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ABSTRACT

Gender Differences in Shirking: Monitoring or Social Preferences? Evidence from a Field Experiment*

This paper studies gender differences in the extent to which social preferences affect workers' shirking decisions. Using exogenous variation in work absence induced by a randomized field experiment that increased treated workers' absence, we find that also non-treated workers increased their absence as a response. Furthermore, we find that male workers react more strongly to decreased monitoring, but no significant gender difference in the extent to which workers are influenced by peers. However, our results suggest significant heterogeneity in the degree of influence that male and female workers exert on each other: conditional on the potential exposure to same-sex co-workers, men are only affected by their male peers, and women are only affected by their female peers.

JEL Classification: C23, C93, J24

Keywords: peer effects, employer-employee data, social preferences, randomized field experiment

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1 Introduction

Recent advances in the economics experimental literature has documented gender differences along various dimensions of social preferences and psychological attributes. For example, empirical evidence suggest that women are, compared to men, more averse to risk and competition, and more other-regarding and reciprocal (see e.g. Bertrand 2011, Croson & Gneezy 2009, for overviews of the literature). Differences in psychological traits and social mindedness are often hypothesized to explain observed gender differences in consumption and investment behavior, as well as differences in the labor market. However, the empirical evidence on disparities in attributes and social preferences between the genders is most often based on laboratory experiments. It is still largely an open question whether evidence from the lab generalizes to economic behavior in real markets (Bertrand 2011).

This paper contributes to the literature on gender differences in social preferences by studying the extent to which social incentives determine productivity behavior of male and female workers. Specifically, we study whether the responsiveness to peers in individual shirking behavior differs between male and female workers, and whether individuals are influenced to the same extent by co-workers of their own gender as by those of the opposite sex.

We use exogenous variation in co-workers' absence induced by a large scale social experiment that altered the incentives for short-term work absence through decreased monitoring for nearly half of all workers in Gothenburg, the second largest city in Sweden.[1] Before the experiment, workers were required to present a doctor's certificate on the 8th day of a sickness absence spell in order to continue receiving temporary benefits for further leave. For individuals assigned to the treatment group, the monitoring-free period was extended to the 15th day of an absence spell. Thus, treated workers could be on leave with benefits at their own discretion for 14 days instead of 7, whereas the control group faced the usual restriction of 7 days of non-monitored absence. The experiment ran for 6 months; from July through December of 1988.

While peer effects can arise due to nonsocial spillovers, such as information sharing and externalities, the experiment provides a setting in which peer effects are likely to be informative of the presence of social preferences in the workplace. First, information sharing is an unlikely channel for peer effects in our context; the experiment was preceded by a massive information campaign making both the experimental design and, if not previously known to workers, the rules of the sickness insurance clear. Second, the experiment is not likely to have altered the health of workers, as it only decreased the *monitoring* of absenteeism during six months. Moreover, in line with Hesselius et al. (2009, 2013) who study peer effects of the same experiment, our results do not lend support to peer effects arising due to health

[1] Sickness absence is determined by workers' health status, but solely considering health is not sufficient to explain the large variation in sickness absence within and across firms. Economists have also stressed the importance of economic incentives and several studies document that workers adjust their absence levels to the generosity of the sickness insurance (see e.g. Johansson & Palme 2005, Ziebarth & Karlsson 2013). Recently, some studies have shown that sickness absence is also influenced by co-workers' absence levels (Ichino & Maggi 2000, Hesselius et al. 2009, 2013) and that social interactions thus are an important determinant of worker absenteeism.

spillovers. Thus, in the absence of social preferences, workers should not respond to their co-workers' behavior in their decision to be absent from work. Hesselius et al. (2009, 2013) conclude that the positive peer effects on absenteeism found in their respective studies were consistent with preferences for fairness or reciprocity.

The experiment also provides a close to ideal setting in which to identify peer effects. Identifying social interactions has proven to be difficult due to the well known problems of endogenous group membership, and reverse causality. The latter arises because each peer group member is simultaneously affecting every other group member (Manski 1993). A commonly used strategy in the previous literature to overcome these identification issues has been to use exogenous variation in peer group membership. However, as argued by Angrist (2013), a more compelling strategy to provide evidence on the nature of peer effects is to use randomized research designs that manipulate peer characteristics in a manner unrelated to individual characteristics. Using variation in co-workers' absence induced by the experiment allows us to address the severe identification problems in the latter manner. First, treatment was randomized based on birth date: workers born on an even date were assigned to the treatment group, and workers born on an uneven date were assigned to the control group. The randomized assignment directly addresses the problem of endogenous group membership since it balances all other determinants of work absence. The reverse causality problem can be addressed because, within each workplace, treatment was assigned to only a subset of employees by virtue of the randomization. The experiment thus altered the incentives for the treatment group, leaving the non-treated workers' incentives unchanged. The response among the non-treated, then, provides information about how the reference group affects individual behavior, and not the other way around.[2]

Our analysis provides four main findings. First, consistent with Hartman et al. (2013), we find that the decreased monitoring significantly increased non-monitored absence among the treated workers. Second, in line with Hesselius et al. (2009, 2013), we find significantly positive peer effects in shirking; non-treated workers are estimated to increase their non-monitored absence as a response to being exposed to treated peers.

Third, we find that male workers react more strongly to the decreased monitoring compared to female workers; there is a larger positive effect of being assigned to treatment on non-monitored absence among male workers. Women's shirking behavior, on the other hand, seems slightly more responsive to peers compared to that of men's shirking. This could potentially imply that women are more other-regarding than men: while male workers take the opportunity to increase absence when monitoring decreases, women look more to their surrounding co-workers' behavior when deciding whether to shirk or not. Interestingly, however, we find significant heterogeneity in the degree of influence that male and female workers exert on each other: men are only affected by their male peers, and women are only affected by their female peers. In fact, when we decompose the effect of the fraction treated peers into fractions of male and female treated peers, respectively, there is no significant

[2]This "partial population intervention" approach was outlined by Moffitt (2001) and has been used by e.g. Lalive & Cattaneo (2009) to study social interaction effects schooling attendance in Mexico's PROGRESA, and by Dahl et al. (2012) to study peer effects in paternity leave in Norway, exploiting reforms in the parental leave system that altered the price of leave-taking for some fathers but not for others.

difference between the effect of peers on male and female workers' absence. Instead, the entire peer effect among men is driven by the effect of male co-workers, and vice versa for women. These results hold true even as we control for the fraction of women at the workplace, industry affiliation, as well as dummies taking into account both the field and level of education. The latter is likely to take into account a large part of the variation in occupations held by men and women. Hence, the stronger influence of same-sex co-workers cannot be explained by gender-segregated workplaces. Rather, our results reflect the influence that (fe)male co-workers exert on each other conditional on the potential exposure to same-sex colleagues.

