

## Peers and Work Absence

by

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### Abstract

We utilize a large-scale randomized social experiment to identify how workers absence behavior are affect by co-workers behavior and/or their option to be more work absent. The experiment altered the incentives to be present at the workplace by postponing the requirement for a doctor's certificate from the eight day to day fifteen in a sickness absence spell for half of all employees living in Göteborg, Sweden. Using administrative data we are able to recover the treatment status of all workers in more than 5,800 workplaces. We first document that treated (those with an option of 14 days non-monitored absence) increase their absence significantly and that absence of the controls increases with the share treated co-workers. On average the treated workers is not affected by the share of peers having this option. We, however, find large gender differences which suggest, among others, that treated females are affected by their treated female peers.

Keywords: Social interactions; Employer employee data; Work absence; fairness; reciprocal preferences

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## 1 Introduction

A substantial amount of theoretical work has suggested that social interactions within the workplace are an important determinant of worker effort and hence firm productivity. With the increasing availability of matched employer-employee data sets a burgeoning empirical literature have aimed at identifying to what extent within workplace social interactions affect productivity in practice. A strand of this literature has in turn focused on the social preferences of sickness absence. Sickness absence decisions are, of course, intimately related to moral hazard and the productivity of the firm.

The purpose of the present paper is to add further empirically insights into to what extent and how co-workers affect the moral hazard of being absent from work when the worker is covered by sickness insurance. We make use of an unusually well conducted randomized social experiment which changed work absence incentives for approximately 50 percent of the workers living in Göteborg, the second largest city in Sweden. The large scale experiment in combination with detailed data on work absence of all workers from a wide variety of workplaces (banks, supermarkets, department stores, pulp factories, etc.) enables us to attain credible internal and external estimates of the importance of social preferences for the decision to be absent from work. A further advantage with the large scale experiment is that it also permits analysis of differences in social preferences across sub-groups.

The randomized social experiment was running in the second half of 1988 (July to December). Assignment into either the control or the treatment group was based on day of birth. Those born on an even (uneven) date was assigned to the treatment (control) group. The experiment shifted the timing of formal monitoring of the treated individuals in the compulsory and universal sickness insurance, from the usual 8th to the 15th day of an absence spell and thereby reduced the treated individuals' incentives to work. Hesselius, Johansson and Larsson (2005) have previously evaluated the impact of the experiment, and find a significant increase in absence spell durations among the treated. However, they did not take potential social interaction effects into account when estimating the treatment effect. Hesselius, Johansson and Nilsson (2010, proceedings JEEA) found that social interactions was affecting the work absence behavior of non-treated individuals.

Our paper sheds light on to what extent other regarding preferences, like fairness and inequality aversions, are important for work absence

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and if these preferences depending on the seniority and gender of the worker.

The data include information on all employees in workplaces within the Göteborg municipality borders. To these data we have matched information on each employee's earnings, educational level, gender and age. Furthermore, the data include information on all employees' daily work absence status from the universe of workplaces in operation prior to, during, and after the experiment.

To fix ideas on why social preferences should matter we view non-monitored sickness absence as leisure (first 7 days and first 14 days during the experiment period). The decision to be absent for the first seven days in a sickness spell is at the discretion of the worker which hence leaves room for shirking by being absent from work. With a high replacement rate and with no other restrictions then monitoring at day 7 an optimizing agent would be reporting sick during work days (5 days) and report as not sick during the weekends (2 days). However, the majority of the Swedish employees almost never report sick. This means that there are other restrictions, than just the medical certificate at day 8, or incitements that matters for attending work. One restriction is that it must be at least 5 days between each consecutive sickness spell but there are of course also long run cost from being absent from work. These costs may be monetary (i.e. from lower wage increases) and/or from a disliking by the colleagues. This cost and potential stigma may differ if the absence stem from an illness or from shirking. If work absence stems from illness or bad health co-workers may, instead of dislike, express sympathies and show altruistic or reciprocal behavior.

In general it is not so easy for an econometrician or for that matter a colleague to identify shirking peers. However, the experiment did not affect the health of the treated and since it was publicly known an increase in the peers work absence is identified by the colleague. The consequence of this is that the experiment can be used to test for social preferences in work absence.

We find that a higher share of treated co-workers on average increases non-monitored sickness absence among the controls. The treated workers non-monitored sickness absence is on average, however, not affected by the share of treated co-workers. In addition we find on average no peer effect on the monitored sickness absence. In the light of our theoretical framework, we interpret these results as that the peer effect is stemming from a fairness or equity concern for leisure.

When we separately study newly hired and senior (tenure > 1 year) controls we find quite small differences in behavior between the two groups. However senior workers are, to some extent, more affected by the senior peers and newly hired workers, are to some extent, more affected by newly hired peers. Even though the difference in effects between the groups are not extremely large, the results shows that the reference group matters and that if mobility is used as a means to estimate norm effects one should take into consideration also the number of new hires at the workplace.

When studying the behavior of the women and men we find large behavioral differences on average. The peer effect for the women and men controls is of the same magnitude; however women are only affected by their fellow female co-workers. The treated women response to the treatment is larger in workplaces where the share of treated female co-workers is high, whereas the treated men's response to the treatment is independent of the share treated colleagues (neither men nor women). In order to further analyze these differences we study monthly sickness absence. We find that the treatment effect appears immediately (i.e. in July) and the effects are constant and of the same magnitude (.10 days and .05 for the men and women) throughout the whole experiment period. The peer effect (i.e. the effects from the share treated) on the controls is also immediate (i.e., in July). The effect from the share of treated women on the response of the treatment for treated women starts however first in September/October. Our interpretation of the female men difference among the treated is that men on average decide on how much they can shirk (when having the opportunity) without consider how co-workers take use of the possibility while women on average to a large extent study how other (women) behave and then react accordingly.

The evidence we provide is in line with several laboratory experiments which have suggested that social preferences are important for agents' behavior.<sup>1</sup> In these studies it has been noted that social preferences shape decisions among a non-negligible part of the population. Empirical evidence on the relevance of social preferences outside of the laboratory is however scarce. Our work is informed by and contributes to the emerging literature focusing on the social determinants of firm productivity. In two recent, interesting and related

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<sup>1</sup> Fehr and Gächter (2000) and Sobel (2001) for surveys of this literature

studies Bandiera, Barankay and Rasul, (2005) and Mas and Moretti (2009) use data from a fruit picking farm in the UK and 6 US supermarket stores, respectively. Our results, from a much larger and more heterogeneous population, confirm the results regarding the peer mechanism at work found in these two previous studies.<sup>2</sup> The dearth of empirical evidence on the relevance of social interactions and work absence is even more salient. Ichino and Maggi (2000) use individual level data on workplace absence and misconduct to study shirking differentials between different branches of a large Italian bank. Identification of social interaction effects in their case is based on movers between different branches of the bank. Thus, by studying changes in absence behavior among the movers, Ichino and Maggi conclude that the average absence levels among co-workers in the workplace are related to employees' absence. They do however stop short of providing insights regarding the specific behavioral mechanisms causing the observed pattern.

