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LABOUR MARKET POLICY  
EVALUATION

**Can sickness absence be affected  
by information meetings?  
Evidence from a social experiment**

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# Can sickness absence be affected by information meetings? Evidence from a social experiment

by

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## Abstract

During the last decade several empirical studies have stressed the importance of norms and social interactions for explaining sickness absence behavior. In this context public discussions about the intentions of the insurance, and of the rights and duties of the receivers, may be important for reducing the sickness absence. In this paper we study whether information meetings about the Swedish sickness insurance affect the length of sickness absence spells. The study is based on experimental data on individuals with weak labor market attachments. The displacement of *when* the call to the meeting was sent out was randomized. Comparing the survival functions of those called immediately with those whose calls were delayed (by about 30 days) makes it possible to study whether the length of sickness absence is affected by receiving the call earlier. The result suggests that the length is reduced by, on average, 20 percent. In the long term (12 months later) there is no effect of the information meeting. This suggests that attendance to the information meeting does not change individuals' long-term behavior.

Keywords: monitoring, moral hazard, public social insurance, survival analysis, instrumental variables

JEL: C93, H51, H55, J22

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# 1 Introduction

The high and volatile work absence due to sickness observed in several countries during the last decades has spurred researchers to discuss the importance of norms and social interactions in this context (Lindbeck and Persson, 2010; Lindbeck and Nyberg, 2006; Lindbeck, Nyberg and Weibull, 2003; Ichino and Maggi, 2000). Several studies have shown that norms are an important determinant for explaining the variation in sickness absence across regions in Sweden (Försäkringskassan, 2006; Lindbeck, Palme and Persson, 2004). In a study based on an extensive experiment undertaken in 1988, Hesselius, Johansson and Nilsson (2009) show evidence of social interactions in the workplace; how co-workers affect each other's effort as measured by work absence. Thus, one explanation for the increase of the sickness absence rate observed in Sweden between 1980 and 2000 could be a displacement of the norm for when it is appropriate to use sickness insurance. In this context public discussions about the intentions of the insurance, and of the rights and duties of the receivers, may be important for reducing sickness absence.

This paper studies the effects of an information program run by the local social insurance office in Uppsala County. The objective of the program was to inform sick-listed persons about the rights and duties associated with the sickness insurance. This was done by holding regular information meetings. The target group was everyone on the sick list in Uppsala County. The meetings were mandatory unless the participant was too sick to attend. Valid reasons for not attending were (of course) in-hospitalization and acute illness. Since we only have data on individuals who lack a formal employer, this study focus on individuals with a weak labor market attachment.<sup>2</sup>

This program could potentially both have increased and decreased the use of sickness insurance. For example, receiving a call to a compulsory meeting could be perceived as increased monitoring and thereby reduce the use of sickness insurance. On the other hand, new information from the meeting could both increase and decrease the use of the

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<sup>2</sup> The local social insurance office in Uppsala (from which the data is collected) is divided into different divisions depending on the occupation of the insured people. Unfortunately, we only have data for this study from the division that handled people with a weak labor market attachment, i.e., unemployed or temporarily employed.

insurance depending on the extent of the candidate's knowledge about the insurance previous to the meeting.

The main focus of this study is to estimate the combined effect of the meeting itself and of the call to the meeting on the duration in sickness absence. However, we will also use an instrumental variables estimator to estimate the effect of the meeting on sickness prevalence in the long term. The potential impact on the duration of the sickness can stem from a pre-treatment effect and/or from attending the meeting. We term the pre-treatment effect a "threat-effect", since we believe that such an effect stems from perceived monitoring induced by the call.<sup>3</sup>

The analysis is based on a randomized displacement of *when* an individual received the call. The design is as follows. Case-workers contacted sick-listed individuals via letter to information meetings (IM) about once a month. When the case-worker had selected eligible individuals, half of them received a call as planned and the remainder of the individuals received the call on the next occasion, about one month later. This method enabled us to look into whether the group that received the call early (in their sickness-spell) left their sickness period faster than those who received the call later.