The paper contributes to two strands of literature. First, we contribute to the literature on gender differences in social preferences by studying if these matter outside the laboratory. The body of work from laboratory experiments has so far provided mixed evidence. Studies on reciprocity and fairness sometimes show that women are more trusting than men and sometimes less. In their review of the experimental literature, Croson & Gneezy (2009) hypothesize that this variance is explained by a differential sensitivity of men and women to the social conditions of the experiment. They further argue that small differences in experimental design and implementation can affect these social conditions, leading women to appear more other-regarding in some experiments and less other-regarding in others. They conclude that women are neither more nor less socially oriented, but that their social preferences are more malleable. Our results are in line with the result in Croson & Gneezy (2009) in that women do not seem to be more other-regarding than men. However, our findings cast some doubt on the hypothesis that women's social preferences are more malleable: both male and female workers care about their social context when this is defined by worker similarity. Thus, women's decisions do not seem to be more situationally specific than men's in our setting.

Second, our findings also contribute to the emerging literature on social determinants of worker productivity. Bandiera et al. (2005, 2010) exploit data from a fruit picking farm in the UK and study whether workers have social preferences, both in settings where worker effort imposes an externality on other workers, and in cases where there are no externalities. In the former, they find that the productivity of the average worker is higher under piece rates than under relative incentives, under which worker effort imposes an externality on others' payoffs. They find that this is due to workers partially internalizing the negative externality. In the case without externalities, the authors find that a given worker's productivity is higher when she works alongside friends who are more able than her, and lower when she works with friends who are less able. Mas & Moretti (2009) study peer effects in the workplace and investigate whether, how, and why the productivity of a worker depends on the productivity of co-workers in the same team using data from a large supermarket chain in the US. They find strong evidence of positive productivity spillovers from the introduction of highly productive personnel into a shift. While this body of work examines social preferences as determinants of worker productivity on the intensive margin, the evidence provided in the present paper shows that social incentives also affect worker productivity on the extensive margin.

The rest of the paper is organized as follows. The next section describes the Swedish sickness insurance and the experimental design. Section 3 briefly discusses how to interpret the effect of treatment and peer effects in the experiment, Section 4 presents the data, identifying strategy, and empirical specifications. Section 5 present the results, and Section 6 concludes the paper.

2 The Swedish sickness insurance and experimental design

2.1 The sickness insurance system

The sickness insurance in Sweden is compulsory and covers all workers, unemployed individuals and students. It is financed through a proportional pay-roll tax and replaces individuals' foregone earnings due to temporary illness. In an international context the replacement levels are rather generous. In 1988, the year in which the experiment took place, the benefit level for most workers was set to 90 percent of previous earnings, up to an inflation-adjusted cap. In addition to the public insurance, most Swedish workers are covered by top-up sickness insurance regulated in agreements between the unions and employers' confederations, which generally covers 10 percent of the foregone earnings. The total compensation for work absence due to temporary illness could thus be as high as 100 percent.

The public sickness insurance does not include limits to the duration of sickness benefit payments, or to how often benefits can be claimed.[3] While benefit payments are generous, the monitoring is lax. A sickness absence spell starts when the worker calls the public insurance office and the employer to report sick. On the 8th day of the sickness absence spell, the worker must confirm eligibility status in order to be entitled to *continued* sickness absence by presenting a medical certificate that proves reduced work capacity. The medical certificate is reviewed by the public insurance office, after which further sick leave is either declined or approved. In practice, caseworkers at the public insurance office rarely turn down requests for certificates. Of course, some rules make it possible for the caseworkers to monitor more strictly. When abuse is suspected they could, for instance, visit the claimant's home. Claimants who have been on sickness absence too frequently in the past may be asked to provide a doctor's certificate from day one of the absence spell. Moreover, a new absence spell starting within five working days of the first spell is viewed as a continuation of the first spell, making it impossible to e.g. report sick every Monday without ever visiting a doctor. Individuals with chronic illnesses, on the other hand, need not verify their eligibility status each time illness prevents them from going to work. Given the rather high benefit level and the lax monitoring, it is not surprising that ex-post moral hazard in the Swedish sickness insurance system is found to be high (see e.g. Johansson & Palme 1996, 2002, 2005, Henrekson & Persson 2004, for empirical evidence).

[3]Such limits are in place today. However, in this section we describe the rules that applied at the time of the experiment.

2.2 The experiment

In the second half of 1988, the regional social insurance board in the municipality of Gothenburg, which is the second largest city in Sweden, performed a social experiment that altered the timing of the requirement for a medical certificate.[4] The treatment group, which was randomly assigned, was allowed to be on temporary sickness absence for 14 days before having to present a medical certificate in order to continue their absence spell. The control group faced the usual restriction of 7 days of non-monitored sickness absence. Assignment to treatment was based on individuals' date of birth: individuals born on an even date were assigned to the treatment group, and individuals born on an uneven date were assigned to the control group. For an individual to be eligible for the experiment, they had to reside in Gothenburg municipality.

The arguments put forth by the insurance agency for running the experiment were based on the belief that extending the monitoring-free period would decrease costs and reduce work absence. The main argument was that, with the 14-day restriction, unnecessary visits to medical doctors could be avoided, which would cut costs not only for the worker, but also for the public health care system. The insurance agency also believed that medical doctors routinely prescribed longer absences than necessary. With an extended certificate-free period, many individuals would have time to return to work before a medical certificate was needed, and thus individual and public costs would be reduced.

The experiment was running during the second half of 1988 and, in addition to the social insurance staff, all employers and medical centres were informed before or during the experiment. Thus, the experiment was non-blind, and a massive information campaign also preceded the experiment including mass-media coverage and distribution of pamphlets and posters at workplaces. Brief information about the experiment was also written on the form which every insured worker reporting sick had to fill in and send to the insurance office to receive sickness benefits.

The existing evaluation of the experiment shows that absence spell durations increased, on average, substantially among the treated compared to the control group. Hartman et al. (2013) estimated that average absence duration in the treatment group increased by 6.6 percent. They also report differential treatment effects between women and men, where men were found to prolong their work absence spells substantially more than women.

3 Decreased monitoring, shirking and social interactions

The sick-pay that workers receive is paid by the Swedish government, which means that for employers, the only cost of worker absenteeism is the cost of finding and hiring replacement workers and/or foregone productivity.[5] In general, an employer in Sweden cannot fire a

[4]The experiment was also conducted in Jämtland, a large and sparsely populated region in the north of Sweden. Here, we only analyze data from the Gothenburg experiment.

[5]In the current system, however, employers are obligated to pay sick-pay for the first 14 day of an employee's absence spell, after which governmental benefits are paid for continued absence. However, the worker must still present a doctor's certificate on the 8th day to receive continued sick-pay and benefits.

worker for shirking. The only valid reason for laying off a worker is if the worker has engaged in illegal activities, such as working during his or her sickness absence. Both these facts imply that the incentives for the employer to monitor employees' sickness absence are low. Given the high level of workers' discretion, we interpret a prolonged absence due to the decreased monitoring as a shirking effect.

