number of months with benefits (OLS)
Neighbors with lower education	0.840*** (0.039) [0.158]	0.072*** (0.004)
Neighbors with education of approximately same length	1.292*** (0.032) [0.243]	0.176*** (0.004)
Neighbors with higher Education	0.775*** (0.042) [0.146]	0.113*** (0.005)
Model specification - Number of variables included		
Individual fixed effect (N)	550,982	1,027,253
TWA-year fixed effect	1,321	1,321
Included in individual trend		
Birth cohort×age	271	271
Gender×age	29	29
Gender×year	16	16
Education×year	31	31

Notes: Standard errors in parentheses. Marginal impacts of a 1 percentage point increase in long-term SI dependency in brackets, evaluated at average rate (0.25). The three neighborhood groups are for each individual weighted by size. Comparison of education levels is based on three groups: i) Less than 11 years (primary education only), ii) 11-13 years (lower or upper secondary), iii) more than 13 years (college, university).
*(**)(***) Significant at the 10(5)(1) percent level.

Tables 4 and 5 present the results for age-differentiated and education-differentiated neighborhood-influences; respectively. As expected, the results indicate that individuals are more strongly affected by similar than by dissimilar neighbors, both in terms of age and education. However, we do not find clear patterns with respect to whether those who are older than *i* are more or less important than those who are younger and whether those who have higher education than *i* are more or less important than those who have lower education.

How do our findings fit with existing Norwegian evidence? As mentioned in Section 2, Rege *et al.* (2012) report social multipliers for disability pension entry with respect to neighbors of similar age (41-62 years) in the range of 1.3-1.4. Given that they apply a completely different identification strategy (using neighborhood layoffs as instrument for neighborhood disability program entry) and also that their dependent variable only covers one of the SI programs included in our outcome measures, it is notable that the social multiplier estimates they end up with are strikingly similar to ours.

5.2 Schoolmates

We now turn our attention to networks consisting of persons who went to the same junior high school at the same point in time. Junior high school in Norway is a three-year track, normally attended at age 13-15. The total group of school mates during this period thus consists of five birth-cohorts; those at the same age, and those born up to two years before and two years after. Pupils at the same age will often go to the same class, and also be schoolmates during the whole three-year track. Older and younger schoolmates will go to different classes, and only attend the same school for parts of the three-year period. To explore the importance of relational distance in this setting, we may thus compare the influence exercised by pupils who graduated from the same school at *exactly* the same time with the influence exercised by those who graduated from the same school 1-2 years before or after. Due to data limitations, we can only use a subset of our analysis population for this purpose, namely those born between 1961 and 1971 (11 cohorts). To ensure that older and younger students really went to a different class, we also require the group of levelmates to comprise at least 30 persons. Finally, we remove siblings from each person's peer group. In total, we construct data for 5,896 annual schoolmate groups, on average consisting of 88.4 persons. Descriptive statistics are provided in Table 6.

Table 6. The two outcome measures – Descriptive statistics – Schoolmates

Number of individuals	527,393
Average size of same-level peer group	107.9
Long-term dependency (at least 4 months)	
Number of individuals with long-term dependency in all years	12,408 (2.4%)
Number of individuals with no long-term dependency in any of the years	233,375 (44.3%)
Number of individuals with variation in long-term dependency	294,018 (55.8%)
Mean fraction long-term dependent all individuals	0.178
Mean fraction long-term dependent for individuals with variation only	0.274
Number of months with benefits	
Mean annual number of benefit months all individuals	1.82
Number of individuals with 0 benefit months all years	93,096 (17.7%)
Number of individuals with 12 benefit months all years	5,277 (1.0%)

The estimation results are displayed in Table 7. Focusing first on the overall interaction effect among schoolmates, we find that a 1 percentage point increase in the long-term dependency rate among all junior high school peers raises the dependency-risk of a typical agent by 0.19 percentage points. And a one-month rise in annual SI claims raises the number of expected claimant months by 0.14. In order to evaluate the role of relational distance, we divide the schoolmates into six groups, defined by level (lower, same, or higher) and gender (same or opposite). Again, we weight each group's SI rates by relative group size, so that all the estimated coefficients are directly comparable to those obtained for all schoolmates. The results clearly show that same-sex peers at the same level (levelmates) have significantly larger influence than other schoolmates. For opposite-sex schoolmates, there tend to be somewhat smaller differences between the levels, indicating that opposite-sex friendships to a lesser extent are confined to own class. The observation that women are less affected by older than by younger boys may at first sight appear surprising, but it is consistent with recent evidence reported by Poulin and Pedersen (2007) indicating that most adolescent opposite-sex friendships where the boy is older than the girl take place outside the school, in contrast to friendships where the girl is older than the boy.