An advantage of this experimental design is that the case-workers could continue to work as usual once the experiment was in place. This has two important implications. First, it was easy to perform the experiment.<sup>4</sup> Second, this design does not imply any substantial ethical considerations.<sup>5</sup> A further advantage of this experiment is that the sick-listed individuals have not been informed about the study, so there is no concern about potential Hawthorn effects, i.e. the participants will not change their behavior because they know that they are participating in an experiment. On the negative side, we can only identify a lower bound of the combined effect of receiving the call versus not receiving it.

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<sup>3</sup> This term is used in the context of tying benefit payments to labor market programs (Black *et al.*, 2003; Geerdsen, 2006).

<sup>4</sup> In social sciences, experimental studies are rare. One reason for this is that they are often associated with practical inconveniences and high organizational costs. With this design practically no extra costs were imposed on the case-workers.

<sup>5</sup> Unequal treatment is an argument often used to prevent implementation of an experiment. With this design, also the control individuals may receive the treatment later.

The result suggests that the duration of sickness absence is reduced by at least 20 percent by the information program. A low attendance at the meeting (30 percent) suggests that a significant part of the effect stems from the “threat”. However, we also find an effect of attending the meeting on sickness prevalence in an intermediate perspective (about 6 months after the call) but no long-term effects (about 12 months after the call).

Our conclusion is that, at least for this population, it is possible to shorten the length of sickness absence with quite small measures. Calling sick-listed individuals to an information meeting is both inexpensive and an easy to implement. However an information meeting, where the intentions of the insurance are discussed, seems not to have changed individuals’ long-term behavior *per se*.

The paper has the following structure. In section 2, we describe the Swedish sickness and unemployment insurances. In section 3, we describe IM and section 4 presents the experiment. Section 5 discusses identification issues. The results and sensitivity analysis are presented in sections 6 and 7, respectively. Section 8 discusses the relative importance of the two components of the combined treatment and, finally, section 9 concludes the paper.

## **2 Sickness and unemployment insurances**

The sickness and unemployment insurances (henceforth SI and UI respectively) sometimes overlap in a way that can generate unintended flows between them (see e.g. Kreuger and Meyer 2002). Several studies have shown that persons progress from the unemployment insurance into the sickness insurance when the replacement rate is higher in the latter (Henningsen, 2006; Autor and Duggan, 2006; Karlstrom, Palme and Svensson, 2007 and European Economic Advisory Group, 2007). In Sweden, the sickness insurance is more generous than the unemployment insurance. Hence, as expected, we have a progression from the unemployment insurance into the sickness insurance (Larsson, 2006, and Larsson and Runeson, 2007). In the following section, we present the differences between the Swedish SI and UI at the time of this study.

## 2.1 Sickness insurance

Compensation from the Swedish sickness insurance consists of two main benefits: disability benefit and sickness benefit. Disability benefit compensates individuals whose work capacity is permanently reduced. Sickness benefit replaces part of the income loss during temporary illness. In this study, the focus is on sick-listed individuals who received sickness benefit.

The sickness benefit is income-related and covers all employed workers. Students and unemployed workers are also eligible as long as they have been employed before undertaking studies or becoming unemployed. Their benefits are based on the wage they received before their studies or unemployment. Furthermore, students must be seeking employment when their sickness absence ends and unemployed people must be registered at a local employment office as a job seeker.

The replacement rates have changed over time.<sup>6</sup> At the time of this study, the first day of sickness was uncompensated. From the second day onwards the benefit was about 80 percent of the earnings up to a limit equal to yearly earnings of about SEK 380,000, which corresponds to earnings in the 90<sup>th</sup> percentile of the earning distribution. The employers paid the benefits for the first 14 days of the illness period. Thereafter the Swedish Social Insurance Agency (SSIA) paid the sickness benefit. For students and the unemployed, the SSIA paid sickness benefit from the second day of the absence.

