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Social Interaction and Sickness Absence*

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Abstract

Is the sickness absence of an individual affected by the sickness absence behavior of the neighbors? Well-known methodological problems, in particular the so-called reflection problem, arise when trying to answer such questions about group effects. Based on data from Sweden, we adopt several different approaches to solve these problems. Regardless of the approach chosen, we obtain statistically significant estimates indicating that group effects are important for individual sickness absence behavior.

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1. Introduction

The behavioral consequences of social insurance cannot be fully understood solely by reference to traditional medical entities, such as the health status of the population. Of course, economists would also emphasize economic incentives, such as prices (i.e., replacement rates) and income. One may however argue that these entities do not suffice to explain behavior; social interaction, for instance in the form of group effects on individual behavior, should also be taken into account. While group effects have been extensively analyzed theoretically, empirical analysis has been held back by methodological problems.[1] In particular, the so-called reflection problem (Manski, 1993) may make it difficult to distinguish causal group effects from other forms of correlations between group and individual behavior.

In this paper we study how group behavior in terms of sickness absence influences an individual's utilization of sick-pay insurance ("temporary disability insurance"), which is an important element of social insurance in Europe. Based on Swedish data, we ask two questions. Is there evidence that group influence exists in sickness absence behavior? And, if so, how large might those effects be?

Social interaction may occur on several different arenas. For instance, national mass media may inform the individual what is considered as "normal" behavior in the nation as a whole. Social interaction may also take place within country-wide professions or organizations. In this paper, we focus on personal interaction at local levels, namely neighborhoods. More specifically, we exploit variation in absence behaviour across neighborhoods for the purpose of identifying social interaction, and we define group effects as the individual's adjustment to normal behaviour among his or her neighbors. The basic idea is that encounters with neighbors are one important mechanism by which social norms are transmitted and upheld.[2]

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[1] For general analyses of the influence of social norms on individual behavior, see, for instance, Parsons (1952), Moffit (1983), Bicchieri (1990), Lindbeck (1995), Lindbeck, Nyberg and Weibull (1999), Manski (1993) and Glaeser, Sacerdote and Scheinkman (2003).

[2] In Section 4, we also allow for influences from workmates.

While much of the previous empirical literature on group effects deals with interaction within ethnic networks, our study concerns interaction within geographical areas. This approach is feasible because our data contain neighborhood identification for every individual in Sweden.

There are several characteristics of the Swedish sick-pay insurance system that facilitate the emergence of local benefit-dependency cultures. First, the replacement rates are quite high for a majority of employees (80-90 percent of previous earnings), which is likely to create a strong temptation to “overuse” the system (moral hazard). Second, the administration of the system was quite lax during the period under study, i.e., individuals themselves could to a large extent choose whether to live on sickness benefits. Sickness spells longer than one week requires a doctor’s certificate, but there is strong evidence that doctors rarely turn down requests for such certificates. For instance, Englund (2008) found that doctors were prepared to provide certification in 80 percent of the cases where the doctors themselves believed that sick leave was either not necessary or even harmful to the individual. There is no limit to the number of days that an individual may receive sickness benefits. Moreover, empirical studies are facilitated by the fact that the system is government-run and mandatory, with the same rules applying throughout the country.

The study utilizes a rich data set covering the entire Swedish population, with information on both each individual’s sickness history and various socioeconomic characteristics of the individual. When using this data set to study group effects, we apply four different approaches to deal with various econometric problems connected with the estimation of group effects. The first two approaches rely on rather weak identifying assumptions and are designed to find traces of group effects, without attempting to quantify these effects:

A. We investigate whether the behavior of an individual is influenced by the interaction between the network in his neighborhood and the network at his job.

B. We ask whether individuals in Sweden who have moved from one neighborhood in to another tend to adjust their sickness absence behavior to normal (average) behavior in the new neighborhood.

A more ambitious task is to quantify the magnitude of the group effects. For this purpose, we use two approaches that rely on somewhat stronger identifying assumptions:

C. We study whether immigrants to Sweden (basically refugees) have adjusted their behavior to that of native Swedes in the neighborhood where the immigrants have settled down.

D. We use an instrumental-variable approach, based on the different absence patterns of public-sector and private-sector employees.

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

As is well known, the identification of causal group effects is fraught with statistical difficulties; we will address these difficulties in several different ways in this paper. There is, however, a more basic problem of interpreting such causal group effects. Suppose that the statistical problems have been satisfactorily dealt with, and a significant group influence has been identified. Then the question remains whether the influence reflects the dissemination of *information* or of *social norms* on the individual's behavior.

For instance, Duflo and Saez (2002, 2003) point out that their estimates do not tell how much of the group effects is due to the transmission of information and how much is due to social norms. However, the study by Aizer and Currie (2004) is designed to make such a distinction for the participation in publicly funded maternity care. They assume that mothers who have previously used such care do have information about the availability of the services. The authors therefore argue that the estimated group effects for such mothers reflect social norms, rather than the transmission of information.

Since the Swedish sick-pay insurance system is mandatory and uniform, the rules and the availability are well-known to all individuals in the country.[3] Indeed, in virtually all families, some individual has at least on some occasion visited a physician and utilized the sick-pay insurance system. By contrast, the pension plans studied by Duflo and Saez (2002, 2003) are quite complicated and difficult to digest; thus the dissemination of information is likely to be important in this case. However, the acquisition and interpretation of information about the Swedish sick-pay insurance system is a trivial task. This means that estimates of group effects can hardly be interpreted as the result of the dissemination of information about the availability of sickness benefits. This strengthens our interpretation of group effects as the result of social norms, rather than of information.

A special feature of our study is that it investigates the robustness of the results in a fundamental way. Usually, robustness checks are done by variations in the vector of covariates within the context of one specific analytical approach. We have chosen a more comprehensive robustness check: in addition to varying the vector of covariates, we study group effects by trying several different models (namely approaches A – D mentioned above). In fact, it turns out that variations in the vector of covariates have very small consequences for the estimates of group effects.