Table 7. Estimated interaction effects for schoolmates

	Long-term dependency (CLM)			Number of months with benefits (OLS)		
	Men and women	Men	Women	Men and women	Men	Women
All schoolmates	1.278*** (0.061) [0.189]			0.137*** (0.007)		
Own sex						
1-2 level below		1.055*** (0.220) [0.156]	0.436*** (0.169) [0.064]		0.070*** (0.020)	0.040* (0.022)
Same level		1.672*** (0.337) [0.247]	1.680*** (0.259) [0.248]		0.179*** (0.031)	0.168*** (0.033)
1-2 levels above		0.170 (0.228) [0.025]	0.557** (0.173) [0.082]		0.046** (0.021)	0.073*** (0.022)
Opposite sex						
1-2 level below		0.030 (0.198) [0.004]	1.341*** (0.195) [0.198]		0.017 (0.018)	0.124*** (0.025)
Same level		1.178*** (0.302) [0.174]	0.628** (0.300) [0.092]		0.088*** (0.027)	0.074* (0.038)
1-2 levels above		0.830*** (0.201) [0.122]	0.559** (0.202) [0.082]		0.072*** (0.018)	0.059** (0.026)
	Model specification - Number of variables included					
Individual fixed effect (N)	320,466	140,038	180,428	573,371	292,069	281,302
TWA-year fixed effect	1,321	1,321	1,321	1,321	1,321	1,321
Included in individual trend						
Birth cohort×age	166	166	166	166	166	166
Gender×age	24			24		
Gender×year	16			16		

Notes: Standard errors in parentheses. Marginal impacts of a 1 percentage point increase in long-term SI dependency in brackets, evaluated at average rate (0.18). Groups at different levels are for each individual weighted by size. *(**)(***) Significant at the 10(5)(1) percent level.

Again, we may ask whether it is possible to explain our findings with references to idiosyncratic trends or unobserved shocks. While we acknowledge that it is difficult to rule out selective primary school enrolment as well idiosyncratic shocks that correlate with persons' school affiliations – even with all our control functions in place – we find it difficult to see how this could explain the much larger influence exercised by same-level-same-sex pupils compared to pupils belonging to adjacent cohorts and the opposite sex. We thus interpret the overall pattern revealed by Table 7 as convincing evidence of social interaction effects.

5.3 Nationalities

Some of the most influential existing studies on social insurance interaction effects are based on data for ethnic minorities (Bertrand *et al.*, 2000; Aizer and Currie, 2004; Åslund and Fredriksson, 2009). We follow up on this literature by looking at SI use among immigrants from low-income countries.[14] Our focus is on immigrants who reside in areas where there are sufficient numbers of other immigrants from the same country for a network of some size to be established. More specifically, we define a nationality-network as a group of immigrants from the same origin country who resided in the same local area in 1992 (the "neighborhoods" discussed above are too small for this purpose). To be included in the analysis, we require a network size of minimum 10 persons. Based on this strategy, we end up with 28,116 persons from 23 different countries, divided between 889 local immigrant networks; see Table 8 for descriptive statistics on the outcomes.

One could imagine that the social interaction effects decrease with geographical distance for immigrants as well as for natives, suggesting that we should examine how the estimated effects change as we substitute close groups with more distant ones (but with the same nationality). Our data impose some limitations, however, as nationality networks of the required size are typically located close together. Instead, we use immigrants from other low-income countries as candidates for more "distant" peers. In addition, we look at how immigrants are affected by SI use among natives within the same local area. Again, we compose

[14] We disregard immigrants from high-income countries here, both because they do not tend to be concentrated in particular geographical areas and because they do not tend to reside permanently in Norway.

the groups of other immigrants and natives such that they are of equal size and share the same individual characteristics as the person's own same-nationality network.[15]