This paper also adds to the literature on differences in social preference of men and women and confirms a pattern seen in psychology (see e.g. Gilligan 1982) that women are more sensitive to social cues to determine their behavior than men. In the economic experimental literature this theory has been used to explain results where women's behavior in ultimatum and dictator games is seen as to be more contextual specific than the men's behavior (cf. Croson and Gnezy, 2009).

The paper unfolds as follows. In section 2 we briefly describe the general context of our study, providing details on the Swedish sickness insurance system and the experiment. Secondly in section 3 we discuss the theoretical framework. The identification strategy and the data are discussed in section 4. The empirical analysis is discussed in section 5 and finally section 6 concludes.

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<sup>2</sup> Bandiera, Barankay and Rasul (2005) study how social preferences interact with pay schemes. They find that under relative pay workers reduce their effort as high effort induces a negative externality on their co-workers, but only when mutual monitoring is possible. Mas and Moretti (2009) find out that among super market cashiers productivity is affected by colleagues productivity, but also here these effects is only present when mutual monitoring is possible.

## 2 Swedish sickness insurance and experiment Design

### 2.1 The sickness benefit system

The Swedish sickness insurance is compulsory and universal to all employed workers, students and unemployed. It is financed by a proportional pay roll tax and replaces individuals earnings lost due to temporary health problems. The benefit level received is related to the lost earnings during the absence spell.

In an international context the sickness benefit levels are, and have been rather generous. In 1988, during the experiment, for most workers the benefit level was set to 90 percent of previous earnings. Some workers at the very top of the wage scale were however excluded from receiving the full 90 percent due to a benefit cap. Besides the public insurance, most Swedish workers are also covered by extra sickness insurance regulated in agreements between the unions and the employer's confederations. These top-up insurances generally cover about 10 percent of the lost earnings but there is considerable variation. Hence the total compensation in case of work absence due to illness could be fully 100 percent.

The public insurance has no limit for how long and how often sickness benefits are paid. Many spells stretch over a full year and there are examples of even longer durations. While the benefit payments are generous, the monitoring before the 8th absence day is lax. A sickness absence spell starts when the worker calls the public insurance office (and her employer), then within a week (on the 8th day) he/she should confirm her eligibility with the insurance office by presenting a medical certificate proving reduced work capacity due to illness. The public insurance office reviews the certificate and then declines or approves further sick-leave. In all but very few cases the certificate is approved.<sup>3</sup>

Of course, some exemption rules make it possible for the public insurance offices to monitor more (or less) strict. When abuse is suspected, they may visit the claimant at home. Claimants who have been on sickness benefits too frequently in the past may be asked to show a doctor's certificate from day one. Moreover, a new sick spell starting within five working days of the first is counted as a

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<sup>3</sup> 98.5 percent of all medical certificates were approved in 2006 (Försäkringskassan, 2007). Since the approval rate has decreased lately was the approval rate most likely even higher in 1988.

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continuation of the first, making it impossible to report sick every Monday without ever visiting a doctor. Individuals with chronic illnesses, on the other hand, need not verify their eligibility each time illness forces them to remain at home.

Given the rather high benefit level and the rather lax control and monitoring the ex post moral hazard in the Swedish sickness insurance system is high (see e.g. Johansson & Palme (1996, 2002, and 2005) and Henreksson & Persson (2004) for empirical evidence).

### **2.2 The experiment**

In the second half of 1988 the regional social insurance agency in the municipality of Göteborg, the second largest city in Sweden, and in Jämtland, a large and relatively thinly populated region in the north of Sweden, agreed on performing a social experiment regarding the timing of the requirement for a physician's certificate. A randomly assigned treatment group was allowed to be sick absent for 14 days before they needed a physician's certificate in order to continue their absence spell with insurance compensation. The control group faced the ordinary restriction of 7 days. Individuals were assigned to the treatment and control group based on their date of birth. Those born on even days ended up in the treatment group, and those born on uneven days in the control group.

The insurance agencies had several arguments for running the experiment. All were based on a notion that extending the time-period without monitoring would decrease costs and reduce work absence. The main argument was that with the 14 day restriction unnecessary visits to physicians could be avoided, which would cut costs for both the individual and the public care system. The insurance agency also believed that physicians by routine prescribed longer absence from work than necessary. With an extended certificate free period of two weeks 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 implemented during the second half of 1988 and besides the personnel at the social insurance office, all employers and medical centers was informed before or during the experiment. A massive information campaign also preceded the experiment at the two locations, including mass-media coverage, distribution of pamphlets and posters at workplaces, etc. Short information about the experiment



was also written on the form which every insured reporting sick needed 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 substantially among the treated group compared to the control group on average. Hesselius et al. (2005) estimated that the average absence duration in the treatment group increased by 6.6 percent. They also report differential treatment effects between women and men. Men prolonged their work absence spells substantially more compared to women.

### 3 Conceptual framework

The purpose of this section is to provide a simple framework for discussing potential social interaction effects from the introduction of the experiment. In the present context formal monitoring of absent workers occurs at day 8 of an absence spell when medical screening by a physician is required for continued sick pay eligibility. In particular we seek to distinguish between the cases when workers have social preferences and when they do not. If workers have social preferences they care about the work absence of their peers also if the peers' absence does not have any negative externality on their own effort. Thus, in the absence of negative externalities and/or social preferences the peers absence will be irrelevant for the workers own work absence decision.

The sick pay is paid by the Swedish government which means that the only cost for the employer from an employee's absence is from the indirect cost of finding and hiring replacement workers and/or lost productivity.<sup>4</sup> In general, an employer cannot fire a worker for shirking in Sweden. The only possibility is if (s)he has been performing non-legal acts, e.g., by working during his/her sickness absence. Both these facts imply that the incentives for the employer to monitor employees' sickness absence are low.

Let  $T$  be the total time endowment for an individual and  $C$  be his/her contracted working hours at wage rate  $W$ . Let  $S$  be the time on non

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<sup>4</sup> This cost may especially be true in team production. In order to give workers incitements to not be absent in these types of jobs may employers offer higher wages than in other occupations. See e.g. Heywood and Jirjahn (2004) for an empirical application where workers in team production have lower work absence than workers involved in team

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monitored work absence for which an individual receives  $(1-\delta)W$ , where  $\delta$  is the replacement rate. The real working time is hence equal to  $h = C - S$ , where  $S < 8$ , and the real wage rate is  $W^* = W(C - (1-\delta)S)/h$ . If  $\delta > 0$  then  $W^* > W$ . The implication of this is that with no restrictions an optimizing agent would be sick listed during work days and not sick listed during the weekends (or non working days). Still most people in Sweden have no or very low levels sickness absence during a year. This means that there are other incentives than just the short run gains from being absent or that there are norms that restrict the take up rates. The long run economic consequences of sickness absence can however be large since it may signal low attachments and low productivity which may stop career opportunities. The take up rates may of course also depend on the monitoring by the employer and on the reaction/monitoring of the co-workers, i.e. the norm at the work place.

The experiment allowed 50 percent of the workers in Göteborg to be at home from work at their own discretion for 14 days. This hence means that the restrictions to be absent changed for the treated individuals. If the treated individuals increase their work absence as a response to the experiment then their leisure time is increasing (or the wage rate,  $W^*$ , is increasing).