To study whether there are peer effects in shirking behavior, we focus on the non-treated workers and interpret a potential increase in the work absence among the non-treated in response to treated peers as evidence of peer effects. The argument behind this interpretation is that, if workers have social preferences, they care about the work absence of their peers in their own decision to be absent from work. Of course, a positive spill-over effect can also be the result of nonsocial spill-overs. For example, if treated workers increase their absence, it is possible that presenteeism decreases, such that the remaining workers are less exposed to ill co-workers. In this case, we would expect to find negative effects on absence among the non-treated. However, if treated workers increase their absence due to shirking, this is not a likely scenario. Another possible scenario is that *negative* externalities arise. If an increased absence among the treated shifts the workload to other workers, the latter must increase their work effort. In turn, this might lead to increased stress and thereby illness, which could lead to an increased absence also for the non-treated.

A second possible explanation of a positive peer effect that is not the result of social preferences is joint leisure: co-workers might use the sickness absence to enjoy leisure time together. Evidence provided in Hesselius et al. (2009, 2013), who study social interaction effects in the Gothenburg experiment, do not support the joint leisure or health externality hypothesis. Rather, their evidence suggest that the positive spill-over effects found among the non-treated are consistent with fairness or reciprocity concerns being the main channel. If workers care about fairness, the non-treated workers could - as a response to an expected increase in shirking behavior among their peers - increase their own absence in order to get the same amount of leisure as their treated peers. Alternatively, non-treated workers might feel that they are being unfairly treated by the sickness insurance agency and, as a consequence, increase their work absence.

4 Identifying strategy and Data

4.1 Identifying strategy

Identifying social interaction effects has proven to be difficult due to the problems of reflection, correlated unobservables and endogenous group memberhsip (Manski 1993). The reverse causality problem (reflection) arises because person A's actions affect the actions of person B, and vice versa. As illustrated by Moffitt (2001), suppose we have $g = 1, ..., G$ groups with two individuals $i = A$ and B in each group. Let y_{ig} be the outcome variable of interest for individual i in group g, let x_{ig} be individual socioeconomic characteristics of individual i in group g, and let ϵ_{ig} be an unobservable and assume the structure to be:

$$y_{Ag} = \alpha_g + \theta_1 x_{Ag} + \theta_2 y_{Bg} + \theta_3 x_{Bg} + \epsilon_{Ag} \quad (1)$$

$$y_{Bg} = \alpha_g + \theta_1 x_{Bg} + \theta_2 y_{Ag} + \theta_3 x_{Ag} + \epsilon_{Bg} \tag{2}$$

The social interaction effects are represented by the parameters θ_2 (endogenous social interaction effect)[6] and θ_3 (the exogenous social interaction effect). Manski (1993) shows that the parameters in (1) and (2) are not identified. Under the assumptions that ϵ_{Ag} and ϵ_{Bg} are independent to both x_{Ag} and x_{Bg} and of no group sorting (i.e., $E(\alpha_g y_{ig}) = 0$)), it is easy to show the existence of social interactions *in general*. The coefficients on the other individuals' x in the reduced from indicates whether any type of social interaction is present, but endogenous social interactions cannot be distinguished from exogenous social interactions. In addition to the reverse causality problem, however, there is also the potential problem of sorting (unobservables). In the presence of unobservables, even the weak form of identification obtained from the reduced form, i.e., of the existence of *any* social interactions, is lost.

To overcome these identification problems, we study the influence of co-workers by exploiting variation in the incentives for work absence for a subset of employees at workplaces, induced by a randomized social experiment (see Moffitt 2001). Let D_{ig} denote treatment, where $D_{ig} = 1$ if individual i in group g is eligible for treatment and $D_{ig} = 0$ otherwise. Moreover, treatment is randomly allocated to a subset of each group such that $0 < \overline{D}_g < 1$. In the example above, suppose that individual A is randomly (independently of α_g) assigned to receive treatment, whereas individual B is not. Equation 1 now becomes:

$$y_{Ag} = \alpha_g + \theta_1 x_{Ag} + \theta_2 y_{Bg} + \theta_3 x_{Bg} + \theta_4 D_{Ag} + \epsilon_{Ag} \tag{3}$$

The absence of D_{Ag} in Equation (2) allows all parameters in the model to be identified. Thus, there exists one exogenous variable that affects A directly, but affects the other individual only through the endogenous social interaction. The identifying assumption is that individual B is not directly influenced by D_{Ag}. If individual B, however, knows that individual A is treated (differently) then he or she may also respond to the assignment directly. This response may be due to social preferences like e.g. envy or preferences for fairness. The implication is then that the exclusion restriction is violated.

Since the experiment was known by individuals living in Gothenburg, it is not unlikely that there is an effect of the peers' assignment to treatment in itself on the non-treated, which is why we do not aim at estimating endogenous social interactions. The experiment itself is, however, very useful in identifying social behavior effects using a reduced form model. The intuition is that if treatment is randomly assigned to a subset in a network, we can explore whether the untreated individuals in the network change their behavior. The response among the non-treated gives us information on how the reference group affects individual outcomes, and not the other way around. In the absence of social behavior, the non-treated should be unaffected by the fraction treated in their peer group.

[6]The parameter θ_2 can be given a structural interpretation if we add some assumptions on the individuals' optimizing behavior. If we assume that there is a social cost involved when deviating from a work norm and some further, quite restrictive, assumptions on e.g. rational expectations, θ_2 measures the social norm effect (Brock & Durlauf 2001).

4.2 Data

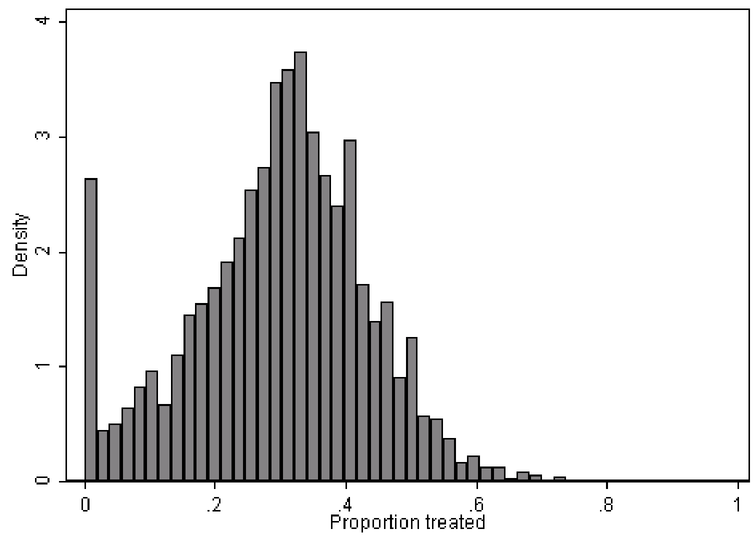
The analysis is based on data from a set of administrative registers maintained by Statistics Sweden. In addition to a set of background characteristics, the data contains information on start- and end-dates of all absence spells during 1987 and 1988. We also observe the workplace where the individual is employed.[7] We start by constructing a matched employer-employee data set to obtain information on individual- and workplace characteristics. Since eligibility for the experiment was conditioned on residence in Gothenburg municipality, we restrict attention to individuals who live in Gothenburg in the empirical analysis. Thus, while commuting co-workers are included when calculating workplace average characteristics, commuting workers (who live outside Gothenburg) are not included in the estimation sample. Moreover, we focus on individuals working at workplaces with 10-100 employees, as social interactions are likely to be more prevalent in small- to medium sized workplaces. Our main outcome variables are the number of days spent on sick leave spells that are shorter than 15 or 8 days, which correspond to *non-monitored* absence for treated and non-treated workers, respectively.