Table 8. The two outcome measures – Descriptive statistics – Immigrant networks

Number of individuals	28,116
Average size of immigrant network	98.0
Long-term dependency (at least 4 months)	
Number of individuals with long-term dependency in all years	1,946 (6.9%)
Number of individuals with no long-term dependency in any of the years	8,007 (28.5%)
Number of individuals with variation in long-term dependency	18,021 (64.1%)
Mean fraction long-term dependent all individuals	0.320
Mean fraction long-term dependent for individuals with variation only	0.389
Number of months with benefits	
Mean annual number of benefit months all individuals	3.32
Number of individuals with 0 benefit months all years	5,478 (19.5%)
Number of individuals with 12 benefit months all years	799 (2.8%)

Table 9 presents the results. There is clearly a strong social interaction effect among immigrants from a common source country – stronger than what we have found to be the case among neighbors in general and among former schoolmates. A one percentage point increase in SI dependency among same-country immigrants (in the same local area) raises own SI dependency by approximately 0.29 percentage points. On the other hand, there is no effect at all of natives' behavior, and there is a small *negative* interaction effect of SI dependency among immigrants from other source countries. The latter result is of interest, as it may indicate a preference for not being associated with other immigrant-groups. Although we are not aware of any direct evidence on inter-ethnic interaction effects of this kind, it is interesting to note that Conley and Topa (2002) report a significant negative correlation in unemployment rates across census tracts in Chicago that are distant in their racial/ethnic composition.

[15] The groups are matched on gender, age, and, and four education levels. Based on these characteristic we obtain exact matches for 99.4 % in the group of natives and for 69.7 % in the group of immigrants from other countries.

Table 9. Estimated interaction effect for immigrant networks

	Long-term dependency (CLM)	Number of months with benefits (OLS)
Immigrants from same source country living in same local area	1.320*** (0.046) [0.288]	0.177*** (0.007)
Similar immigrants from other low-income source countries in same local area	-0.079* (0.045) [-0.017]	-0.001 (0.007)
Similar natives in same local area	0.028 (0.069) [0.006]	0.004 (0.010)
Model specification - Number of variables included		
Individual fixed effect	18,021	28,116
TWA-year fixed effect	991	991
Included in individual trend		
Birth cohort×age	481	481
Gender×age	39	39
Gender×year	16	16

Notes: Standard errors in parentheses. Marginal impacts of a 1 percentage point increase in long-term SI dependency in brackets, evaluated at average rate (0.32). The groups of natives and similar immigrants from other countries are matched on age, gender, and education. *(**)(***) Significant at the 10(5)(1) percent level.

We find it hard to see how the conspicuous difference in the roles of persons from own and other low-income countries could be explained by unobserved confounders. Immigrants from different low-income countries typically work in similar sectors of the economy, with a domination of low-skill service sector jobs (Bratsberg et al., 2010b); hence they would typically be similarly affected by, say, business-specific cyclical fluctuations.

5.4 Families

We conclude our examination of network effects by looking at within-family interactions. A family is in this context defined as consisting of parents, siblings, uncles, aunts, and cousins. To identify such families, we need to observe grandparents. Since we rarely observe grandparents for elderly individuals (because of data limitations), we restrict the analysis to person born after 1950 and we include only those with identified families of at least 10 persons. This leaves us with 90,455 persons. Descriptive statistics are provided in Table 10. Note that family networks, in contrast to the networks discussed above, are not mutually exclusive; i.e., a single individual may belong to more than one family (e.g., as a brother or father in one and as a cousin or uncle in another).

Table 10. The two outcome measures – Descriptive statistics – Families

Number of individuals	89,142
Average size of family network	14.7
Of which are siblings or parents	3.2
…uncles / aunts /cousins	11.4
Long-term dependency (at least 4 months)	
Number of individuals with long-term dependency in all years	2,282 (2.7%)
Number of individuals with no long-term dependency in any of the years	32,335 (36.3%)
Number of individuals with variation in long-term dependency	53,791 (60.3%)
Mean fraction long-term dependent all individuals	0.199
Mean fraction long-term dependent for individuals with variation only	0.263
Number of months with benefits	
Mean annual number of benefit months all individuals	2.02
Number of individuals with 0 benefit months all years	11,982 (13.4%)
Number of individuals with 12 benefit months all years	1,167 (1.3%)

Estimation results are provided in Table 11. There is a statistically significant – though quantitatively moderate – interaction effect within families; see Columns I and V. One explanation for the relatively small effects compared to the other network types is simply that the family networks are small (only 14.7 persons on average). When we divide the families into different categories, we obviously get very few persons behind each group-specific mean; hence the results become somewhat unstable. Nevertheless, a relatively clear pattern emerges: Family influences decline rapidly with distance, both geographically (Columns II and VI) and relational (Columns III and VII). We realize, however, that the interaction effects estimated within families are less “confounder-resistant” than the effects estimated for other types of networks, since, e.g., the disposition towards being hit by adverse health shocks is influenced by biological components.