Hence if workers care about fairness or equity the controls could as a response to an (expected) increase in work absence increase their absence to get the same amount of (expected) leisure as the treated workers. If fairness concerns for leisure are the only motive the treated would not adjust their work absence in response to an increase in the controls absence. It is also important to understand that the controls in comparison with their treated colleagues (who has the option of being absent longer) may feel that they are being treated unfair by the sickness insurance agency and as a consequence they increase their absence. If the peer effect is stemming from fairness concerns we, thus, have the following two predictions: (i) there should not be a peer-effect on the treated and (ii) the peer-effect could be instantaneous (i.e. at the time of the start of the experiment).

The potential behavioral response of the treated (i.e. an increase in sickness absence by the reduced monitoring) is a moral hazard effect. Above we discussed that the degree of moral hazard may depend on the long run economic consequences of sickness absence but also on the stigma or norm at the workplace. This stigma of being absent may be dependent on the health of the absent worker. An absence that is motivated by a severe illness may, most likely, be less stigmatizing than

when the absence is known to be from shirking. If the treated care about the stigma associated with what is acceptable shirking/leisure may the treated first observe other treated peers behavior and then after some time adjust their behavior. Thus, if peer effects are stemming from stigma and norms then: (i) we should find a peer effect also for the treated and (ii) this peer effect should be increasing over the experiment period.

## 4 Identification and data

The implication from the framework discussed above is that if there are peer effects then both control and treated individuals' work absence should increase with the share of treated peers. If the peer effect stem primarily from a fairness concern then only the controls work absence should be affected by the share of treated peers.

There is however other potential reasons, for why individual sickness absence could increase with the share of treated peers then from fairness concerns and/or peer pressure. Another potential social interaction effect is synchronized leisure. Synchronized leisure can be a reason however an increase in this behavior could for the controls only be changed by taking out more monitored (more than 7 days) absence. Hence, if we find that the controls increase their monitored absence then this could be the results from an increase in synchronized leisure. The treated could though increase the non-monitored absence as a consequence of synchronizing their extended absence period with treated colleagues. The implication of this is, hence, that we can test for the fairness hypothesis by studying the behavior of the control individuals.

A potential, treat to the identification strategy is that an increase in work absence among the treated may create negative externalities for the controls. This means that they would need to increase their effort and this may increase illnesses. This would then lead to an increase in monitored work absence.

Thus, in order to test the hypothesis of social preferences stemming from a fair distribution of leisure we regress non-monitored absence on the share of treated for both treated and controls. In order to test the hypothesis of negative externalities causing increased illness we also regress monitored absence and incidence into monitored absence on the share treated for the controls. If non-monitored work absence is increasing with the share of non-treated for the controls and no other

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effect is found we take this as evidence that fairness matter for the decision to be absent.

If we also find that work absence is increasing with the share treated among the treated we test for if this stems from norms/stigma of the colleagues by estimate monthly time profiles. If there is an effect from synchronized leisure the “peer effect” should take place immediate whereas a gradually increasing peer effect over the experiment period (i.e. the effect of the share treated) suggest that norms or stigma are important for the moral hazard in the sickness insurance and, hence, for work absence.

### 4.1 Data

We use data from a set of administrative registers compiled by Statistics Sweden. The data contains, besides a set of individual background characteristics, data on start and end date of all absence spells during 1987-1991. We also observe the workplaces where the individual is gainfully employed. A few individuals have multiple workplaces, but for simplicity we assume that the workplace from which the highest yearly earning is received is also the main arena for co-worker interaction. The treatment status of each worker was decided by date of birth (even/uneven) and whether the individual is residing in Göteborg municipality or not.

As seen in Figure 1, the between workplace variation in the share treated is considerable. The average workplace has around 30 percent treated workers. The variation in the share treated workers stem from the random assignment of treatment, but also from the number of commuting co-workers. In the main analysis we focus on workplaces with between 10 and 100 employees as social interactions is probably most prevalent in small to medium sized workplaces. The workplaces with 10 employees and less are excluded from the sample as alternative rules may apply to these workplaces. In figure 2 we display the histogram of the share treated for this sample of individuals. The distribution is, as expected, more compressed at the both tails. The average share is, however basically unchanged (now the average workplace has around 31 percent treated). The mass point at zero co-workers stems from employees who commute outside Göteborg municipality.

[Figure 1 and 2 about here]

[Table 1 and 2 about here]

Tables 1 and 2 provide descriptive statistics for the population of workers living in Göteborg. Table 1 gives descriptive statistics for the original data without restricting the size of workplaces. Descriptive statistics for the analysis populations (i.e., individuals employed in workplaces with more than 10 and less than 100 workers) is given in Table 2. From these tables we can see that: (1) there are no differences in sickness absence (spells shorter than 15 days) between the treated group and control group in the period before the experiment took place; (2) the two populations describe in Tables 1 and 2 are very similar when it comes to population characteristics and (3) the difference in sickness absence between the treated and control group is 0.45 days and 0.41 days for the two populations, respectively.

All in all, this suggests that the experiment is well conducted and that the all variables including the treatment effect is of the same magnitude in the two populations which supports external validity of the results.

One potential problem with the empirical strategy is that workplaces with different shares of treated differ with regard to sickness absence also in the absence of the experiment. In Table 3 we display descriptive statistics for four groups of workers: those with less than 1.4 percent treated peers, between 1.4 and 22 percent treated peers, between 22 and 35 percent treated peers and finally more than 35 percent treated peers. From this table we can see that there are quite substantial differences between the groups. The largest difference is, of course, with respect to commuting peers. 92 percent of the peers in group 1 commute but only 35 percent of the peers in group 4. The education level is the highest in group 1 and this group has also the lowest income and age. We can also see that pre experimental sickness absence is, almost monotonously, increasing with the share treated.

[Table 3: about here]

The correlation of share of treated with pre experimental sickness absence complicates the analysis, since it is not likely that the share of treated is exogenous. However it is likely that the difference in sickness

absence is stemming from difference in commuters<sup>5</sup> and since we observe the difference we can control for it and in the estimation take use of the random variation in share of treated given the number of commuters.

It is also interesting to note that when we compare half year (2:nd to 1:st) differences in sickness absence over the different groups in a difference in difference setup we obtain a monotonous increasing effect. The results from the estimations are displayed in Table 4. From this table we can see that there is a substantial treatment effect in all groups and that social preference may be important.

[Table 4 : about here]

## **5 Analysis**

In the estimation we control for the share of commuter as well as for the other covariates displayed in Table 2. One problem, with this approach is that workplaces with different shares of commuters also are systematically different in some unobserved way. One way to deal with this problem would then be to use difference or a difference in difference estimators. We have also used such estimators in sensitivity analyses (see discussion below). The present selection on observables estimator which allow us to non-parametric identify peer effects has the, additional, advantage of providing more precise estimates. Another advantage is that the identification strategy can be tested by using the pre-experiment data.