Figure 1 graphs the distribution of the proportion treated employees for workplaces at which individuals in our analysis sample are employed. There is considerable variation in the fraction of treated workers between workplaces. The average workplace has about 30 percent treated workers. The variation in the fraction treated comes from the random assignment of treatment, but also from the number of commuting workers; recall that eligibility status for the experiment was conditioned on residence in Gothenburg municipality, so the mass point at zero treated workers stems from employees who live outside the experiment region. Similarly, individuals can also commute from Gothenburg to bordering municipalities, which means that some eligible workers have employments at workplaces located in bordering municipalities where the share of treated workers will be low. The commuting patterns can be seen in Figure A1 in the Appendix, where the upper graph shows the proportion of individuals working in Gothenburg as a function of the kilometer distance between the residence neighborhood and Gothenburg city center. 80 percent of workers residing in central Gothenburg work in Gothenburg. This picture is corroborated in the middle graph of Figure A1, which shows the proportion treated co-workers to the individuals in our study sample, as a function of the kilometer distance between residence neighborhood and Gothenburg city center. The graph shows that individuals living outside Gothenburg municipality (i.e., about 20 kilometers and further away from the city center) have some treated co-workers. The lower graph depicts the proportion assigned to treatment, and shows that workers living outside Gothenburg (further than 20 kilometers away) are never assigned to treatment, whereas about 50 percent of those living in the city center have been assigned to the treatment group.

Table A1 in the Appendix depicts the means and standard deviations of individual- and workplace characteristics by treatment status, for all workers residing in Gothenburg and employed at workplaces with 10-100 employees. The treatment group exhibits, on average, more days on sickness absence during the Fall of 1988 (the experiment period) compared

[7] A few individuals have multiple workplaces, but for simplicity we assume that the workplace from which the highest yearly earnings are received is also the main arena for co-worker interactions.

Figure 1: Distribution of the fraction treated workers at workplaces with 10-100 employees.



to the control group, with a difference of 0.41 days on average. However, the treatment- and control groups are similar in terms of sickness absence in the time periods preceding the experiment, both in terms of individual- and workplace characteristics, which indicates that the experiment was well conducted.

To measure the presence of peer effects in sickness absence, we make use of the random variation in the share treated co-workers induced by the experiment. One potential threat to the empirical strategy employed is that workplaces with different shares of treated workers differ with respect to sickness absence also in the absence of the experiment. In Table A2 we display the same descriptive statistics depicted in the previous table, but for workers at four different types of workplaces, characterized by the proportion treated workers: those with less than 13 percent treated workers, between 13-28 percent, 28-35 percent and more than 35 percent treated workers, respectively.[8] Indeed, there are some differences between the groups. For instance, one large difference between the groups is commuting workers: 64 percent of the employees at workplaces in group 1 commute, whereas the corresponding number for group 4 is 18 percent. The share of workers with some college education is highest in group 4, but average earnings are the highest in group 1. Furthermore, the share of female employees increases with the share treated (women are less likely to commute).

Importantly, the pre-experimental sickness absence is almost monotonously increasing with the share treated. This is true both in terms of workplace-averages and individual sickness absence. This difference likely arises from the randomization being only on workers living in Gothenburg municipality, and that workplaces with different shares of commuting workers differ in terms of worker characteristics. The analysis includes only workers who were assigned to either the treatment or control group. However, to take workplace hetero-

[8]The division is defined by the 25th, 50th and 75th percentiles of proportion treated workplaces with 10-100 employees.

geneity into account we control for the share of commuters at the workplace, a number of other workplace characteristics as well as the workplace average sickness absence. Thus, we make use of the random variation in treatment and the share of treated co-workers induced by the experiment, conditional on the share of non-eligible workers and workplace characteristics. The empirical specifications employed are discussed in further detail in the following section.

4.3 Empirical Specifications

We begin by estimating the effect of being assigned to treatment, and to capture potential peer effects we estimate the effect of the proportion treated co-workers on individual sickness absence. Our baseline model is specified as:

$$y_{ig} = \beta_0 + \beta_1 T_{ig} + \beta_2 \pi_{ig} + x'_{ig}\beta_3 + z'_{(-i)g}\beta_4 + \epsilon_{ig} \tag{4}$$

where y_{ig} is the number of days (including zero) on work absence - for spells that are shorter than 15 days or shorter than 8 days (corresponding to non-monitored absence for the treated and non-treated, respectively) in the second half of 1988, for employee i who is employed at workplace g. T_{ig} takes on the value one if individual i at workplace g is treated, and zero otherwise. π_{ig} is the share of treated co-workers at employee $i{:}s$ workplace (excluding employee i). β_1 then measures the main effect of the experiment on work absence, and β_2 the effect of the proportion treated co-workers on individual work absence. x'_{ig} is a vector of individual characteristics and $z'_{(-i)g}$ a vector of workplace characteristics (excluding individual i), such as the number of employees, the average age of workers, share female employees, average income, share of workers with at most high school education or some college education and dummies for industry affiliation. $z'_{(-i)g}$ also includes the workplace average days on sickness absence in Spring 1988, Spring and Fall 1987, as well as dummy variables for different shares of commuting employees at the workplace (10 percent bins). This selection-on-observables estimator allows us to non-parametrically identify peer effects. Compared to a difference-in-differences estimator or to a fixed-effects estimator, this identification strategy has the advantage of providing more precise estimates.[9] An additional advantage is that the strategy employed can be tested using pre-experimental data. Inference is based on standard errors that are clustered at the workplace level, i.e., they are robust to unspecified conditional correlations between individuals at the workplace.

We also estimate a similar specification to Equation (4) where we focus separately on treated and non-treated workers, respectively, to estimate the effect of the share treated co-

[9]Since we do not control for workplace-fixed effects in our estimations, one could be worried that our identification strategy does not take into account potential sorting of workers across workplaces. However, if there is no worker mobility across workplaces during the experiment, we are effectively using within-workplace variation. Worker mobility due to the experiment seems unlikely as this would imply individuals changing jobs due to an experiment that is known to last for only 6 months. The sorting that is potentially more problematic in this setting is instead the one that comes from commuting workers. However, we take into account both the share of commuting workers, as well as pre-experimental sickness absence at the workplace, and thus address workplace heterogeneity.

workers on individual work absence:

$$y_{ig} = \beta_0 + \beta_1 \pi_{ig} + x'_{ig}\beta_2 + z'_{(-i)g}\beta_3 + \epsilon_{ig} \tag{5}$$

where the vectors x'_{ig} and $z'_{(-i)g}$ are the same as in Specification (4).