Within a week, at the very latest on the eighth day of the sickness spell, the claimant must verify eligibility by showing a doctor's certificate that documents reduced working capacity due to illness. The public insurance office then judges the certificate and decides upon further sickness benefit. It is very rare that the certificate is not approved. The certificate contains an expectation of the length of reduced working capacity. In general, a sick-listed individual needs to renew the certificate regularly (about every fourth week) in order to prove continued reduced working capacity.

When an individual has been granted sickness benefit, a rehabilitation plan should be set up. The doctor has the responsibility for the medical rehabilitation, and the potential employer has the responsibility for workplace rehabilitation. If the insured is

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<sup>6</sup> In 2003, the replacement rates in the two insurances were harmonized in order to avoid unintended flows from the UI to the SI.



unemployed, the SSIA has the responsibility for the non-medical rehabilitation. The SSIA coordinates the different rehabilitation plans.

## **2.2 Unemployment insurance**

The UI is administrated by 36 UI funds representing workers from different occupational groups. All together, the UI funds cover 85 percent of the work force and 65 percent of the adult population.

At the time of this study, there were several reasons for reporting sick if unemployed.<sup>7</sup> First, there was at least six-months qualification period within the UI, while the SI provided income replacement from the second day. Thus, people who were approaching the end of a short-term employment (less than months) were insured by SI but not by UI.

Second, for many people the replacement rate was higher within the SI. In both insurances 80 percent of income was replaced up to a limit, but the limit was higher in the SI. Thus, the SI was more generous for those with incomes over the ceiling. In addition, the limit was lowered after 100 days with UI payments, but not with SI payment. In simple terms, in comparison to UI, the maximal replacement was 65 percent higher within SI during the first 100 days of payment, thereafter 74 percent higher (SOU, 2007).

Finally, there were differences in the maximum duration of compensation: unemployment benefit was limited to 300 working days, whereas sickness benefit had no formal time limit.

## **3 Information meetings**

Information meetings have been held between 2005 and 2006 by the Local Social Insurance Office in Uppsala, which will be termed the 'Office' hereafter. To each meeting, 20 to 30 sick-listed individuals were called by a letter sent out about two weeks before the meeting. It was clearly implied in the letter that the meeting offered information only. The letter was short, but it was clear that the information was about

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<sup>7</sup> This section is based on SOU (2007), in which further details about differences between SI and UI are found.

rehabilitation and the rights and duties of the sickness benefit claimant and the potential employer. It was also stated that the meeting would last for about an hour and that participation was mandatory; the called individuals should attend the meeting or contact the local Office and present a valid reason for not attending. Examples of valid reasons for not attending were hospitalization and mental problems implying difficulties to visit a public meeting.

The Office is divided into different divisions. For example, there are special divisions for the insured who are publicly employed, privately employed and unemployed. Sub-areas, outside the city of Uppsala, also have their special divisions for the insured living in the particular area. The experiment was conducted at the division for those without permanent employment and at the division for the small sub-area of Gimo. These two divisions are called “Division for the unemployed” and “Division Gimo”, respectively.

### **3.1 Division for the unemployed**

An individual belongs to the division for the unemployed if they lack an employer with rehabilitation responsibility. The Office obtains this information from the insured’s application for sickness benefit. This application is renewed regularly during a longer period of sickness absence. If, therefore, an individual is employed at the beginning of a sickness period but becomes unemployed during the sickness period, they are transferred to the division for unemployed in the middle of a sickness absence.

Between January 2005 and December 2006, the inflow of all sick-listed individuals into the division for the unemployed was called to IM. Exceptions were if the case-worker had information indicating that it would not be appropriate to call (for example if the individual was in hospital). The calls and the meetings were held about once a month. In total, around 400 individuals per year have been called by this division.