The baseline model is specified in equation (1)

$$Y_{ij} = \beta_0 + \beta_1 T_i + \delta sh_{ij} + \gamma_1 shcom_{ij} + \gamma_2 shcom_{ij}^2 + \alpha \#employ_{ij} + \eta' X_{ij} + \varepsilon_{ij} \quad (1)$$

Here  $Y$  is the number of days (including zero) in work absence in spells shorter than 15 days in the second half of 1988 of employee  $i$  who are employed at workplace  $j$ .  $T$  takes the value 1 if the employee is treated, and 0 otherwise.  $Sh$  is the share treated peers at employee  $i$ 's workplace (i.e. excluding employee  $i$ ). We control for the share of commuters ( $shcom$ ), number of employees ( $\#employ$ ) at the workplace as well as

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<sup>5</sup> There are several reasons for this; for instance is the relative cost of working higher for commuters than for non-commuters and they may also be more exposed to spreads of diseases.

the individual specific variables,  $X$ , displayed in Table 2. A significant estimate of  $\delta$  is a measurement of the average peer effect. Inference is based on estimated 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 the model separately for those being treated and controls and for the control individuals we also estimate models with  $Y$  being the number of days in work absence in spells shorter than 8 days in the second half of 1988.

As the dependent variable measures both duration in a spell and incidence ( $\text{days} > 0$ ) is  $\delta$  measuring the total average effect on work absence which consist in potential effects on incidence into work absence and duration in work absence. We therefore also separately study the incidence into work absence as well as the duration given a work absence spell by taking use of the regression model (1).

The main results from the estimations are given in column 2 in table 5. Column 1 displays the average treatment effects when we control for the potential social interaction effect by including the share of treated.<sup>6</sup> Columns 3-8 presents the results when the model is estimated using the pre-experiment period work absence data (i.e., first half year 1988 and first and second half year 1987).

From the first row and columns 1 and 2 we can see a statistically significant treatment effect of 0.41 days and a marginally (10 percent level) significant peer effect of 0.81 days. An extra week before a doctor certificate is needed would on average increase work absence by 0.41 days and this effect is the same effects as of having 50 percent treated peers.

[Table 5: about here]

Results from where we have estimated peer effects separately for treated and control are displayed in rows 2 and 3 (column 2). The results from these estimations are that the peer effect stem from the controls only. The peer effect for the controls is estimated to 1.21 days while for the treated the estimate is 0.47 days and this effect is not statistically significant. Thus, having 50 percent treated co-workers

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<sup>6</sup> That is the stable unit treatment assumption (SUTVA) is violated and by controlling for the share of treated we obtain the average treatment effect.

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would on average increase the work absence of the controls by 0.60 days. Turning to the estimation results when we concentrate on sickness absence shorter than 8 days we can, from row 4, see that the parameter estimates is basically the same as for the model with less than 15 days of absence. Hence, all the peer effect is for the non-monitored work absence.

Considering the parameter estimates for the pre experiment period displayed in columns 3 to 8 we can see that all estimates are of lower magnitudes and that none is statistically significant.

In Table 6 we present the results when we study effects of the share of treated on the incidence into work absence and the incidence into a monitored work absence conditional on a work absence spell of seven days. In addition we study the effect on the length of work absence conditional on the incidence. The result from this table is that there is an, about, 4.5 percentage point higher inflow into work absence in a network with 50 percent treated in comparison with workers who don't have any treated peers. We can also see that there is no increased inflow into spells longer than 8 days or that these spells are becoming longer if the share of treated are large.

[Table 6: about here]

In addition to model (1) we also tested for non-linear effect of the share treated on work absence. We could not reject the linear effects specification presented above. We have estimated models where we only control for the share of commuter which gave very similar results as displayed in Table 5. We also made several sensitivity analyses: we tested for the inclusion of higher order term for number of employees and share of commuter and age. In all these tests the inclusion of higher order terms are rejected. We also estimated half year difference models and difference in difference models (2:nd to 1:st difference in 1988 minus the same difference for 1987). These models give qualitatively the same results. The difference estimator gives results that are very close to the results displayed above. The difference in difference estimator gives larger standard errors.

We have found that on average the control individuals increase their work absence more if they have more treated colleagues than if they have few and that the treated do not respond to the share of treated colleagues. We also found that inflow into work absence is on average



increasing with the share treated colleagues. The interpretation of this peer effect is that it stem from a fairness concern for leisure.

## 5.1 Heterogeneity analysis

In this section we extend this analysis of the total population by examining in section 5.1.1 if newly hired (hired in 1988) respond in the same way to the treated colleague's absence as the employees also employed in 1987 and in section 5.1.2 whether women and men respond in the same way.

The reason for studying newly hires is that it is highly likely that it is less easy for a newly hired worker to have an understanding of what is considered as a fair distribution of leisure (i.e. to identify shirking), than it is for more senior workers. Hence, if we find smaller effects for newly hired workers than for more senior this a further indication that fairness plays a role for social preferences. Another reason for studying movers is that the strategy of using mover has, to a large extent, been used to study social interactions in the literature (cf. Ichino and Magi (2000) for a study of social interactions in sickness absence).

The main reason for studying gender differences is that in the experimental literature one have found differences in women and men behavior of inequity and fairness. Croson and Gnezy (2009) review the literature and their conclusion is that the social preferences of the women are more malleable than for the males. That is, their choices are made with greater consideration of the circumstances surrounding their decision Evidence from dictator games suggest that women are more inequality adverse then men but also that women's decision are more contextual specific than men's. Also outside of the lab there is some evidence of gender differences in other regarding preferences. In a news paper experiment Guth, Schimdt and Sutter (2007) finds that women care more about equal distribution than the men.

### 5.1.1 Seniority

The share of treated senior workers will be higher in workplaces with many senior workers. Hence, if the social preferences differ at workplaces with more senior workers may any observed difference in peer effects are from heterogeneous workplace effects rather than differences in peer effects due to seniority. To deal with this we control for the number of newly hired employees when we separately estimate model 1 for newly hired workers and senior (tenure > 1 year) workers.

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In addition, in a sensitivity analysis we also estimated the same model or the pre experiment period.

We have also estimated models where we, instead of the share of treated workers, included the share of treated senior workers and the share of treated newly hired workers. That is, the share of treated is equal to the sum of these two components.<sup>7</sup>

Figure 3 displays the distribution of the share of treated newly hires among all new hires. From this figure we can see a quite large variation. The distribution is concentrated at zero which shows that the newly hired workers are more likely to be hired in work places with high share of commuters.

[Figure 3 and Table 7: about here]

From Table 7 we can see (row 2) that there is no difference between the two categories of workers when it comes to the effect from treatment. There is an overall lower peer effect among the newly hires. In Table A1 in the appendix we present the results from the corresponding placebo regressions. This tables shows, as expected, no effects from treatment or peer effects.

When including the share of treated newly hired workers and treated senior workers we find a statistically significant peer effect for the senior workers from the share of treated senior workers (row 4). The peer effect of the newly hired workers from the share of treated newly hired workers (row 5) are of the same magnitude as the peer effect for the senior workers, this effects is however not statistically significant. Among the controls the peer effects for the two groups of workers (see row 6) are both statistically significant and of the same magnitude: having 50 percents treated peers would increase the number of day absence by 0.5 days on average. Here (see rows 7 and 8), the response for the senior is mainly from senior co-workers whereas peer effect for the newly hired is to the same extent from both senior and newly hires.