Table 11. Estimated interaction effects within families

	Long-term dependency (CLM)				Number of months with benefits (OLS)			
	I	II	III	IV	V	VI	VII	VIII
Whole family	0.377*** (0.026) [0.060]				0.064*** (0.003)			
Family in same TWA		0.453*** (0.035) [0.072]				0.077*** (0.004)		
Family in different TWA		0.286*** (0.038) [0.046]				0.049*** (0.004)		
Siblings and parents			0.888*** (0.050) [0.142]				0.148*** (0.006)	
Uncles, aunts and cousins			0.186*** (0.031) [0.030]				0.034*** (0.004)	
Family mother's side				0.232 (0.038) [0.037]				0.042*** (0.004)
Family father's side				0.270 (0.042) [0.043]				0.047*** (0.005)
	Model specification - Number of variables included							
Individual fixed effect (N)	53,791	53,791	53,791	53,791	89,142	89,142	89,142	89,142
TWA-year fixed effect	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321
Included in individual trend								
Birth cohort×age	346	346	346	346	346	346	346	346
Gender×age	34	34	34	34	34	34	34	34
Gender×year	16	16	16	16	16	16	16	16

Note: Standard errors in parentheses. Marginal impacts of a 1 percentage point increase in long-term SI dependency in brackets, evaluated at average rate (0.20). For the models in Columns II, III, IV, VI, VII, and VIII, the different family groups are for each individual weighted by size.
*(**)(***) Significant at the 10(5)(1) percent level.

6 Conclusion

We have shown that there are significant social interaction effects in the use of social insurance (SI) benefits in Norway. Social insurance dependency is contagious. Exogenous changes in SI dependency thus tend to be enlarged by self-reinforcing group-behavior, implying the existence of a social multiplier. Within small neighborhoods, this multiplier is conservatively estimated to be around 1.3. We have also identified significant social multipliers among previous schoolmates and within families and immigrant networks. The complementarities in individual behavior exposed in our empirical analysis imply that social insurance dependency is path dependent and subject to multiple equilibria. This can potentially explain why large regional differences in dependency rates tend to persist and why we frequently witness time-trends with no apparent observed cause.

The methodological approach used in this paper has been designed to identify and estimate local social propagation mechanisms, and we have argued that we have done so in a way that convincingly and robustly distinguishes endogenous interactions from other sources of within-group correlations. In particular, we have identified a conspicuous tendency for estimated interaction effects to rise with measures of relational closeness in a way that it is difficult to find alternative explanation for. Any social contagion operating at the aggregate or regional level, however, for example through an effect of overall SI propensity on the disutility/stigma of claiming SI benefits, have been effectively "controlled away" by the use of separate year dummy variables for different travel-to-work areas. We have done this *not* because we claim that such aggregate/regional effects are empirically irrelevant, but because we see no way to convincingly disentangle them from other sources of time changes in SI dependence rates. We actually believe that the identification of social multipliers at local levels may be indicative of such effects being present at the aggregate level as well.

The networks used in our analysis are clearly imperfect representations of the groups of people that persons really interact with. There will typically be a number of neighbors, former schoolmates, and family members to whom an individual has no relationship at all. Hence, to the extent that the estimates reported in this paper are interpreted as measuring social interaction effects among genuine peer groups, they will most likely be significantly biased towards zero.

The policy implications of the endogenous social interaction effects identified in this paper are important. If governments can find ways to reduce the social insurance rolls directly – e.g., by tightening gate-keeping, increasing rehabilitation efforts, reducing benefit levels, or by expanding activation programs – they can expect a significant "bonus" reduction through the social multiplier. This implies that strategies to get individuals off the SI roll may be cost effective even when the direct costs exceed the benefits for each individual claimant. Furthermore, the mere existence of (sizeable) social interaction effects can be interpreted as evidence that moral hazard problems are empirically relevant: SI claims are not triggered by exogenous job loss or health shocks alone; they are the result of individual choices made on the basis of individual preferences. And these preferences apparently incorporate a malleable social norm.

7 References

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