[3] Even immigrants are informed about the details of the social insurance system when settling down in Sweden.

2. A First Look at the Data

Our data set combines individual sickness absence data from the Swedish National Insurance Agency with a large number of socioeconomic variables obtained from the LISA database, compiled by Statistics Sweden. In addition to providing information on numerous individual characteristics, the combined data set allows us to identify each individual's neighborhood and workplace. The data consist of an unbalanced panel for the seven-year period 1996-2002. Although the data set covers the entire population in Sweden, we confine our study to private- and public-sector employees in the age group 18-64 (almost 5 million individuals, generating about 25 million observations in the entire panel). The data set includes all spells of sickness absence longer than 14 days, all being paid for by the national insurance system.[4]

When studying local social norms, a first issue is to determine the most relevant geographical domain. Municipalities may be too large for this purpose. We therefore chose to use the so-called Small Area for Market Statistics unit (SAMS) for geographical domains in Sweden.[5] Such areas provide reasonably homogeneous districts based on geographical proximity among inhabitants and similarity in housing.[6] There are 8,951 SAMS in our database, with an average population of 404 persons. In the following, we use the term "neighborhoods" for these areas.

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

[4] The reason why only spells longer than 14 days are included in the data is that the employer pays compensation for shorter spells, and individual data on such spells are not systematically reported. The total average number of paid sick days (including short spells) was about 25 per year during the period under study, as compared to 17.8 in our data set (which does not contain the first 14 days of a spell).
[5] See Statistics Sweden (2005) for a detailed description of this geographical specification.
[6] It turns out that our empirical results are approximately the same regardless of whether we use municipalities, church parishes or the SAMS as the basic geographical unit.

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

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

$$S_{in} = \alpha + X_{in}^{'}\beta + \varepsilon_{in}, \qquad (1)$$

where the *X* vector contains three types of socioeconomic variables: individual characteristics (such as age and education), characteristics of the individual's workplace (such as industry and plant size), and neighborhood characteristics (such as urban/rural, local unemployment, and a local health variable). We have chosen explanatory variables that, in many studies, have turned out to be important for sickness absence. Due to the large number of observations, we have been able to apply a flexible specification of the regression equation, using dummies rather than specific functional forms; see Table A1 in the Appendix for a full list of the variables in the *X* vector.[7]

As expected, the *X* vector explains very little of each individual's behavior, since idiosyncratic factors tend to dominate at the individual level. More surprisingly, the *X* vector also explains very little of the variation in *average* sickness absence, $\overline{S}_n$, across

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

neighborhoods. While the standard deviation of average absence across neighborhoods in the 2002 raw data was 13.2 days, it was almost the same (12.9 days) after controlling for all of the socioeconomic variables in the *X* vector. To find out whether the remaining differences among neighborhoods (the average residuals $\bar{\varepsilon}_n$) are systematic rather than random, we also estimated an equation with neighborhood-specific intercepts α_n:

$$S_{in} = \alpha_n + X_{in}^{'}\beta + \varepsilon_{in} \,. \tag{1'}$$

An *F* test suggests that (1') fits the data significantly better than the original specification (1) with a uniform intercept (*F* = 2.650, implying significance at the one-percent level[8]). To rule out the possibility that this simply reflects fixed unobservable factors, we also estimated (1) and (1') in terms of *changes* in sickness absence. As in the case of levels, the average residuals of changes between 2001 and 2002 across neighborhoods vary systematically, i.e., in a non-random fashion (*F* = 1.370, again implying significance at the one-percent level). Thus, there is systematic local variation in average sickness absence not accounted for by the socioeconomic factors in our *X* vector. As we have seen, this holds not only for levels, but also for changes. Indeed, this result holds for the entire panel, and not only for specific years.

3. Measuring the Effect of Social Interactions

Our basic hypothesis is that these large geographical differences in sickness absence – after controlling for a battery of social and economic variables – are related to differences in local social norms concerning benefit dependency. In other words, we assume that there are local variations in group effects on individual behavior. We then measure group behavior by the average number of sick-absence days in a neighborhood, and we would in principle want to estimate an equation of the following type:

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

$$S_{in} = \alpha + X_{in}'\beta + \gamma \overline{S}_n + \varepsilon_{in}, \tag{2}$$

where $\overline{S}_n$ is the average absence of the neighbors of individual *i*; naturally, we have excluded individual *i* him/herself from the average.

The idea behind equation (2) is that group behavior ($\overline{S}_n$) to some extent reflects prevailing sick-absence norms in the neighborhood, and that individuals tend to conform to such norms. We assume that the individual receives information about the norms when interacting with neighbors and observing their absence behavior. Basically, we refer to the same type of mechanisms as are often assumed in analyses of group effects in the fields of schooling, crime, drug use, unemployment etc.

However, running an OLS regression on (2) is likely to yield an upward-biased estimate of γ. One reason is that it is difficult to distinguish the effects of social interaction from so-called contextual influences (such as unobserved heterogeneity). This difficulty reflects the fact that individuals who live in the same neighborhood are exposed to similar unobserved circumstances, such as environmental factors not included in the X vector. This may create a statistical relation between average behavior and individual behavior that is not the result of social interaction. [9]

One specific example of contextual effects that may be misinterpreted as local social interaction is contagious diseases that hit some neighborhoods but not others. However, the only major epidemic that has hit the Swedish population in recent decades is ordinary influenza. Since sickness absence due to flu symptoms seldom lasts longer than two weeks, this kind of contextual effect is not important in our dataset.[10]

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[9] Two alternative ways of dealing with unobserved heterogeneity are to estimate equation (3) on differences instead of levels, or to allow for individual fixed effects. However, these methods are feasible only if the heterogeneity is fixed. In realty, the factors underlying unobserved heterogeneity may very well change over time.