Even though the difference in peer effects between the two shares are not large, the results shows that the reference groups matters and that if one is using mobility to identify norms one should, potentially take into consideration the number of new hires at the workplace.

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<sup>7</sup> Note that the share of treated new workers (displayed in Figure 3) is obtained by dividing share of treated newly hired workers with the fraction of new employees.

### 5.1.2 Men and Women

In a gender segregated labor market could observed gender differences in peer effects stem from differences in the types of jobs for women rather than from gender differences in social preferences. In order to take this problem into account we control for the fraction of women at the workplace in all regressions.

In figure 4 we present the share of treated women among the women at the workplace. From this graph we can see that we have quite a large variation in the variable.

[Figure 4 and Table 8: about here]

The results from the estimation of model 1 separately for men and women are given in Table 8. We find (see rows 1 and 2) that the treatment effect is about twice as large for the men as compared to the women (.55 days compared with .27 days). We can also see that the peer effect is more than three times as large for the women (1.27 compared with .39). For the men the effect from the share of treated co-workers is not even statistically significant. We have also run placebo regression for the pre experiment period. We find no statistical significant peer effects, however two statistical significant treatment effects ( $p$ -value  $< 0.05$ ): one with a positive sign and one negative. The estimates are however quite small in magnitude in comparison with the estimates seen in row 1 why we believe that this not jeopardize our empirical strategy

When we instead of the share treated include the share of treated women and men we find (see row 5) that all of the peer effect for the women is from treated female peers. Turning to the effects on non-monitored absence for the control individuals we can, however, see (row 6) that the peer effect for the men is only marginally smaller than for the women (.97 compared with 1.17). Thus, having 50 percent of treated peers would on average increase work absence by around a half day for both men and women. From row 8 we can see that the social interaction effect for the women stems from social interactions with female peers.

The analysis for women and males suggests that there are gender differences. The main difference is that women care about female peers only. Another aspect is that potentially also treated women are affected by the number of treated peers whereas the treated men are not.

## Work in progress - do not quote

In order to increase the understanding of the process we study the effect on work absence longer than 15 days for the treated and controls separately. Furthermore we study the effects on incidence into work absence and into work absence longer than 8 days. The results from the estimations are given in Table 9. From this table we can, as expected, see that treated women respond to the share of treated peers, which is not the situation for the men. Further analysis (not displayed) shows that the effect for the women is only from the treated female peers. For women, also incidence into work absence for both treated and controls are affected by the share of treated peers, however for males the controls are only potentially (t-ratio = 1.50) affected. We do not find any peer effect on the incidence into longer than 8 days spells for both treated and controls (see rows 5 and 6).

[Table 9: About here]

We have hence seen that there are average gender differences. The treated men have on average higher moral hazard effects and this moral hazard is seen to be independent of the number of treated peers. We found peer effects on the non-monitored absence for the male and female control individuals. The peer effect for the women is from females peers. Since the controls inflow into monitored absence is not affected by the share treated is this effect, most likely, neither from negative externalities causing increased illness nor from shared leisure

We also found that the moral hazard effect among the treated women is depending on the share of treated female peers. The question then is if this is an effect of shared leisure among the treated women or from norms and fairness. In order to further our understanding we have estimated models with monthly absence instead of works absence at each half year.

[Table 10: About here]

The results from this analysis are given in Table 10, from which we can see that there is an immediate effect of the treatment on the treated work absence. Before July there is no effect from the treatment but in July the effect is statistically significant and very stable for all months for both men and women. On average is the works absence increased by around 0.10 days for the treated men and by around 0.05 days for the

treated women. With respect to the share of treated peers we do not find any effects for the men, but quite many effects are positive and statistically significant for the women. Disregarding the statistically significant effect in June, we believe that one can see a gradual increasing effect to October and then the effect taper off. This would then suggest that the effect is from norms rather than from synchronized leisure. The problem with this interpretation is of course the statistically significant effect in June. To further our understanding we now estimate models separately for treated and non-treated women. In these models we also include the share of treated women and treated men.

The results from the estimation are displayed in Table 11, from which we can see that the effect in June is only for the treated women and from the association with treated female peers. The question is if this is a result from the experiment (e.g. a pre treatment effect) or if this is just a random event? At the moment we cannot give a definite answer. The table however also shows that the peer effect for the female controls is early on in the experiment period (July – September) whereas the peer effect for the treated females is in the period September – December. Note that in September there is an equally large negative peer effect from the share of treated men why the net peer-effect is zero. Hence the peer effect for the treated is more likely starting in October.

The pattern for the treated females is expected if the degree of moral hazard is formed by studying other treated female's behavior in taking advantage of the 7 days extra of non-monitored sickness absence. The pattern for the female controls could stem from fairness and as a response to the expected behavior of the treated. The effect could then potentially taper off if the response among the treated was lower than expected by the non-treated.

[Table 11 about here]

As the Swedish labor market is highly gender segregated we have tried to make sure that the observed pattern is from gender differences and not from labor market differences by performing extensive sensitivity analyses. One should remember that we have controlled for the fraction of women at the workplace in all of the analyses above, which should take most of the segregation into account.

We have performed the analysis separately for different industry classification according to SNI two digits level. From this analysis we

could not discern systematic differences between more female oriented industry classifications in comparison with more male oriented classifications. We have controlled for income of the individuals in our analyses above however as the response to both treatment and peers could depend on the income and since there are differences in income between women and men may the observed differences be from differences in income rather than from gender differences. We have therefore also estimated models separately for different income quartiles (at both the workplace level, at the gender level and on average). We could not find any statistical significant differences in the treatment and peer effects across the groups.

We hence conclude that the estimated average differences between the genders are most likely from average differences in other social preferences.

## **6 Conclusion**

We utilize a large-scale randomized social experiment to study peer effects and work absence. The experiment reduced the incentives to work by extending the formal monitoring in a sickness absence spell by one week for approximately 50 percent of the workers living in Göteborg, the second largest city in Sweden.

Data is taken from detailed registers of all individuals 16-65 years of age living in Sweden before, during and after the experiment was conducted. It hence, includes information on all employees in workplaces surrounding and within the Göteborg municipality borders. To these data we have matched information on each employee's daily work absence, earnings, educational level, gender and age. The large scale experiment in combination with detailed individual data provides a basis for extensive sensitivity analysis of the identification strategy and enables not only estimation of internal and external valid average estimates but also analysis of differences in social preferences across sub-groups.

We find statistically significant peer effects for the control individuals, that is, the control individuals absence is increasing with the share treated at the workplace. The results from the estimation suggest that if 50 percent of a worker's peers have the option to be absent an extra week would this on average increase his/her absence with 0.5 days. On average is not the treated worker affected by the share of peers having this option. We find no evidence of peer effects on monitored sickness

absence. In the light of our theoretical framework, we interpret the result that the peer effect is stemming from a fairness or equity concern for leisure.