5 Results

5.1 The effect of relaxed monitoring and the impact of peers on shirking

Before studying gender heterogeneity in the effects of treatment and in peer effects, we analyze the impacts of the experiment for the full sample. While this analysis is provided in Hartman et al. (2013) and Hesselius et al. (2009, 2013), respectively, our analysis is made on a different sample (we exclude commuting workers from our analysis), and with a different estimation strategy. Thus, this analysis is provided to ascertain that the impacts are similar, despite our differences in sampling and estimation approach. Table 1 reports the results from estimating Equation (4) and shows that treated workers increased their absence by 0.36 days in the second half of 1988 compared to the control group. Columns (2) and (3) report results from estimating Equation (5) on non-monitored absence for treated and non-treated workers separately. There is no statistically significant peer effect among treated workers, but a significantly positive peer effect among non-treated workers of 0.82 days. Table A3 in the Appendix reports results from estimating Equation (4) on monthly absence days in 1988. The increased shirking among the treated is instantaneous; while there are no differences in absence between treated and control individuals in January through June (which are essentially placebo tests), treated workers are estimated to have 0.06 days more absence compared to the control group in July, an effect that remains fairly constant throughout the rest of 1988. The peer effect, however, appears already in June, and then gradually wears off. Interestingly, the peer effect thus started one month before the experiment. This is likely a result of the massive information campaign that preceded the experiment, which included mass-media coverage. In fact, an article appeared in the largest newspaper in Gothenburg, Göteborgsposten, on June 9th, 1988, with the headline "Sickness absence without medical certificate". It explained that all workers born on an even date would be able to be on sick leave at their own discretion for 14 days. The start-date of the experiment was however not printed in the article. It is thus possible that the newspaper article (and other media) created an expectation among those born on an uneven date that their treated peers would increase their absence, and that this expectation itself triggered an early response to having co-workers that would receive a longer duration of non-monitored absence. Furthermore, since the peer effect is instantaneous, it is unlikely to be driven by health spillovers due to e.g. increased workload for the non-treated when treated co-workers are absent from work; such an effect would arguably imply a successively increasing peer effect over time. The absence of a statistically significant peer effect among the treated workers suggest that joint leisure or an endogenous effect are not driving mechanisms for the estimated peer effect. Both joint

Table 1: Parameter estimates from the OLS estimation of the effect of treatment and effect of share treated co-workers on sickness absence days

	All <15 days	Treated <15 days	Non-treated <8 days
A. *Sickness absence days in Fall 1988*			
Treatment	0.36***		
	(0.05)		
Proportion treated	0.82**	0.53	0.92***
	(0.33)	(0.47)	(0.32)
B. *Sickness absence days in Fall 1987 (Placebo)*			
Treatment	0.03		
	(0.04)		
Proportion treated	-0.09	-0.40	-0.06
	(0.22)	(0.31)	(0.22)
Observations	61715	30339	31376

NOTES.— The outcome variables are the number of days on non-monitored absence in the Fall of 1988 and the Fall of 1987 (placebo year). Included covariates are gender, age, earnings, dummies for schooling level, dummies for the share commuters at the workplace (divided in 10 percent bins), share female employees, average age at workplace, average earnings at workplace, share employees with compulsory-, high school- and college education, dummies for industry affiliation, workplace average sickness absence days (excluding individual i) in fall and spring of 1987 and spring 1988. The samples consists of individuals living in Gothenburg municipality and employed at workplaces with 10-100 employees. Standard errors are clustered at the workplace level. *p<0.1, **p<0.05 ***p<0.01.

leisure and endogenous effects would arguably yield similar peer effects for both the treated and non-treated workers. Moreover, since the peer effect is instantaneous, it is unlikely that the response among the non-treated is due to an endogenous effect nor due to negative externalities on health; if an increased absence among peers would cause an increased workload, and thereby more stress, a more likely pattern would have been a gradual increase in the peer effect over time. Thus, in line with Hesselius et al. (2009, 2013), our findings suggest that the peer effects are not driven by nonsocial spill-overs. We also estimate placebo regressions based on Specification (4) with the outcome variable being sickness absence days in the fall of 1987, i.e., one year before the experiment. The results are presented in panel B of Table 1 and shows no significant effects of either treatment or of the share treated co-workers.[10]

[10]We have also estimated the effect of treatment and share treated on monthly sickness absence in 1989, which is the first post-experiment year. Results show that there are no significant effects of being assigned to treatment in any month of 1989, and thus sickness absence is higher among the treated only during the experimental period. However, there is a somewhat lingering peer effect. We also tested the sensitivity of our estimates for the inclusion of higher order terms for the number of employees and workers age, as well as including the share of commuters linearly in the model, both with and without higher order terms for the share of commuters. The results are robust to all these variations of the specification and the results are available

5.2 Heterogeneous responses by gender

Whether women are more other-regarding than men can in our setting be studied by simply analyzing whether the influence of peers differs in magnitude for male and female workers. If women care more about what others do, we expect the peer effect to be of greater importance for women than for men. To study whether women's social preferences are more situationally specific than men's, we can examine whether potential peer effects differ when taking into account who the peers are. Specifically, we study whether men and women are affected to the same extent by same-sex peers as those of the opposite gender.

Table A4 in the Appendix presents summary statistics separately for the male and female workers in our sample. In line with previous empirical findings, female workers have more days on sick leave compared to male workers, in both 1987 and 1988. However, the difference in work absence between the first and second half of 1988 is larger for male workers. Moreover, women earn significantly lower incomes compared to men, and are employed at workplaces with a larger share of female employees, lower average earnings, higher average educational level and a smaller share of commuting co-workers. Thus, the labor market is highly gender segregated, and the absence levels at the average woman's workplace is higher than that of the average male worker's.

Table 2 presents the results from OLS regressions, based on Equation (4), of the effect of being assigned to treatment and of the fraction of treated peers on the full sample, male and female workers, respectively. The effect of being assigned to treatment is larger for men than for women: being assigned to treatment increases male workers' absence by, on average, 0.46 days in the second half of 1988, whereas the corresponding increase among women is 0.28 days. The table also includes baseline absence days, which correspond to the average number of days spent in spells shorter than 15 days in the second half of 1987, i.e., one year before the experiment. Compared to the baseline absence, the increase in male workers' absence correspond to a 19 percent increase, and for women an increase of about 10 percent. Hence, the effect of decreased monitoring on shirking is almost twice as large for men compared to women.

One potential explanation for this result could be that male workers have a lower threshold to shirking compared to female workers. For instance, Thoursie (2004) studies moral hazard in the Swedish sickness insurance by estimating the change in the number of men and women who report sick during a popular sporting event, and provides evidence that the number of men who reported sick increased in order to watch sporting events on television. However, a stylized fact in the study of absenteeism is that women, on average, utilize the sickness insurance to a greater extent than men. Under the assumption that the health of women and men is the same, the difference in the effect of monitoring could also stem from men being less inclined to visit a doctor to obtain a certificate. Hence, decreasing the requirement would increase the absence more for male workers than for female workers.

Interestingly, the social interaction coefficient is larger in magnitude for female workers (and not statistically significant for men). In addition, we have also estimated the social interaction effects separately by treatment status and found that the estimated peer effect

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upon request.

for women is driven by female non-treated workers, who increase their non-monitored absence.[11] One interpretation of these findings is that women are indeed more socially minded than men: while women take their co-workers' behavior into account to a greater extent when deciding whether to shirk or not, men seem to be more constrained by formal monitoring in the absence decision.