### **3.2 Division Gimo**

Division Gimo oversees all kinds of sick-listed individuals. Eligible candidates were those whose sickness benefit qualifications were unclear. In practice, many of them lack permanent employment. The meetings were organized in the same way as for the

division for the unemployed, but eligible candidates were mostly picked from the stock of sick-absentees. At this division, IMs were held during 2006.<sup>8</sup>

## 4 The experiment

We have created an exogenous variation in the timing of calling sick-listed individuals to IM. When a case-worker had recorded an eligible individual in a digital register, a random generator decided whether the individual should be called at once or whether the call should be postponed until the next occurrence, about 30 days later. The random generator was built into the computer and the assignment was balanced, i.e. there was a 50 percent chance of being called immediately or at a later date. This randomization was repeated every time an IM was planned to be held. Thus, at the next occasion, the case-worker called half of all the new candidates and all of the individuals whose calls had been postponed on the previous occasion.<sup>9</sup> In total, the experiment included 11 subsequent randomizations: eight at the division for the unemployed and three at the local office in Gimo.

### 4.1 Data

The experimental data was collected between May and December 2006 by the Office in Uppsala. This data included detailed information on the experiment: the date for each randomization, when actually called, when the meeting was held, if the insured individual participated in the meeting or not.

We have matched (via a personal identification number) the experimental data with a register, provided by the SSIA, about all individual sickness-absence spells during the study period. From this register, we have selected the first sickness spell during which a randomization took place. In total, the experiment includes 352 individuals. Out of the 352, 275 were actually on sick-leave when a randomization took place. The reason for why about 20 percent do not match is that case-workers selected eligible candidates from registers about two weeks before the actual randomizations. Thus, a significant

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<sup>8</sup> Excluding Gimo from the analysis does not change the main conclusions.

<sup>9</sup> This implies that an individual (who received a call with delay) could have ended their sickness absence before receiving a call.

part of all the individuals called were, hence, not eligible at the time of randomization. All statistics presented in the following are based on the 275 individuals who participated in the experiment and who were on sick leave during a randomization. We observe these 275 individuals until the 20 September 2008. At this point of time 54 people were still claiming sickness leave.

Finally, in order to describe the study population and to check the validity of our results, we have added detailed register information on social background and the sick spell (for example diagnosis at the commencement of the sickness absence).

## **4.2 Study population**

Table 1 presents descriptive statistics on the individuals who have been called and, as a comparison, all other sick spells in Uppsala County during the study period. As the table shows, our study population differs in several aspects from the average insured individual who reported sick. Since we are studying individuals who are weakly established on the labor market, it is not surprising that the study population consists of more immigrants, lesser-educated individuals and individuals with a lower average income. There are fewer women than men in the study population, the average age is lower and fewer individuals are married. Finally, among all the sick-listed individuals, the two most common reasons for sickness absence are related to mental problems or musculoskeletal disorder (the diagnosis at the beginning of the sickness-spell). In the study population, the fraction with a mental diagnosis is larger, compared with the corresponding share among all sick-listed individuals.

Table 1 Descriptive statistics for sick-listed individuals in Uppsala County during the study period

<i>Variables</i>	<b>Experimental data</b>		<b>All sick spells</b>	
	<i>Mean</i>	<i>St. dev.</i>	<i>Mean</i>	<i>St. dev.</i>
Female = 1 if yes	0.53***	0.50	0.62	0.49
Age	44.21**	11.54	46.53	11.73
Immigrant = 1 if yes	0.27***	0.44	0.17	0.38
Post upper sec. education = 1 if yes	0.20**	0.40	0.27	0.44
Married = 1 if yes	0.37***	0.48	0.46	0.50
Divorced = 1 if yes	0.16	0.37	0.16	0.36
Number of children	0.67	1.01	0.75	1.05
Earned income (SEK 100,000/year)	1.36***	2.41	1.68	1.35
Mental diagnosis	0.30***	0.46	0.20	0.40
Musculoskeletal disorder	0.23	0.42	0.24	0.43
Observations	275		18,001	

Notes: St. dev. is standard deviation. \* stat. sign. at 10 percent level, \*\* stat. sign. at 5 percent level and \*\*\* stat. sign. at 1 percent level. Significance levels are based on standard t-test.

### 4.3 A first look at data

Table 2 presents descriptive statistics of the persons who participated in the experiment, divided upon when they were called. As expected, there is no statistically significant difference between the groups with respect to any background variable which allows us to conclude that the experiment was conducted as planned.