This result is in line with several laboratory experiments which have suggested that social preferences are important for agents' behavior. In these studies it has been noted that social preferences shape decisions among a non-negligible part of the population. Empirical evidence on the relevance of social preferences outside of the laboratory is however scarce. Our work is informed by and contributes to the emerging literature focusing on the social determinants of firm productivity. In two recent, interesting and related studies Bandiera, Barankay and Rasul, (2005) and Mas and Moretti (2009) use data from a fruit picking farm in the UK and 6 US supermarket stores, respectively. Our results, from a much larger and more heterogeneous population, confirm the results regarding the peer mechanism at work found in these two previous studies.

In the analysis of differences in social preferences across sub-groups we found small differences in peer effects depending on the seniority of the worker. We, however, found large behavioral differences between males and females. The peer effect for the women and men controls is of the same magnitude; however women are only affected by their fellow female co-workers. The treated women response to the treatment is larger in workplaces where the share of treated female peers is high, whereas the treated men's response to the treatment is independent of the share treated colleagues (neither men nor women). We also found peer effects for the inflow into sickness absence for the females but no effect for the males.

In order to understand these gender differences we can think of the males being either shirkers or non shirkers. Since the experiment was randomized is 50 percent of the male shirkers treated and 50 percent in the controls. The implication of the grouping into shirkers and non shirkers is that the incidence should not be affected by the share treated and that there should not be a peer effect among the treated. For the women we cannot make the same grouping into shirkers and non shirkers since there is (i) an inflow peer effect and (ii) a peer effect for the treated females. The implication is that female moral hazard is on average formed by how female peers are using the insurance.

This interpretation of female and male behavior has some support in the psychological literature. Gilligan (1982) for instance, argues that fairness is more of an absolute matter of principle for men and that

woman is less likely to be driven by a rigid ethical code. That is, fairness is not, to the same extent than among the men, seen as moral imperative among the women. This result are also in line with results on gender differences in ultimatum and dictator games where there is evidence that women behavior are more contextual specific than the men's (cf. Croson and Gneezy, 2009).

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## Appendix

Table A1: parameters estimates from the OLS estimation of the treatment effects (T) and of the peer effect (Share treated) for senior and newly hired workers and women and men for the pre experiment period on work absence shorter than 15 days.

	1:st half year 1987		2:nd half year 1987		1:st half year 1988	
	Senior workers	Newly hired	Senior workers	Newly hired	Senior workers	Newly hired
T	-.03 (.04)	.06 (.06)	.05 (.04)	.07 (.06)	.04 (.05)	.04 (.06)
Share treated	.04 (.36)	-.23 (.42)	.09 (.34)	-.41 (.45)	.02 (.40)	-.21 (.48)
	Men	Women	Men	Women	Men	Women
T	.02 (.05)	-.10** (.05)	.08 (.05)	.02 (.05)	.11** (.05)	-.04 (.05)
Share treated	.14 (.39)	-.12 (.39)	.01 (.39)	-.08 (.39)	.06 (.45)	.19 (.42)

Note: \*\*\*/\*\*/\*\* denoted statistical significance at 10/5/1 percent level, respectively. The number of co-workers, gender, age education, annual earnings and share commuter, share commuters<sup>2</sup> fraction newly hired workers and fraction women are included as control variables. Standard errors are reported in parenthesis and are cluster adjusted at the workplace level.

## Tables and Figures

Table 1: Descriptive statistics (mean and standard deviation (sd)) subdivided into the group treated (T = 1) and controls (T = 0) for the population working (excluding governmental employees and zero income earners) in Göteborg 1988

	T = 0		T = 1	
	Mean	sd	Mean	Sd
15 days of sickness absence spring 1987	2.88	5.16	2.83	5.08
15 days of sickness absence fall 1987	2.76	5.03	2.75	5.03
15 days of sickness absence spring 1988	3.46	5.54	3.42	5.49
15 days of sickness absence fall 1988	4.01	6.01	4.46	6.79
Share commuters	0.33	0.22	0.34	0.22
Women = 1	0.50	0.50	0.50	0.50
Income (swedish kronor) /1000	94.13	69.40	94.45	69.70
Age	46.71	13.09	46.71	13.09
High Education = 1	0.29	0.45	0.28	0.45
Share treated	0.30	0.14	0.32	0.15
Number of employees/100	17.36	36.09	17.37	36.02
N	119,662		114,686	

Table 2: Descriptive statistics (mean, standard deviation and standard error) subdivided into the group treated (T = 1) and controls (T = 0) for the population working (excluding governmental employees) in a workplace with 10 to 100 employees in Göteborg 1988

	T = 0		T = 1	
	Mean	sd	Mean	Sd
15 days of sickness absence spring 1987	2.74	4.89	2.70	4.87
15 days of sickness absence fall 1987	2.58	4.73	2.63	4.85
15 days of sickness absence spring 1988	3.34	5.27	3.37	5.30
15 days of sickness absence fall 1988	3.86	5.76	4.27	6.52
Share commuters	0.35	0.22	0.36	0.22
Women = 1	0.50	0.50	0.50	0.50
Income (swedish kronor) /1000	96.98	68.39	97.19	68.24
Age	46.69	12.98	46.56	12.99
High Education = 1	0.29	0.45	0.29	0.45
Share treated	0.31	0.13	0.32	0.13
Number of employees	39.42	25.38	39.51	25.34
N	38,001		36,783	

## Work in progress - do not quote

Table 3: Descriptive statistics (mean and standard deviation (sd)) subdivided into different groups depending on the share treated colleagues in the workplaces working population (excluding governmental employees) in Göteborg 1988

Share treated	Group 1 < 1.44%		Group 2 1.44% - 22%		Group 3 22% -33%		Group 4 >33%	
	mean	sd	mean	sd	mean	sd	mean	sd
T	0.52	0.50	0.48	0.50	0.48	0.50	0.51	0.50
15 days of sickness absence spring 1987	2.21	4.41	2.47	4.56	2.70	4.85	2.91	5.08
15 days of sickness absence fall 1987	2.14	4.46	2.35	4.50	2.60	4.79	2.78	4.94
15 days of sickness absence spring 1988	2.76	5.07	3.06	4.97	3.31	5.25	3.59	5.48
15 days of sickness absence fall 1988	3.28	5.68	3.69	5.79	4.02	6.11	4.34	6.38
Share treated	0.00	0.00	0.15	0.05	0.29	0.03	0.43	0.07
Share commuters	0.92	0.14	0.58	0.20	0.35	0.13	0.20	0.11
Women	0.41	0.49	0.39	0.49	0.46	0.50	0.61	0.49
University/high school	0.35	0.48	0.29	0.45	0.26	0.44	0.31	0.46
Income/100	88.59	69.37	104.55	72.22	100.74	71.03	90.30	62.69
Age	44.19	12.18	46.36	12.70	46.57	13.04	46.98	13.12
# employees	28.89	22.96	36.45	25.33	42.28	25.12	39.14	25.36
N	1,835		15,862		27,242		29,845	

Table 4. Difference in difference estimates of a treatment effects.

	2:nd-1:st half year difference in work absence 1987	2:nd-1:st half year difference in work absence 1988	Difference in 2:nd- 1:st half year difference 1988 to 1987
Group 1: share treated < 1.44%	-0.07 (0.15)	0.52 (0.17)	0.59 (0.23)
Group 2: share treated 1.44% - 22%	-0.12 (0.05)	0.64 (0.06)	0.76 (0.08)
Group 3: share treated 22%-33%	-0.10 (0.03)	0.71 (0.05)	0.82 (0.05)
Group 4: share treated > 33%	-0.13 (0.04)	0.75 (0.05)	0.88 (0.06)

Note: standard errors are displayed within the parentheses.