[10] From this point of view, the fact that our data set only covers spells of absence longer than two weeks may be regarded as an advantage rather than a limitation of the analysis – although from other points of view, a study of shorter spells would, in itself, also be informative.

Another type of contextual effects is that a shock may affect some specific population group (for instance, a particular profession or age group) and that this group happens to be relatively large in certain neighborhoods. To the extent that such effects are observable, they may be taken into account simply by inserting the average characteristics of individuals in the neighborhood, $\overline{X}_n$, into the regression equation (2); we have done so when relevant in our regressions. However, we cannot guarantee that non-observable heterogeneity does not exist in the data – a problem shared with all econometric studies not based on controlled experiments. The most obvious example in our study is perhaps that individuals might self-select into neighborhoods. As always in this type of analysis, it is therefore necessary to make specific identifying assumptions to differentiate between causal group effects and other influences; these assumptions will be specified and discussed later on.

Contextual effects and/or unobserved heterogeneity among individuals are the background to the “reflection problem” mentioned in the introduction. In a naïve regression of equation (2), one would simply obtain an estimate of the relation between the behavior of the representative individual and the behavior of the average individual – i.e., basically the same individual. Indeed, such a regression gives $\hat{\gamma} = 0.8658$, which is significant at the one-percent level, reflecting an upward bias in the estimate. Our adoption of four different approaches, mentioned in the introduction, is intended to avoid such a bias.

4. Approach A: Interaction between Neighborhood and Workplace Networks

One way of analyzing the effects of social interaction without running into the reflection problem may be to ask whether the *strength* of an individual’s network contributes significantly to explaining his behavior. We may assume that social norms and attitudes are transmitted more easily when individuals meet in more than one arena. We therefore ask whether an individual is more influenced by their neighbors if he also meet them at the workplace – i.e., if individuals meet not only during leisure time, but also during working time. [11]

To deal with this issue we estimate the following model:

$$S_{inwt} = \alpha + X_{inwt}^{'}\beta + \nu \cdot (CA_{inwt} \cdot \overline{S}_{nt}) + \lambda_t + \kappa_w + \mu_n + \varphi CA_{inwt} + \varepsilon_{inwt} \,, \qquad (3)$$

where the subscript *w* denotes the workplace. Here, S_{inwt} is the number of days of paid sickness absence (for sick spells longer than two weeks) of individual *i*, living in neighborhood *n* and working at workplace *w*, at time *t*. CA_{inwt} is defined as the fraction of the individual's neighbors who are also his coworkers; it can be regarded as a measure of the additional strength of the network facing individual *inw* at time *t* when he belongs to two different networks. The parameters λ_t, κ_w and μ_n are fixed effects for year, workplace and neighborhood, respectively.[12] Hence we deal with the reflection problem by identifying group effects on the interaction term, controlling for fixed workplace and neighborhood effects.

The fixed effects μ_n and κ_w control for omitted variables in the *X* vector.[13] In addition, equation (3) includes the density (concentration) measure CA_{inwt} separately. This allows us to control for the possibility that the strength of the network in itself may be correlated

[11] It is conceivable that group effects are particularly easily transmitted within homogeneous subgroups of neighbors or coworkers. For example, Duflo and Saez (2003) found that group effects are stronger within subgroups (of the same gender, of the same age, etc.) than for the entire population within a workplace. Bokenblom and Ekblad (2007) also found that inter-individual influences on sickness absence in public-sector workplaces in a Swedish city occurred mainly within gender and age groups.

[12] Equation (3) has basically the same analytical structure as the corresponding equation in Bertrand et al. (2000). In an analysis of the utilization of social assistance ("welfare" in U.S. terminology) among ethnic minorities in the United States, they studied the interaction between language groups and neighborhoods.

[13] The vector X_i in (3) is a subset of the previously used *X* vector, in the sense that neighborhood and work place characteristics have been excluded. The reason is that the neighborhood and workplace variables in *X* become redundant when we enter neighborhood and workplace fixed effects into the regression equation. The network-intensity variable only varies on the neighborhood/workplace level; we therefore adjust the standard errors for clustering within the cells consisting of the intersection of neighborhoods and workplaces (see e.g. Moulton, 1986).

with unobservable characteristics systematically related to the propensity to be absent from work. Our identifying assumption then presumes that there is no correlation between the interaction term $CA_{inwt} \cdot \overline{S}_{nt}$ and any remaining non-observable variables that affect sickness absence, i.e.,

$$E\left(\varepsilon_{inwt} \mid CA_{inwr} \cdot \overline{S}_{nt}, \overline{S}_{nt}, X_{inwt}, \mu_n, \kappa_w, \lambda_t\right) = E\left(\varepsilon_{inwt} \mid \overline{S}_{nt}, X_{inwt}, \mu_n, \kappa_w, \lambda_t\right).$$

Table 1 shows the results from the OLS estimation. The parameter $\hat{\nu}$ is significantly different from zero. Thus, the regression is consistent with our hypothesis that an individual's behavior is more affected by his neighbors if he also meets some of them at his workplace. In other words, we find evidence of social interaction on sickness absence: the strength of networks affects the extent to which average behavior influences individual sickness absence. We calculate the marginal effect with respect to changes in average absence in the neighborhood, i.e., $\partial S_{inw} / \partial \overline{S}_n$; it is easily seen to be equal to $\nu \cdot CA_{inwt}$. This number tells us how an increase in the average absence $\overline{S}_{nt}$ in a neighborhood influences individual absence through the interaction between neighborhood and workplace networks.

The estimates in Table 1 mean that if the average absence in an average Swedish neighborhood increases by ten days, the strength-of-network effect *adds* 0.5 days to the average individual's absence. The fact that the parameter estimate is significant at the one-percent level is a strong indication that group effects actually exist.