Table 3 presents descriptive statistics on how many days the individual had been on sick-leave, both when randomized and after randomization until the end of our follow-up period. The difference between those called at once and those called with a delay is statistically insignificant at the time for randomization, but not after. At the time for randomization, individuals in both groups have been on sick leave for about 100 days. The reason for this relatively long mean duration is that the randomizations took place when a sick-listed individual was assigned into the division for the unemployed (or picked from the stock in Gimo) and not from the inflow into sickness benefit. Further, the variation in the number of days on sick-leave at the time for randomization is large. Descriptive statistics for each randomization separately show that this variation is rather constant across randomizations and, hence, is not explained by seasonal variation.

After randomization there is a difference: those who were called with delay were on average on sick leave 65 days longer than those who were called at once. The difference in median between the groups is 94. This number indicates a positive effect of the program; the probability to leave a sick spell increases when an individual is called to

an IM. However, this should be interpreted with caution since 54 sick spells are right censored and the experiment consists of eleven different randomizations with potentially different distributions of individuals at each randomization. In the next section we discuss identification and estimation in this context.

Table 2 Descriptive statistics by treatment status and test for mean differences

<i>Variables</i>	<b>Called without delay</b>		<b>Called with delay</b>		<i>t-test of difference in means</i>
	<i>Mean</i>	<i>St. dev</i>	<i>Mean</i>	<i>St.dev</i>	
Female = 1 if yes	0.55	0.50	0.51	0.50	-0.52
Age	45.57	11.85	44.84	11.24	-0.52
Immigrant = 1 if yes	0.28	0.45	0.25	0.44	-0.43
Post upper sec. education = 1 if yes	0.20	0.40	0.21	0.41	0.21
Married = 1 if yes	0.40	0.49	0.34	0.48	-1.04
Divorced = 1 if yes	0.18	0.38	0.14	0.35	-0.80
Number of children	0.65	1.01	0.69	1.01	0.40
Earned income (SEK 100,000/year)	1.21	1.20	1.51	3.29	1.02
Mental diagnosis	0.27	0.45	0.33	0.47	1.06
Musculoskeletal disorder	0.24	0.43	0.21	0.41	-0.64
Observations		141		134	

Note: St. dev. is standard deviation and t-test is with respect to differences in means.

Table 3 Descriptive statistics by treatment status

	<b>Called without delay</b>		<b>Called with delay</b>		<b>Mean difference</b> <i>p-value</i>	<b>Median difference</b> <i>p-value</i>
	<i>Mean</i>	<i>Q50</i>	<i>Mean</i>	<i>Q50</i>		
Days on sick leave:						
At randomization	103.55 (85.11)	77.00	112.48 (96.08)	74.50	0.21	0.85
After randomization	293.73 (276.17)	172.00	358.60 (300.47)	265.50	0.03	0.13

Notes: Standard errors are displayed within parenthesis. *Q50* is the median. Equality of means is tested with standard t-tests. A Pearson chi-squared test is performed for the equality of the medians.

## 5 Identification and estimation

The aim is to estimate the combined effect of the call and the meeting. The outcome of interest is the potential effect on the sickness absence duration or the hazard to leave a sickness spell. Although the analysis is based on experimental data, the identification and non-parametric estimation is not straightforward.

Normally, the untreated population is used to estimate the counterfactual hazard or the survival function (i.e., the survival function the treated population would have had in the absence of the call). In this study a traditional control group only exists for about

30 days, i.e., the time period between the first and the delayed call. Thus, we cannot estimate the counterfactual survival function for longer durations than 30 days.