Table 5: parameters estimates from the OLS estimation of the treatment effects (T) and of the peer effect (Sh) on work absence shorter than 1 day and 8 days for the non-treated.

Year	1988				1987			
	2:nd		1:st		2:nd		1:st	
Half year	T	Sh	T	Sh	T	Sh	T	Sh
Work absence	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) 15 days	.41*** (.05)	.80* (.42)	.04 (.04)	.07 (.35)	.06 (.03)	-.09 (.30)	-.04 (.04)	-.03 (.29)
(2) 15 days (T =1)		.47 (.53)		-.38 (.42)		-.36 (.37)		-.19 (.38)
(3) 15 days (T =0)		1.21*** (.48)		.05 (.43)		.21 (.38)		.16 (.36)
(4) 8 days (T =0)		1.03*** (.37)		.04 (.33)		.01 (.25)		0.00 (.23)

Note: \*/\*\*\* denoted statistical significance at 10//1 percent level, respectively. The number of co-workers, gender, age education, annual earnings and share commuter, share commuters<sup>2</sup> are included as control variables. Standard errors are reported in parenthesis and are cluster adjusted at the workplace level. The number of individuals is 72,026 (36,591 35,435 non-treated and treated) and 72,803 (37,008 and 35,795 non-treated and treated) for 1987 and 1988, respectively. Number of workplaces is 5,861 for the experiment period and 5,938 for the 1987.

Table 6: Parameters estimates from the OLS estimation of the effect of share treated on work absence on incidence and conditional durations.

	Effect
Incidence to work absence	0.089** (.041)
Duration given a work absence (N = 18,785)	0.850* .459
Incidence to work absence longer than 8 days	-0.007 (.010)
Duration given a work absence spell longer than 8 days (N = 757)	-.050 (1.67)

Note: \*/\*\* denoted statistical significance at 10/5 percent level, respectively. The number of co-workers, gender, age education, annual earnings and share commuter, share commuters<sup>2</sup> are included as control variables. Standard errors are reported in parenthesis and are cluster adjusted at the workplace level. The number of individuals is 38,001 and the number of workplaces is 5,861.

**Work in progress - do not quote**

Table 7: parameters estimates from the OLS estimation of the treatment effects (T) and the effect of share treated on work absence subdivided into the effects of senior (tenure more than one year) and newly hired workers.

	Senior workers	Newly hired workers
Work absence less than 15 days		
(1) T	.42*** (.06)	.39*** (.08)
(2) Share treated	.98** (.45)	.48 (.64)
(3) T	.42*** (.06)	.39*** (.08)
(4) Share treated senior workers	1.00** (.48)	-.10 (.72)
(5) Share treated newly hired workers	.95 (.75)	1.02 (.66)
Work absence less than 8 days and T = 0		
(6) Share treated	.93** 0.42	1.20** (.59)
(7) Share treated senior workers	1.06*** 0.46	1.02 (.68)
(8) Share treated newly hired workers	.59 (.63)	1.33** (.64)

Note: \*/\*\*/\*\* denoted statistical significance at 10/5/1 percent level, respectively. The number of co-workers, number of newly hires, gender, age, education, annual earnings and share commuter, share commuters<sup>2</sup> are included as control variables. Standard errors are reported in parenthesis and are cluster adjusted at the workplace level. The number of individuals/clusters is 44,048/4,736 (all old workers), 28,755/5,006 (all new workers), 22,413/4,150 (old non-treated workers) and 14,595/4,101 (newly employed non-treated workers).

Table 8: parameters estimates from the OLS estimation of the treatment effects (T) and the effect of share treated on work absence subdivided into the effects of men and women.

	MEN	WOMEN
Work absence less than 15 days		
(1) T	.55*** (.07)	.27*** (.06)
(2) Share treated	.39 (.56)	1.27*** (.50)
(3) T	.55*** (.07)	.28** (.06)
(4) Share treated men	.29 (.66)	-1.04 (.71)
(5) Share treated women	.59 (.72)	2.20*** (.57)
Work absence less than 8 days and T = 0		
(6) Share treated	.93** .42	1.17*** (.45)
(7) Share treated men	1.01 (.57)	-.63 (.66)
(8) Share treated women	.68 (.67)	1.88*** (.51)

Note: \*/\*\*/\*\* denoted statistical significance at 10/5/1 percent level, respectively. The number of co-workers, share of women at the workplace, gender, age, education, annual earnings and share commuter, share commuters<sup>2</sup> are included as control variables. Standard errors are reported in parenthesis and are cluster adjusted at the workplace level. The number of individuals/clusters is 44,048/4,736 (all men), 28,755/5,006 (all women), 22,413/4,150 (old non-treated workers) and 14,595/4,101 (newly employed non-treated workers).

Table 9: parameters estimates from the OLS estimation of the treatment effects (T) and the effect of share treated on work absence subdivided into the effects of men and women.

	MEN	WOMEN
Work absence less than 15 days		
(1) Share treated (T == 1)	-.35 (.78)	1.30*** (.65)
(2) Share treated (T == 0)	1.23*** (.64)	1.24*** (.59)
Incidence		
(3) Share treated (T == 1)	-.072 (.06)	.11*** (.05)
(4) Share treated (T == 0)	.08 (.05)	.09* (.04)
Incidence longer than 8 days		
(5) Share treated (T == 1)	.02 (.02)	.01 (.01)
(6) Share treated (T == 0)	-.02 (.02)	.00 (.01)

Note: \*/\*\*/\*\* denoted statistical significance at 10/5/1 percent level, respectively. The number of co-workers, share of women at the workplace, gender, age, education, annual earnings and

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share commuter, share commuters<sup>2</sup> are included as control variables. Standard errors are reported in parenthesis and are cluster adjusted at the workplace level. The number of individuals/clusters is 44,048/4,736 (all old workers), 28,755/5,006 (all new workers), 22,413/4,150 (old non-treated workers) and 14,595/4,101 (newly employed non-treated workers).

Table 10: Parameters estimates from the OLS estimation of the treatment effects (T) and the effect of share treated on work absence each month during 1988 subdivided into the effects of men and women.

Month	Men		Women	
	T	Share treated	T	Share treated
Jan	-.01 (.02)	.10 (.12)	-.03 (.02)	-.23 (.13)
Feb	.02 (.02)	-.08 (.13)	-.01 (.07)	.07 (.13)
March	.02 (.02)	.17 (.13)	.01 (.02)	-.12 (.12)
April	.05 (.02)	-.14 (.13)	.01 (.02)	.07 (.13)
May	0.00 (0.02)	-.02 (.11)	-.02 (0.02)	.12 (.13)
June	.03 (.01)	.01 (.10)	.00 (.01)	.28*** (.10)
July	.10*** (.02)	.05 (.14)	.04** (.02)	.16 (.10)
August	.08*** (.02)	.10 (.14)	.05** (.02)	.26* (.14)
September	.09*** (.02)	.09 (.15)	.05** (.02)	.26* (.14)
October	.10*** (.02)	.23 (.15)	.05** (.02)	.28** (.14)
November	.07*** (.02)	-.07 (.14)	.04** (.02)	.20 (.14)
December	.11*** (.02)	-.02 (.17)	.05** (.02)	.12 (.16)

Note: \*\*\*/\*\*\* denoted statistical significance at 10/5/1 percent level, respectively. The number of co-workers, share of women at the workplace, gender, age, education, annual earnings and share commuter, share commuters<sup>2</sup> are included as control variables. Standard errors are reported in parenthesis and are cluster adjusted at the workplace level.