However, this approach identifies only a limited aspect of social interaction, namely the *accentuation* of neighborhood effects through the interaction of two networks. Thus, the estimate of ν in equation (1) tells us whether there is an *additional* network effect for individuals who are not only neighbors but also coworkers. The coefficient ν, therefore, represents only a small fraction (an accentuation) of total social interaction within neighborhoods.

Table 1: The strength-of-network effect.

No. of Obs.: 24,449,603 ind.: 4,693,560	R^2		$\hat{v}$		$\partial S_{inw} / \partial \overline{S}_n = \hat{v} \cdot \overline{CA}$	
	0.012	0.0007	2.146*** (0.0273)	2.216*** (0.0273)	0.0502	0.0518
X vector included	Yes	No	Yes	No	Yes	No

Note: *** indicates significance at the one-percent level.
The numbers of observations and individuals in this table are somewhat smaller than the corresponding numbers in subsequent tables. The reason is that for each individual, we deleted the individual himself from the data when computing the averages $\overline{S}_n$. For some neighborhoods, there is only one individual who works in each workplace; these cases therefore do not appear in the regression.

4. Approach B: Movers within Sweden

In the previous section, we dealt with the reflection problem and issues of unobserved heterogeneity by including neighborhood and workplace fixed effects in a regression equation with an interaction term $CA_{inwt} \cdot \overline{S}_{nt}$ as explanatory variables. In this section, we use another approach. We control for the individual's type by looking at changes in absence behavior when individuals move from one neighborhood to another.

More specifically, we investigate whether individuals who have changed neighborhood within Sweden change their absence behavior towards the average behavior in the new neighborhood.[14] As an illustration of the analysis, we first look at individuals who have lived in the new neighborhood for only one year. Denoting the old neighborhood by *m* and the new by *n*, we estimate the following model:

[14] Ichino and Magi (2000) carried out a similar analysis of employees at a large Italian bank who have moved from one branch to another.

$$S_{int}^{mover} - S_{im,t-1}^{mover} = \alpha + \lambda_t + (X_{int}^{'mover} - X_{im,t-1}^{'mover})\beta_1 + (\overline{X}_{int}^{'all} - \overline{X}_{im,t-1}^{'all})\beta_2 + \\ +\eta \cdot (\overline{S}_{n,t-1}^{non-mover} - \overline{S}_{m,t-1}^{non-mover}) + \delta \cdot D_{it} \cdot (\overline{S}_{n,t-1}^{non-mover} - \overline{S}_{m,t-1}^{non-mover}) + \varepsilon_{inmt} \quad (4)$$

where

$$D_{it} \equiv \begin{cases} 1 \;\; if \;\; individual \;\; i \;\; has \;\; moved \;\; at \;\; time \;\; t \;\; to \;\; an \;\; area \;\; with \\ \;\; higher \;\; absensce, \; i.e., if \;\; \overline{S}_{n,t-1}^{non-mover} > \overline{S}_{m,t-1}^{non-mover} \\ 0 \;\; otherwise. \end{cases}$$

We use this specification to investigate whether people who move from neighborhood *m* to neighborhood *n* adjust their behavior in response to the difference in average absence between these two neighborhoods. However, there is no reason to expect that individuals are equally likely to adjust to neighbors with high sickness absence as to neighbors with low. We therefore use two coefficients η and δ to capture such adjustment. If $\eta \neq 0$ and $\delta = 0$ the adjustment is symmetric (the individual adjusts his behavior symmetrically when moving to a neighborhood with higher and lower absence). If, on the other hand, $\delta \neq 0$, the adjustment is asymmetric.

The reflection problem is avoided since we have entered absence for different groups of people on the left-hand side (movers) and right-hand side (non-movers) of the regression equation. Our identifying assumption in this analysis is that people who plan to *change* their absence behavior in the future do not tend to move to neighborhoods with a particular level of average sickness absence. This means that people are assumed to move for a variety of reasons (such as changes in the family, in the job prospects, etc.) but not as a result of expected future changes in their own sickness absence.

Table 2: Movers within Sweden, equation (4)

No. of ind.: 1,551,059 obs.: 2,202,466	R^2	$\hat{\eta}$	$\hat{\delta}$
Including both X and $\bar{X}$	0.0055	-0.012 (0.0122)	0.040** (0.0194)
Inluding X but not $\bar{X}$	0.0054	-0.006 (0.0115)	0.073*** (0.0193)
Inluding neither X nor $\bar{X}$	0.0013	-0.005 (0.0115)	0.092*** (0.0194)

The estimates are reported in Table 2. We note that $\hat{\eta}$ is not significantly different from zero, while $\hat{\delta}$ is. This result is robust regardless of whether we include the X and $\bar{X}$ vectors in the regression or not. This suggests an asymmetric effect in the sense that the individual adjusts his behavior if he moves to a neighborhood with a *higher* average absence, while he makes no such an adjustment (at least not within the time span of one year), when moving to a neighborhood with a *lower* absence. According to these estimates, individuals seem to find it easier to adjust to “bad” habits than to “good” habits.

Clearly, it does not seem likely that individuals make full adjustment to the average behavior in a new neighborhood within a year. We have therefore also estimated equation (4) for movers who have lived in the new neighborhood for two and three years, respectively.[15] These results are given in Table 3.

[15] This means that we have replaced the $t-1$ time index in equation (4) by a $t-k$ ($k = 1, 2, 3$) index.

Table 3: Estimates of equation (4), including both the X and the $\bar{X}$ vector, for movers who have lived in the new neighborhood for one, two and three years.