Lastly, Table A5 in the Appendix presents "placebo estimates" where we estimate Equation (4) on sickness absence days in the second half of 1987, i.e., one year before the experiment, separately for male and female workers. We find no significant effects of either treatment or of the fraction treated co-workers for any sub-sample.

Table 2: Parameter estimates from the OLS estimation of the effect of treatment and effect of share treated co-workers on sickness absence days

	All $<$15 days	Male workers $<$15 days	Female workers $<$15 days
Treatment	0.36***	0.46***	0.28***
	(0.05)	(0.07)	(0.07)
Share treated	0.82**	0.70	1.00**
	(0.33)	(0.48)	(0.44)
Baseline absence days	2.62	2.37	2.86
Observations	61715	29826	31889

NOTES.— The outcome variables are the number of days on sickness absence in spells that are shorter than 15 days in the Fall of 1988. Included covariates are age, earnings, dummies for schooling level, dummies for the share commuters at the workplace (divided in 10 percent bins), share female employees, average age at workplace, average earnings at workplace, share employees with compulsory-, high school- and college education, workplace average sickness absence days (excluding individual i) in fall and spring of 1987 and spring 1988. Standard errors are clustered at the workplace level. *p$<$0.1, **p$<$0.05 ***p$<$0.01.

5.3 Differential responses to peers by co-workers' gender

The results presented in the previous section show that the moral hazard effect is larger for male workers. Regarding the peer effects, the coefficient on the share treated colleagues is slightly larger in magnitude for female workers, and not statistically significant for men. The difference in the social interaction coefficient for men and women is, however, not statistically significant. Thus, we do not find any strong evidence that women are more socially minded than men in their shirking decision. Although women and men may be equally other-regarding on average, there may still be differences in how the social preferences of men and women differ depending on the social context.

Although we cannot change the social conditions in the experiment, we can study whether the social interaction effect among men and women differ when we take into consideration

[11] These results are available upon request.

the composition of the reference group. If women's social preferences are more situationally specific we would, for instance, expect to see that the peer effect differs for women depending on who their peers are, whereas the peer effect for men would be the same independently of who their co-workers are. To explore whether this is the case, we consider how the social interaction effect differs with the proportion treated workers that are women or men, respectively. That is, we study whether the similarity of peers matter for the magnitude of the social interaction effect, and whether it matters to a different extent for men and for women.[12] To this end, we decompose the fraction treated co-workers into two variables that measure the fractions of male and female treated workers, respectively. We then estimate Equation (4) where the variable *Share treated* is replaced by the two new variables *Share treated men* and *Share treated women*.

The results are presented in Table 3, where columns (1) and (2) present the results for men and women, respectively, and include the same covariates as in the previous specifications. Looking at the results for women, the coefficient on the share of treated women is positive and statistically significant, suggesting that increasing the share of treated female co-workers from 0.25 to 0.75 increases women's absence by 0.65 days. The coefficient on the share of treated men, however, is small in magnitude and not statistically significant. Turning to the results for male workers in column (1), the pattern is the opposite: the coefficient on the share of treated women is negative, albeit not statistically significant, whereas the coefficient on the share of treated male co-workers is positive and significant, indicating that increasing the share of treated male peers from 0.25 to 0.75 increases male workers' absence by 0.54 days, on average. These evidence suggest that both male and female workers are sensitive to the behavior of their peers, but that not all peers have the same influence on individual behavior. Rather, men seem only affected by other men, and women by other women.

As mentioned previously, the Swedish labor market is highly gender segregated. Hence, one might be worried that these results simply reflect the fact that women are more exposed to other female workers and men more exposed to other male workers. The estimates presented in columns (1) and (2) include controls for the fraction of women at the workplace as well as dummy variables for industry affiliation. Nevertheless, also within workplaces there might be gender segregation in the types of occupations held by women and men. For example, female workers are perhaps more likely to hold occupations with administrative tasks, resulting in more frequent interaction with other administrative (female) staff. Ideally, we would like to control for occupations, on which we lack data. However, we can control for the field of education, as well as the combination of educational field and educational level. The latter is likely to take into account a large part of the variation in occupations across the genders. In columns (3) and (4) of Table 3 we have included a full set of dummies for educational field (9 categories), and in columns (5) and (6) a full set of dummies for the combination of field and education (47 categories). As seen, the results are robust to

[12]The tendency of individuals to prefer associating with others that are similar to themselves has been documented as a relatively robust empirical observation (Currarini et al. 2009, Mas & Moretti 2009). For example, Asphjell et al. (2013) study peer effects within the workplace in fertility decisions and find that women's childbearing decisions are indeed affected by the fertility decisions of their co-workers, but the effect is entirely driven by other female peers.

the inclusion of both field of education as well as field- *and* level of education. Hence, the stronger influence of same-sex co-workers is not likely to be explained by gender-segregated workplaces. Rather, our results reflect the influence that (fe)male co-workers have on each other conditional on the potential exposure to same-sex colleagues.

That workers are mainly influenced by same-sex peers might also have interesting policy implications as it shows that social interaction effects are likely to be a function of the similarity of peers. For example, if individuals are more influenced by peers that are similar to themselves, potential spillover effects of policy interventions will arguably be more sizeable in homogenous groups than in groups with a more heterogenous population.

Table 3: Parameter estimates from the OLS estimation of the effect of treatment and effect of share treated men and share treated women on sickness absence days separately by gender

	(1) Male <15 days	(2) Female <15 days	(3) Male <15 days	(4) Female <15 days	(5) Male <15 days	(6) Female <15 days
Treatment	0.43***	0.28***	0.43***	0.28***	0.45***	0.27***
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Share treated women	-0.06	1.35***	-0.06	1.32***	0.01	1.28**
	(0.65)	(0.50)	(0.65)	(0.50)	(0.65)	(0.50)
Share treated men	1.08*	0.10	1.15**	0.18	1.19**	0.25
	(0.56)	(0.68)	(0.56)	(0.68)	(0.55)	(0.68)
Industry dummies	✓	✓	✓	✓	✓	✓
Field of education, 1 level			✓	✓		
Field of education, 2 levels					✓	✓
Observations	29826	31889	29826	31889	29826	31889

NOTES.— The outcome variables are the number of days on sickness absence in spells that are shorter than 15 days in the Fall of 1988. Included covariates are age, earnings, dummies for schooling level, dummies for the share commuters at the workplace (divided in 10 percent bins), share female employees, average age at workplace, average earnings at workplace, share employees with compulsory-, high school- and college education, workplace average sickness absence days (excluding individual i) in 1987 and 1988 and a full set of dummies for industry affiliation. Standard errors are clustered at the workplace level. *p<0.1, **p<0.05 ***p<0.01.

6 Concluding discussion

In this paper, we exploit a setting in which peer effects are informative of social preferences to study whether there are differences in social preferences between the genders in determining shirking behavior. To this end, we use exogenous variation in co-workers' absence induced by a large scale social experiment that altered the incentives for short term sickness absence for nearly half of all workers in Gothenburg. The experiment increased the monitoring-free period of sickness absence from 7 to 14 days for the treated, which were randomly assigned, whereas the control group faced the usual restriction of 7 days of non-monitored absence.