Table 11: Parameters estimates from the OLS estimation of the effect of share treated men and women colleagues for the female population. June to December 1988 subdivided into the effects for the treated (T =1) and controls (T =0).

Month\Share treated	T = 0		T = 1	
	Men	Women	Men	women
June	0.04 (.22)	0.22 (.15)	.08 (.20)	.54*** (.15)
July	.14 (.22)	.45*** (.15)	.01 (.24)	-.05 (.17)
August	-.14 (.25)	.59*** (.20)	.03 (.13)	.16 (.21)
September	-.26 (.28)	.52** (.21)	-.61** (.28)	.57** (.23)
October	-.04 (.27)	.22 (.21)	-.05 (.29)	.63** (.22)
November	-.34 (.27)	.31 (.21)	-.19 (.28)	.47** (.23)
December	-.43 (.31)	.05 (.25)	-.22 (.33)	.48* (.25)

Note: \*\*\*/\*\* denoted statistical significance at 10/5/1 percent level, respectively. The number of co-workers, share of women at the workplace, gender, age, education, annual earnings and share commuter, share commuters<sup>2</sup> are included as control variables. Standard errors are reported in parenthesis and are cluster adjusted at the workplace level.

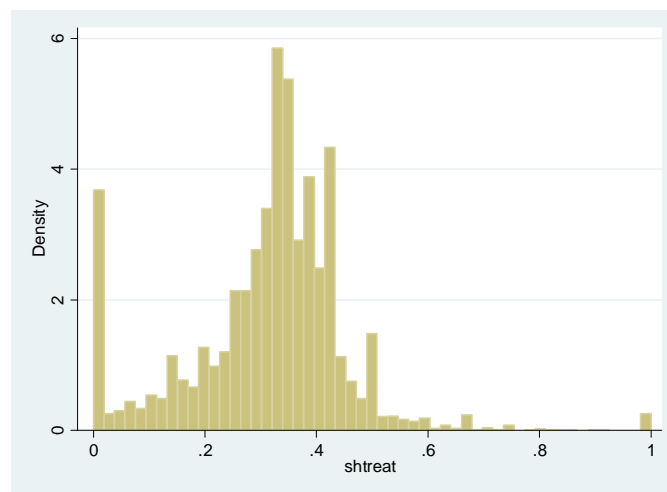


Figure 1 Share treated at each workplace (N = 17,917) for individuals (N = 234,348) living in Gothenborg in 1988. Mean 0.30.

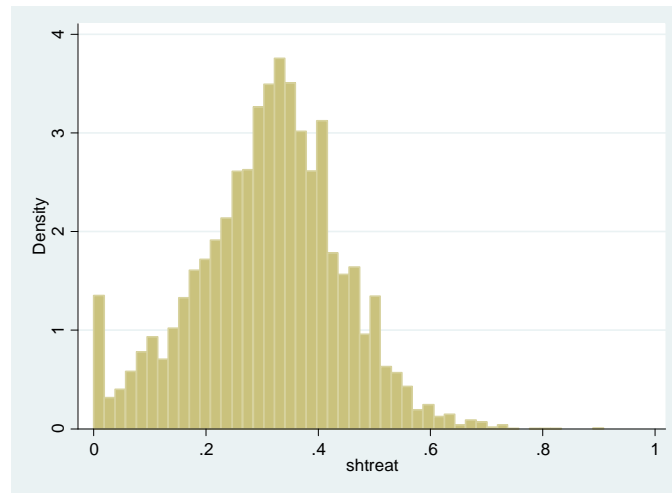


Figure 2: Share treated at each workplace (N = 5,372) for individuals (N = 72,026) working at workplaces with 10 to 100 employees living in Gothenborg in 1988. Mean 0.31.

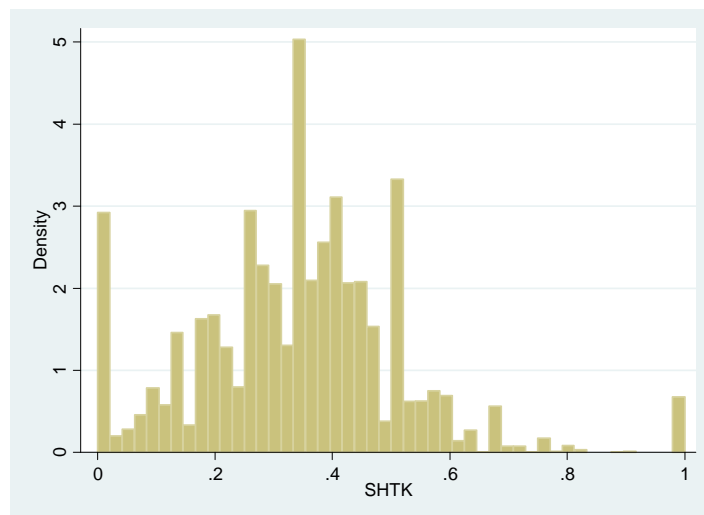


Figure 3: The share of treated newly hires (N = 14,595) for the population analyzed.

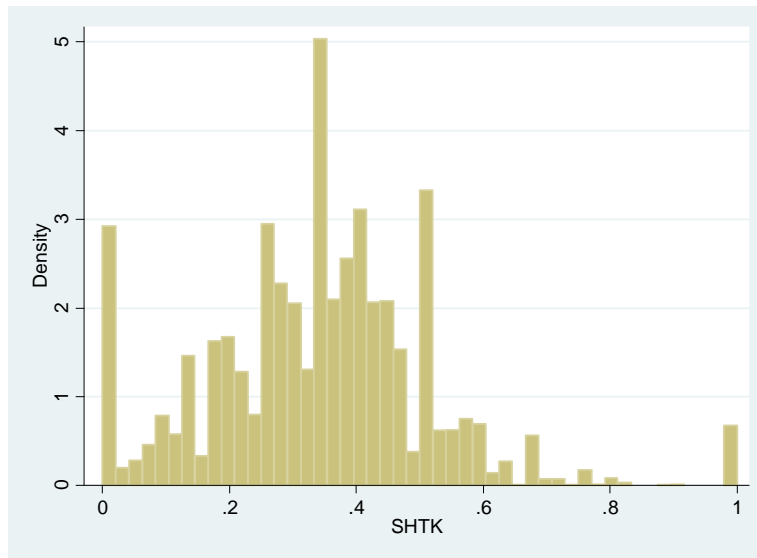


Figure 4: the share of treated women (N = 36,684) for the population analyzed.