Even though this program affects behavior, we do not expect to observe this on the sickness absence durations instantaneously. The reason is the process of sickness benefit entitlement. Sickness absence duration is determined by how many days the sick-listed individual is entitled to sickness benefit, which in turn is determined by the certificate verifying reduced working capacity. The certificate needs to be renewed after a certain number of days. In general, the medical doctor renews the certificate after a personal meeting with the individual. The certificate is written by the medical doctor, but in practice the sick-listed individual can influence the outcome. The length of sickness absence is, according to Arrelöv, Edlund and Goine (2006), largely controlled by the insured person’s motivation. Englund (2001) also finds that doctors frequently take decisions contrary to their own conviction (e.g. they prescribe too long sickness-absence spells). Hence, any potential effect on sick-spell durations is expected first when the certificate has to be renewed.<sup>10</sup> That is, although behavior is affected instantaneously, we would expect to observe this in sick-spell durations – as ended sick-spells – with a delay. In addition, the length of this delay is heterogeneous among the sick-listed individuals depending on when the certificate has to be renewed.

The experimental design in combination with the delay in when we expect observing the effect hampers us to estimate the effect of the program relative to not receiving it. Alternatively, we estimate the effect of receiving the call immediately relative to about month later.

We start by estimating hazard functions for the two groups. The individuals who were called immediately, we denote as “treated” ( $T = 1$ ) and the individuals who potentially received a call with delay, are denoted “controls” ( $T = 0$ ). The survival functions for these two groups are:

$$S(t | T = 1) = \prod_{s=1}^t (1 - h(s | T = 1)) \text{ and } S(t | T = 0) = \prod_{s=1}^t (1 - h(s | T = 0)),$$

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<sup>10</sup> The individual has the option to end the sickness-absence spell in advance but in practice this happens very rarely.

where  $h(s|T=j)$  is the hazard of population  $j$  and  $s$  denotes the time since randomization.

In order to estimate the effect of receiving the call at once relative to receiving it with a delay, we calculate the difference between the two groups. Under the null hypothesis (no effect), we have that  $h(s|T=1)=h(s|T=0)$  for all  $s$ . Under the alternative hypothesis, we expect  $h(s|T=1)>h(s|T=0)$ , for all  $s < 30$  (i.e., a monotonous treatment effect on the hazard), implying  $\Delta(t) = S(t|T=1)-S(t|T=0) \leq 0$ , for all  $t$ . The potential reduction in days absent due to sickness is calculated as the sum of the differences between the groups from the first day after randomization until the end of study, namely 850 days later. That is:

$$\Delta = \sum_{t=1}^{850} \Delta(t)$$

This estimate provides a lower bound of an effect of receiving the call relative to not receiving it.

## 6 Results

Figure 1 presents the difference between the survival functions of the treated and the controls, evaluated up to 850 days since randomization. Initially, there is no difference between the groups. After about 50 days there is a non-statistically significant negative difference and after about 200 days it becomes statistically significant (at the 5 percent level).

During the follow-up period, the sum of the differences between the groups is 74 days. The mean sickness absence duration (of controls) is 359 days (from Table 3). Thus, for a typical individual in the study population, the sick-spell duration increases by about 20 percent on average ( $74/378=20$ ) if the call is delayed by about one month.