After one year		After two years		After three years	
Number of ind.: 1,551,059 obs.: 2,202,466	R^2 0.0055 $\hat{\eta}$ -0.012 (0.0122) $\hat{\delta}$ 0.040** (0.0194)	Number of ind.: 1,299,833 obs.: 1,483,347	R^2 0.0076 $\hat{\eta}$ -0.007 (0.0197) $\hat{\delta}$ 0.051* (0.0304)	Number of ind.: 993,972 obs.: 1,014,338	R^2 0.0101 $\hat{\eta}$ -0.008 (0.0279) $\hat{\delta}$ 0.080* (0.0455)

Table 3 shows that the asymmetry remains when analyzing a somewhat longer period of residence than one year. The quantitative effect of the asymmetric effect increases along with the time of residence, although the significance falls. Thus, the adjustment seems to increase over time, but it is still rather weak after three years. This suggests that the adjustment by newcomers to a neighbourhood is a rather prolonged process.

As we have pointed out before, short-term effects shown in Tables 2 and 3 should be regarded only as *evidence* of group effects, rather than the full magnitude of such effects. We now turn to the issue of quantifying the full size of group effects, which requires stronger identifying assumptions than the ones we used sin Sections 4 and 5.

6. Approach C: Immigrants

Quantifying the full size of group effects means estimating the coefficient γ in equation (2), while at the same time dealing with the reflection problem and issues connected with unobserved heterogeneity. Needless to say, the identifying assumptions can always be questioned in empirical analyses based on non-experimental data. It is therefore useful to try alternative approaches, with different identifying assumptions. In this section, we study group effects by asking whether immigrants adjust to the absence behavior of natives in the new country. To highlight this question, we estimate the following equation:

$$S_{int}^{f} = \alpha + \lambda_t + X_{int}^{f\,'}\beta_1 + \overline{X}_{int}^{f\,'}\beta_2 + \gamma\,\overline{S}_{nt}^{s} + \varepsilon_{int}\,. \qquad (5)$$

Here, S_{int}^{f} is the number of sick days of immigrant *i* in neighborhood *n* at time *t*, while $\overline{S}_{nt}^{s}$ is the average number of sick days among native Swedes in that neighborhood. We avoid the reflection problem since the absence variable on the left-hand side refers to a different group of people than the absence variable on the right-hand side. We are thus again able to rely on OLS. We apply the identifying assumption that there is no tendency among immigrants with a high propensity for sickness absence to settle down in neighborhoods where the absence rates among natives are particularly high (“reverse causation”).

Of course, our assumption that immigrants adjust their behavior to that of native Swedes does not mean that immigrants would receive their norms solely through interaction with Swedes. Most immigrants may very well acquire the Swedish norms indirectly, through their interaction with individuals in their own ethnic group – among which some individuals may in turn have been influenced directly by native Swedes.

Since we have data on each individual’s country of origin, we can also investigate whether immigrants with a cultural background similar to that of Swedes tend to adjust more than other immigrants to the behavior of native Swedes. The rationale for this question is that one would expect that such immigrants are particularly likely to interact with Swedes.

As we want to study the transmission of norms to immigrants, it is natural to exclude neighborhoods where immigrants constitute a majority of the population. Indeed, we confine this regression to neighborhoods where the fraction of immigrants is less than 30 percent of the total population.[16] The results are shown in Table 4. To check the robustness of the model, we also report estimates from regressions where we deleted the variable $\overline{X}_{nt}$ from equation (5). In an alternative specification, we replaced $\overline{X}_{nt}$ by a neighborhood fixed effect μ_n, intended to capture unobservable fixed factors in the neighborhood. The estimates are quite robust to these alternative specifications. We confine our comments to the case where the $\overline{X}_{nt}$ vector is included (the middle column of the γ estimates in Table 4).

According to these highly significant estimates, sickness absence among the immigrant group as a whole is about 0.4 days higher in a neighborhood where average absence among Swedes is one day higher than in another neighborhood. This number is an attempted estimate of the total group effect (i.e., the parameter γ in equation (2)). We would expect that this is an underestimation of the group effects for the country's population as a whole, since immigrants are likely to have weaker networks with native Swedes than does the total population. This presumption is consistent with the observation from Table 4 that the estimated group effects are stronger for immigrants from Nordic countries $(\gamma \approx 0.6)$ and EU countries $(\gamma \approx 0.4)$ than for immigrants from other countries with less cultural affinity to Sweden.

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

Table 4: Estimates of γ in equation (5) with (*i*) the $\bar{X}$ vector included, (*ii*) the $\bar{X}$ vector excluded, and (*iii*) the $\bar{X}$ vector replaced by a fixed neighborhood effect.

Region	Number of ind. and obs.	Estimate of γ (*i*)	(*ii*)	(*iii*)
All regions	618,460 ind. 2,756,607 obs.	0.384*** (0.0098)	0.392*** (0.0120)	0.357*** (0.0106)
Nordic countries	193,221 ind. 974,791 obs.	0.672*** (0.0184)	0.602*** (0.0215)	0.562*** (0.0197)
EU (except Nordic countries)	72,067 ind. 323,704 obs.	0.423*** (0.0264)	0.318*** (0.0320)	0.422*** (0.0281)
Europe (except EU)	130,641 ind. 588,651 obs.	0.152*** (0.0220)	0.223*** (0.0269)	0.220*** (0.0244)
Africa	28,924 ind. 110,887 obs.	0.068* (0.0397)	0.166*** (0.0496)	0.160*** (0.0452)
North America	19,886 ind. 81,298 obs.	0.213*** (0.0394)	0.164*** (0.0492)	0.177*** (0.0426)
Latin America	30,158 ind. 126,665 obs.	0.358*** (0.0429)	0.310*** (0.0536)	0.325*** (0.0459)
Asia	136,059 ind. 518,147 obs.	0.188*** (0.0199)	0.306*** (0.0248)	0.141*** (0.220)
Oceania	3,405 ind. 12,951 obs.	0.194** (0.0785)	0.151 (0.0967)	0.270*** (0.0869)
Former Soviet Union	3,894 ind. 18,926 obs.	0.118 (0.1218)	0.291* (0.1547)	0.114 (0.1196)
Including $\bar{X}_{nt}$ vector		No	Yes	No
Including fixed effects μ_n		No	No	Yes

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

Is it possible that the highly significant estimates reported in Table 4 reflect unobservable heterogeneity rather than social interaction, thereby violating our basic identifying assumption? One mechanism behind such heterogeneity could be that immigrants with a particular characteristic that is not included in our X vector have wound up in neighborhoods where the natives have a similar characteristic. Although there are strong exogenous elements in the geographical distribution of immigrants across neighborhoods in Sweden,[17] we cannot fully rule out the possibility that immigrants with a strong propensity to call in sick in fact have settled down in areas with many Swedes of a similar inclination. It does not seem likely that immigrants consciously choose such a strategy; not even native Swedes, who have been living in the country for decades, have information about differences in sickness absence across the country.