The experiment allows us to address the serious identification issues inherent in estimating peer effects, and to study the presence of social preferences. The latter is made possible due to there being no concern for externalities imposed on other workers from the increased shirking induced by the experiment, and that information sharing is unlikely to be a mechanism for the spillover effects. Thus, in the absence of social preferences, workers should not respond to their co-workers' behavior in their decision to be absent from work.

We find that decreased monitoring significantly increases non-monitored absence among treated workers. Second, we find significantly positive peer effects in shirking; non-treated workers increase their non-monitored absence in response to being exposed to treated peers. Third, we find that male workers increase their absence almost twice as much as female workers when monitoring decreases. Women's shirking behavior, on the other hand, seems slightly more responsive to peers compared to that of men's shirking. Interestingly, however, we find that men are only affected by their male peers, and women are only affected by their female peers. Decomposing the effect of the fraction treated peers into fractions of male and female treated peers shows that there is no significant difference between the effect of peers on male and female workers' absence. Instead, the entire peer effect among men is driven by the effect of treated male co-workers and vice versa for women. These results hold true even as we control for the fraction of women at the workplace, industry affiliation, as well as dummies taking into account both the field and level of education. Hence, the stronger influence of same-sex co-workers cannot be explained by gender-segregated workplaces. Our results reflect the influence that (fe)male co-workers have on each other conditional on the potential exposure to same-sex colleagues.

These findings cast some doubt on the hypothesis that women's social preferences are more malleable: both male and female workers care about their social context when context is defined by worker similarity. Thus, women's decisions do not seem to be more situationally specific than men's in our setting.

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Appendix

Figure A1: The proportion working in Gothenburg municipality (upper graph); proportion treated co-workers (middle graph); and the proportion treated (lower graph) against the kilometer distance between residence neighborhood and central Gothenburg.

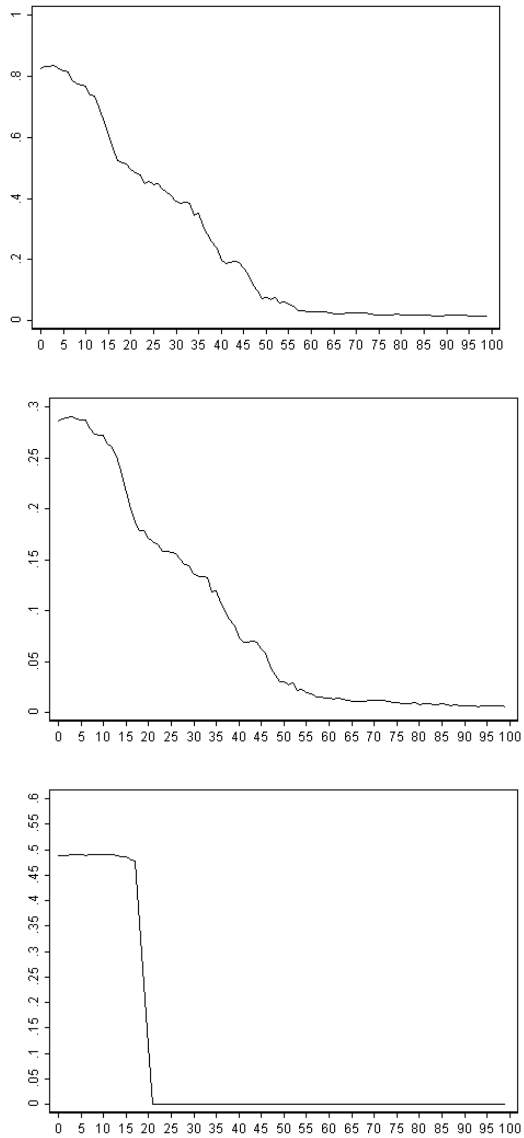

Table A1: Summary statistics by treatment status

	Control		Treated	
	Mean	Std dev.	Mean	Std dev.
Individual characteristics				
Absence days < 15 day spells, Fall 1988	3.972	(5.848)	4.380	(6.637)
Absence days < 15 day spells, Spring 1988	3.444	(5.381)	3.467	(5.399)
Absence days < 15 day spells, Fall 1987	2.607	(4.786)	2.631	(4.874)
Absence days < 15 day spells, Spring 1987	2.735	(4.940)	2.687	(4.892)
Female	0.510	(0.500)	0.508	(0.500)
Compulsory schooling	0.282	(0.450)	0.282	(0.450)
High school	0.443	(0.497)	0.446	(0.497)
College	0.256	(0.436)	0.254	(0.435)
Earnings in 1988, SEK	98553.3	(68934.1)	99189.4	(68901.3)
Age	36.35	(12.69)	36.25	(12.67)
Workplace characteristics				
Share treated	0.293	(0.134)	0.302	(0.141)
Share commuters	0.377	(0.238)	0.382	(0.240)
Number of employees	39.39	(25.35)	39.52	(25.47)
Workplace average age	36.58	(5.899)	36.51	(5.902)
Workplace average earnings	99562.5	(37256.5)	100103.0	(37746.9)
Share employees with compulsory education	0.294	(0.186)	0.293	(0.187)
Share employees with high school education	0.427	(0.176)	0.426	(0.176)
Share employees with college education	0.233	(0.250)	0.235	(0.251)
Share female employees	0.507	(0.313)	0.504	(0.311)
Workplace average sickdays, Fall 1988	3.975	(1.922)	3.976	(1.941
Workplace average sickdays, Spring 1988	3.357	(1.651)	3.346	(1.643)
Workplace average sickdays, Fall 1987	2.557	(1.387)	2.541	(1.380)
Workplace average sickdays, Spring 1987	2.664	(1.350)	2.647	(1.350)

NOTES.— The table presents means and standard deviations (in parentheses) of individual and workplace characteristics. The sample consists of workers living in Gothenburg municipality and working at workplaces with 10-100 employees.