There may, however, be indirect mechanisms for such a selection – although it is difficult to find convincing examples of such mechanisms. One conceivable mechanism is selection on neighborhoods based on the labor-market ambitions of individuals, assuming that high ambitions are correlated with low sickness absence. It might be the case that ambitious individuals – immigrants as well as native Swedes – settle down in areas with strong labor markets. As a consequence, both native Swedes and immigrants in such areas may have relatively low absence rates. However, it should be observed that labor markets are much larger areas than neighborhoods. For instance, the large cities of Sweden, where many immigrants live, may each form one labor market, but comprise hundreds – or even thousands – of neighborhoods. This means that selection based on labor markets is not very closely related to selection based on neighborhoods. Thus it does not seem very likely that a selection mechanism based on labor-market ambitions would result in a situation where immigrants with a high propensity to be absent from work tend to settle down in neighborhoods where native Swedes have the same inclination.

[17] Political refugees, who have formed the dominant immigrant category in Sweden in recent decades, have usually been allocated by the authorities to geographical areas with empty (rent-controlled) apartments.

Although the hypothetical selection mechanism discussed above may seem farfetched, it should be taken seriously. In order to assess the importance of permanent unobserved heterogeneity (as opposed to the observed heterogeneity reflected in the $\overline{X}$ vector) we have also estimated equation (5) with fixed neighborhood effects. The estimates from such a regression are reported in column (*iii*) of Table 4. We see that the coefficient estimates are quite insensitive to whether we include fixed neighborhood effects or not. One possible interpretation is that it is unlikely that the estimates of group effects simply reflect unobserved heterogeneity.

In summary, while our estimate of the coefficient $\gamma \approx 0.4$ may suffer from a downward bias (since immigrants may have weaker networks with Swedes than does the population as a whole), there might also be an upward bias (due to a conceivable, but perhaps not very likely, selection of immigrants on neighborhoods).

7. Approach D: Exploiting Inter-Sectoral Differences in Absence Behavior

Another way of estimating γ without running into the reflection problem is to use an IV approach, exploiting the difference in sickness absence between private-sector and public-sector employees. While the average number of days of sickness absence in our data set (spells longer than 14 days) in 2001 was 12.2 for private-sector employees, it was 15.4 for central government employees and 20.3 for municipal employees. There may be several reasons for these differences. The most obvious one is that private employers have stronger incentives than public-sector employers to prevent absence, since it is costly to the former. It could also be the case that workers with preferences for frequent absence tend to self-select into the public sector.

For these reasons, neighborhoods with a large share of public-sector employees are, on average, likely to have a higher work-absence rate than other neighborhoods. We exploit this fact and use the share of public-sector employees as an instrumental variable for the average work-absence level in a neighborhood. The identifying assumption underlying this approach is that the share of public-sector employees in a neighborhood is unrelated to unobserved characteristics affecting individual work-absence behavior; formally, $E(Z_{nt}\varepsilon_{\text{int}}) = 0$, where Z_{nt} is the public sector's share of employment in neighborhood *n* in year *t*.[18] Thus we assume that workers with specific absence behavior do not choose to reside in neighborhoods on the basis of the proportion between public- and private-sector employees in these neighborhoods. In other words, we assume that the different behavior of these two groups of employees is related to the institutional features of the sectors where they work, rather than to unobserved individual differences. We will return to this issue.

Using Z_{nt} as an instrument for average sickness absence in neighborhood *n*, we apply the following IV model, with not only X_{int} but also $\overline{X}_{nt}$ among the covariates,[19] to explain the behavior of individual private-sector employees:

$$\begin{aligned} \overline{S}_{nt} &= a + k_t + X_{int}'b_1 + \overline{X}_{nt}'b_2 + cZ_{nt} + e_{nt} \\ S_{int}^{priv} &= \alpha + \lambda_t + X_{int}'\beta_1 + \overline{X}_{int}'\beta_2 + \gamma\,\hat{\overline{S}}_{nt} + \varepsilon_{int}. \end{aligned} \tag{6}$$

Conversely, we use $1 - Z_{nt}$ as an instrument to estimate the effects on individual public-sector employees, S_{int}^{publ}:

$$\begin{aligned} \overline{S}_{nt} &= a + k_t + X_{int}'b_1 + \overline{X}_{nt}'b_2 + c(1 - Z_{nt}) + e_{nt} \\ S_{int}^{publ} &= \alpha + \lambda_t + X_{int}'\beta_1 + \overline{X}_{int}'\beta_2 + \gamma\,\hat{\overline{S}}_{nt} + \varepsilon_{int}. \end{aligned} \tag{6'}$$

We also estimate a system like (6) and (6') for the entire population, i.e., without superscript *priv* or *publ* on the *S* in the second equation.

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[18] More exactly, *Z* is the ratio of the number of public-sector employees to the sum of public- and private-sector employees.

[19] The $\overline{X}$ vector is defined as the neighborhood average of all covariates, with the exception of the age dummies. For computational reasons, we have reduced the number of variables by using the average age in the neighborhood (1 variable), rather the share of each age group in the neighborhood (46 variables).

However, before pursuing the IV analysis, let us (without estimating γ) investigate whether there are any traces of group effects in the neighborhoods. More specifically, we ask whether the absence of a private-sector employee is higher if he has many neighbors who work in the public sector, and vice versa. We therefore study the reduced form of the model defined in equations (6), i.e.,

$$S_{int}^{priv} = \alpha + \lambda_t + X_{int}^{'}\beta_1 + \overline{X}_{int}^{'}\beta_2 + \mu \cdot Z_{nt} + \varepsilon_{int}\,, \tag{7}$$

where S_{int}^{priv} is the sickness absence of individual *i* in the private sector in year *t*. We expect the estimate of μ to be positive.

Conversely, we ask whether a public-sector employee tends to be less absent from work if he lives in a neighborhood where there are many private-sector employees:

$$S_{int}^{publ} = \alpha + \lambda_t + X_{int}^{'}\beta_1 + \overline{X}_{int}^{'}\beta_2 + \mu \cdot (1 - Z_{nt}) + \varepsilon_{int} \tag{7'}$$

As a robustness check, we also ran all regressions without an $\overline{X}$ vector on the right-hand side. It turns out that the coefficients then change only marginally. When commenting on the estimation results, we focus on the case where the $\overline{X}$ is included. The results of these estimates are shown in the fourth column of Table 5.

As expected, a higher share of public-sector employees in a neighborhood is associated with higher sickness absence among private-sector employees in that neighborhood. The number 0.045 in Table 5 means that if the share of public-sector employees is 10 percentage points higher in one neighborhood than in another, then sickness absence among the privately employed is approximately 0.45 days higher in the first neighborhood. Similarly, if the share of private employees in one neighborhood is 10 percentage points higher in than in another, the number of sick days among public-sector employees is 0.37 days lower.

(Table 5)

For completeness, we have also carried out reduced-form estimates based on the entire population (private- as well as public-sector employees). The number 0.041 means that if the share of public-sector employees in a neighborhood is 10 percentage points higher than in another, the average number of absence days among all employees is 0.41 days higher.

Note that the estimate of μ is only intended to reflect the influence on sickness absence of the proportion of public-sector employees in a neighborhood; it does not provide a quantification of how much average behavior influences individual behavior. To obtain such quantification, an estimate of γ in the full IV model of equations (6) and (6') is required. The resulting estimates of these equations are shown in the fifth and sixth columns of Table 5.

Table 5: Estimates of μ in (7) and (7'), and of γ in (6) and (6')

Population	Number of individuals and observations	Regressor	Reduced form: μ in equation (7)		First step in IV regression: c in eq. (6)		IV estimate: γ in equation (6)	
All who work in private sector	2,839,410 ind. 14,556,753 obs.	Share of population in neighborhood n that works in public sector (Z_{nt})	0.038*** (0.0013) $R^2 = 0.020$	0.045*** (0.0017) $R^2 = 0.021$	6.670*** (0.0116) $R^2 = 0.499$	6.384*** (0.0128) $R^2 = 0.636$	0.581*** (0.0199) $R^2 = 0.021$	0.698*** (0.0274) $R^2 = 0.021$
All who work in public sector	1,956,740 ind. 10,502,405 obs.	Share of population in neighborhood n that works in private sector ($1-Z_{nt}$)	-0.044*** (0.0017) $R^2 = 0.021$	-0.037*** (0.0023) $R^2 = 0.027$	-5.752*** (0.0123) $R^2 = 0.512$	-6.232*** (0.0139) $R^2 = 0.635$	0.762*** (0.0302) $R^2 = 0.274$	0.595*** (0.0368) $R^2 = 0.279$
All employees	4,796,150 ind. 25,059,158 obs.	Share of population in neighborhood n that works in public sector (Z_{nt})	0.042*** (0.0011) $R^2 = 0.024$	0.041*** (0.0014) $R^2 = 0.025$	6.222*** (0.0084) $R^2 = 0.503$	6.254*** (0.0094) $R^2 = 0.634$	0.672*** (0.0173) $R^2 = 0.025$	0.654*** (0.0225) $R^2 = 0.025$
Including $\overline{X}_{nt}$ vector			No	Yes	No	Yes	No	Yes

*** indicates significance at the 1 percent level.

Let us first look at the last column in Table 5, with the estimate γ = 0.698. According to this estimate, a typical private-sector employee has 0.698 more sick days if he lives in a neighborhood where the *average* number of sick days is one day higher than in another neighborhood. Similarly, a typical public-sector employee would have 0.595 more sick days in the corresponding situation. For the entire population (private plus public sectors), a person who lives in a neighborhood with an average that is one day higher than in another neighborhood would have 0.654 more sick days.

For these estimates to be credible, it is necessary that the instrument is good in the sense that not only $Cov(Z_n, e_{nt}) = 0$, but also $Cov(Z_n, \overline{S}_n) \neq 0$. The latter condition is clearly satisfied. The first-step estimates of equation (3), reported in column five, indicate that Z is a very strong instrument for the average absence in a neighborhood; the standard deviations are minuscule relative to the coefficients.

It is more difficult to know whether the condition $Cov(Z_n, e_{nt}) = 0$ is satisfied. As pointed out earlier, our estimates are based on the identifying assumption that there is no tendency among individuals with a high innate propensity for sickness absence to self-select into neighborhoods with many public-sector employees. As an objection to this assumption, one might speculate that our results at least partly reflect selection mechanisms, although it is difficult to conceive of plausible mechanisms for such selection. Theoretically, one mechanism might be that the availability of physicians is higher in neighborhoods with many public-sector employees; as a result, the cost for the individual of obtaining a doctor's certificate would be lower in such a neighborhood. However, this is not a plausible mechanism. The availability of medical care is very high all over Sweden, and it would be far-fetched to assume that physicians are disproportionally allocated to areas with many public-sector employees. It is equally implausible that public-sector employees are particularly prone to settle down in neighborhoods with good access to physicians.