Table A2: Summary statistics by share of treated co-workers

	(1) $< 13\%$	(2) $13\% - 28\%$	(3) $28\% - 35\%$	(4) $> 35\%$
Individual characteristics				
Absence days < 15 day spells, Fall 1988	3.714	4.195	4.219	4.651
	(5.887)	(6.247)	(6.276)	(6.607)
Absence days < 15 day spells, Spring 1988	3.061	3.395	3.570	3.872
	(5.017)	(5.362)	(5.478)	(5.715)
Absence days < 15 day spells, Fall 1987	2.293	2.680	2.642	2.926
	(4.475)	(4.952)	(4.836)	(5.078)
Absence days < 15 day spells, Spring 1987	2.373	2.711	2.811	3.018
	(4.534)	(4.880)	(5.002)	(5.268)
Female	0.405	0.447	0.548	0.654
	(0.491)	(0.497)	(0.498)	(0.476)
Compulsory schooling	0.262	0.286	0.289	0.296
	(0.440)	(0.452)	(0.453)	(0.456)
High school	0.461	0.479	0.428	0.408
	(0.498)	(0.500)	(0.495)	(0.491)
College	0.263	0.217	0.263	0.275
	(0.441)	(0.412)	(0.440)	(0.446)
Earnings in 1988, SEK	104915.4	102790.1	98076.4	88491.7
	(74070.6)	(70842.9)	(67552.0)	(60157.2)
Age	35.57	36.17	37.03	36.57
	(12.35)	(12.73)	(12.87)	(12.78)
Workplace characteristics				
Share treated	0.127	0.278	0.351	0.466
	(0.0783)	(0.0237)	(0.0220)	(0.0648)
Share commuters	0.642	0.365	0.281	0.180
	(0.240)	(0.127)	(0.110)	(0.105)
Number of employees	36.55	41.00	44.24	36.60
	(25.14)	(24.41)	(25.45)	(25.82)
Workplace average age	36.40	36.19	37.05	36.57
	(5.907)	(5.840)	(5.882)	(5.938)
Workplace average earnings	106121.6	103926.7	99516.2	88510.3
	(38503.1)	(38019.8)	(37549.3)	(32857.4)
Share employees with compulsory education	0.302	0.288	0.288	0.293
	(0.185)	(0.182)	(0.184)	(0.195)
Share employees with high school education	0.450	0.457	0.410	0.385
	(0.179)	(0.168)	(0.177)	(0.169)
Share employees with college education	0.210	0.210	0.253	0.269
	(0.241)	(0.228)	(0.265)	(0.262)
Share female employees	0.418	0.433	0.541	0.648
	(0.295)	(0.302)	(0.303)	(0.292)
Workplace average sickdays, Fall 1988	3.471	3.952	4.064	4.515
	(1.635)	(1.843)	(1.900)	(2.201)
Workplace average sickdays, Spring 1988	2.975	3.297	3.427	3.782
	(1.472)	(1.623)	(1.577)	(1.817)
Workplace average sickdays, Fall 1987	2.260	2.556	2.577	2.862
	(1.266)	(1.397)	(1.314)	(1.495)
Workplace average sickdays, Spring 1987	2.365	2.624	2.717	2.973
	(1.216)	(1.311)	(1.270)	(1.531)

NOTES.— The table presents means and standard deviations (in parentheses) of individual and workplace characteristics for individuals with different proportions of treated co-workers, where the subgroups are defined by the 25th, 50th, and 75th percentiles.

Table A3: Parameter estimates from the OLS estimation of the effect of treatment and effect of share treated co-workers on monthly sickness absence days in 1988

	Treatment	Proportion treated
January	-0.01	-0.06
	(0.01)	(0.08)
February	0.00	-0.08
	(0.01)	(0.09)
March	0.01	0.08
	(0.01)	(0.09)
April	0.02*	-0.10
	(0.01)	(0.09)
May	-0.01	0.11
	(0.01)	(0.08)
June	0.01	0.30***
	(0.01)	(0.08)
July	0.06***	0.19**
	(0.01)	(0.09)
August	0.06***	0.15
	(0.01)	(0.10)
September	0.05***	0.14
	(0.02)	(0.10)
October	0.06***	0.25**
	(0.02)	(0.10)
November	0.06***	0.06
	(0.02)	(0.10)
December	0.08***	0.04
	(0.02)	(0.13)

NOTES.— The outcome variables are the number of days on sickness absence in spells that are shorter than 15-days in each month of 1988. Included covariates are gender, age, earnings, dummies for schooling level, dummies for the share commuters at the workplace (divided in 10 percent bins), share female employees, average age at workplace, average earnings at workplace, share employees with compulsory-, high school- and college education, dummies for industry affiliation, workplace average sickness absence days (excluding individual i) in fall and spring of 1987 and spring 1988. The samples consists of individuals living in Gothenburg municipality and employed at workplaces with 10-100 employees. Standard errors are clustered at the workplace level. *p<0.1, **p<0.05 ***p<0.01.

Table A4: Summary statistics by gender

	Male		Female	
	Mean	Std dev.	Mean	Std dev.
Individual characteristics				
Absence days < 15 day spells, Fall 1988	3.900	(6.307)	4.436	(6.187)
Absence days < 15 day spells, Spring 1988	3.136	(5.319)	3.764	(5.440)
Absence days < 15 day spells, Fall 1987	2.367	(4.720)	2.863	(4.920)
Absence days < 15 day spells, Spring 1987	2.447	(4.722)	2.967	(5.084)
Compulsory schooling	0.274	(0.446)	0.290	(0.454)
High school	0.471	(0.499)	0.421	(0.494)
College	0.233	(0.423)	0.276	(0.447)
Earnings in 1988, SEK	117900.1	(81158.3	80476.1	(47821.3)
Age	35.87	(12.53)	36.71	(12.81)
Workplace characteristics				
Share treated	0.272	(0.132)	0.322	(0.137)
Share commuters	0.433	(0.232)	0.328	(0.235)
Number of employees	39.97	(25.29)	38.95	(25.52)
Workplace average age	36.07	(5.682)	37.00	(6.070)
Workplace average earnings	106964.6	(38012.9)	92933.2	(35664.6)
Share employees with compulsory education	0.310	(0.179)	0.277	(0.192)
Share employees with high school education	0.456	(0.167)	0.399	(0.179)
Share employees with college education	0.180	(0.222)	0.285	(0.264)
Share female employees	0.312	(0.242)	0.693	(0.251)
Workplace average sickdays, Fall 1988	3.899	(1.894)	4.049	(1.964)
Workplace average sickdays, Spring 1988	3.243	(1.556)	3.456	(1.723)
Workplace average sickdays, Fall 1987	2.485	(1.333)	2.612	(1.428)
Workplace average sickdays, Spring 1987	2.561	(1.248)	2.747	(1.437)

NOTES.— The table presents means and standard deviations (in parentheses) of individual and workplace characteristics for male and female workers separately. The sample consists of workers living in Gothenburg municipality and working at workplaces with 10-100 employees.

Table A5: Placebo estimates from the OLS estimation of the effect of treatment and effect of share treated co-workers on non-monitored absence in 1987

	All <15 days	Treated <15 days	Non-treated <8 days
A. *Fall 1987, Male workers*			
Treatment	0.03		
	(0.05)		
Proportion treated	0.08	-0.26	-0.02
	(0.32)	(0.46)	(0.33)
N	29826	14710	15116
B. *Fall 1987, Female workers*			
Treatment	0.02		
	(0.05)		
Proportion treated	-0.18	-0.40	-0.10
	(0.32)	(0.45)	(0.31)
N	31889	15629	16260

NOTES.— The outcome variables are the number of days on non-monitored absence in the fall of 1987. Included covariates are gender, age, earnings, dummies for schooling level, dummies for the share commuters at the workplace (divided in 10 percent bins), share female employees, average age at workplace, average earnings at workplace, share employees with compulsory-, high school- and college education, dummies for industry affiliation, workplace average sickness absence days (excluding individual i) in fall and spring of 1987 and spring 1988. The samples consists of individuals living in Gothenburg municipality and employed at workplaces with 10-100 employees. Standard errors are clustered at the workplace level. *p<0.1, **p<0.05 ***p<0.01.