Another conceivable selection mechanism would be based on a negative relation between career ambitions and the propensity to take sick leave. The story would be that individuals with modest ambitions often choose to work in the public sector and that low-ambition individuals in the private sector tend to settle down in areas where they can find neighbors with similar attitudes. In principle, this would create unobserved heterogeneity (in terms of career ambitions) which could violate our identifying assumption. To shed light on this possibility, we assume that the education level is a reasonable proxy for an individual's career ambitions. We therefore estimate two "placebo" equations that are similar to (7) and (7'), but where the dependent variable now is education, rather than sickness absence:[20]

$$EDU_{int}^{priv} = \alpha + \lambda_t + X_{int}^{'}\beta_1 + \overline{X}_{int}^{'}\beta_2 + \mu_1 \cdot Z_{nt} + \varepsilon_{int} \tag{7a}$$

and

$$EDU_{int}^{publ} = \alpha + \lambda_t + X_{int}^{'}\beta_1 + \overline{X}_{int}^{'}\beta_2 + \mu_2 \cdot (1 - Z_{nt}) + \varepsilon_{int} \, . \tag{7a'}$$

If low-ambition individuals (as defined here) would self-select into neighborhoods with many public-sector employees, the coefficient μ_1 should be negative, and the coefficient μ_2 positive. However, it turns out to be just the opposite: $\mu_1 = 0.113^{***}$ and $\mu_2 = -0.198^{***}$. Thus, our "placebo experiment" suggests that the estimates of γ in Table 5 are not biased upward by unobserved heterogeneity with respect to career ambitions. Rather, such heterogeneity might bias the estimates downward. This result holds regardless of whether we include $\overline{X}$ as a regressor or not (see Table A2 in the Appendix). Our conclusion about group effects on sick-absence behavior is therefore not an artifact due to selection based on heterogeneous career ambitions. The estimates may rather understate the size of group effects.

Even if it is hard to conceive of plausible selection mechanisms giving an upward bias to our estimates of γ in Table 5, there may be unobserved "contextual" variation across

[20] Needless to say, this formulation removes the individual's education has from his *X* vector.

neighborhoods (i.e., factors not included in our vector of observables). It may be tempting to control for such factors by introducing neighborhood dummies, for instance by replacing α with α_n in equations (6)-(7). However, such dummies would absorb the influence of our instrument Z_n and hence invalidate our notion of permanent benefit cultures in some neighborhoods and not in others.[21]

To summarize, our analysis yields estimates of γ in the interval 0.6 – 0.7. These estimates are surprisingly similar to the estimate of γ in Section 6 of 0.6 for immigrants from the Nordic countries. However, to the extent that we have not been able to solve the problem of unobserved heterogeneity (mainly self-selection of individuals on neighborhoods) these estimates would be biased upwards. However, it should be noted that the conceivable selection mechanisms are quite different in the two cases. In the analysis of Section 6, an upward bias may occur if immigrants with particular labor-market ambitions would settle down in *neighborhoods* (not just labor-market regions) in which native Swedes have the same ambitions, and labor-market ambition is correlated with sickness-absence behavior. In the analysis of Section 7, an upward bias may occur if private-sector employees with a particularly strong inclination to call sick would settle down in neighborhoods with many public sector employees – although our placebo test of this possibility did not point in this direction.

8. Concluding Remarks

[21] .

We have used four different approaches to trace neighborhood effects on absence behavior. They all yield results that are consistent with our hypothesis that such effects do exist. Each approach relies on a specific identifying assumption:

A. The model of interaction between neighborhood networks and workplace networks in Section 4 relies on the assumption that there is no correlation between unobservable variables and the term for network interaction.

B. The analysis of movers within Sweden in Section 5 relies on the assumption that individuals who expect to increase (decrease) their absence do not choose to move to neighborhoods with a high (low) average absence rates.

C. Our analysis of immigrants in Section 6 relies on the assumption that immigrants with a high propensity to be absent from work have not settled down (as a result of administrative discretion or self-selection) in neighborhoods with particularly high absence rates among native Swedes.

D. The private- vs. public-sector model in Section 7 relies on the assumption that private-sector employees with a high propensity to call sick do not self-select into neighborhoods with a high share of public-sector employees.

We have tried to answer two questions: Is there evidence of group effects on individual sickness absence? And if so: What is the magnitude of such effects? As for the former question, all four approaches point to the existence of such effects. Since, in particular, approaches A and B rely on rather weak identifying assumptions, we may conclude that group effects do exist in this context. However, a limitation of these two approaches is that they do not deal with the magnitude of the *full* effects. By contrast, approaches C and D were designed to estimate the full effects, and both provide quantitatively similar estimates. Approach C suggests that one day longer average absence leads to about 0.4

days longer absence for the representative individual (although the number is 0.6 for immigrants from the Nordic countries), while the corresponding estimate in approach D is about 0.6 days. This means that the "social multiplier" $(1/(1-\gamma))$ in our data set is in the interval 1.7 – 2.5.

Naturally, there is no reason to expect that social multipliers are of the same magnitudes for all types of behavior, in every country, and at every point in time. Nevertheless, it is interesting to note that surprisingly similar multipliers have been estimated in the U.S. for such different areas as school achievements due to interaction among room mates, criminal behavior, and human capital spillovers. Except for one outlier (= 8.2), a survey by Glaeser et al. (2003) reports social multipliers in the interval 1.4 – 2.2, to compare with our estimates 1.7 – 2.5.

Previous research has shown that coinsurance and controls, in a wide sense, affect the utilization of social programs. Our study shows that such policy measures also have indirect long-term effects through norms and social interaction in society. This is important to consider when designing a well-functioning social insurance program.

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Appendix

Table A1: Explanatory variables in the *X* vector

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

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

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

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