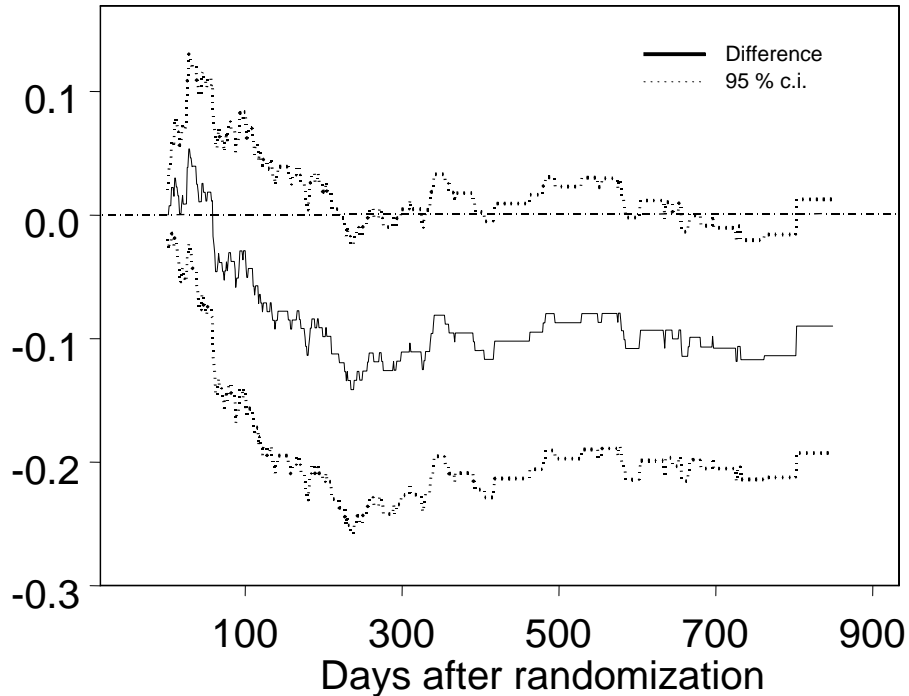


Figure 1 Difference in survival function between treated and controls

Note: 95 percent confidence interval (95 % c.i.) is estimated point wise and in accordance with Greenwood (1926).

## 7 Sensitivity analysis

The experiment consists of 11 sequential randomizations. Within each randomization the number of individuals is small. Thus, a potential concern is whether the treatment and the control group balance in important aspects. In order to address this, we perform three different sensitivity analyses.

First, we perform a log-rank test (Mantel and Haenszel, 1959). That is, we test the equality of the survival functions between the treatment group and the control group by comparing the difference in expected and actual numbers of ended sick-spells.<sup>11</sup> This we do for each of the 11 experiments separately. Combining the differences from all the

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<sup>11</sup> We have applied the log-rank test of equal survivor functions provided by STATA 11. Details about this test can be presented upon request.

randomizations into a single overall statistic generates a statistically significant result at the 10 percent level ( $p\text{-value} < 0.07$ ). The advantage of this test is that it is non-parametric. The drawback is that it does not allow us to estimate the magnitude of the effect.

Second, we apply a matching approach. Using the nearest neighbor without replacement matching estimator (Rosenbaum and Rubin, 1983), each treated individual (within each randomization) receives one control with about the same number of days on previous sick-leave at the time for randomization. Since each randomization does not balance in the number of treated- and control individuals, this procedure excludes 25 treated individuals for which we could not find a suitable match in the control group. On this matched sample, we re-estimate the difference in survival functions between treated and controls. The result is basically the same as in Figure 1 – the estimated difference is 60 days, implying an effect of 17 percent (which is a somewhat smaller effect). Figure A 1 in Appendix 1 presents the result on the matched sample.

Finally, we estimate the effect of delaying the call with the semi-parametrically Cox regression model (Cox, 1972). Cox regression models enable us to control for the different randomization dates, the month of the sick-spell start and individual observed heterogeneity. The drawback is the assumption of a proportional effect over time on the baseline hazard. However, when we test the proportional hazard assumption, we cannot reject the hypothesis of a proportional hazard model.<sup>12</sup> Table 4 presents estimation results of four different Cox regression models. Column (1) shows the estimate without control variables. In column (2) we stratify on randomization date, in column (3) we add dummy variables for the month of sick-spell start and, finally, in column (4) we add individual covariates.

The estimated effects are of the same magnitude irrespectively of model specifications. Hence, the result is robust and the interpretation is that the hazard rate of leaving the sickness absence increases by on average 29 percent when called without delay.<sup>13</sup>

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<sup>12</sup> The test is performed according to Grambsch and Therneau (1994) on the full model with all covariates (those presented in Table 1).

<sup>13</sup> This effect is estimated as  $100 * (\exp(\text{estimate}) - 1)$ .

Table 4 The effect of an early call on the hazard from a sickness absence

	(1)	(2)	(3)	(4)
Early call	0.281 (0.135)**	0.249 (0.138)*	0.211 (0.143)	0.254 (0.146)*
Stratified by randomization	No	Yes	Yes	Yes
Month of sick spell start	No	No	Yes	Yes
Covariates	No	No	No	Yes
Observations	275	275	275	275

Notes: Cox regression models estimated with maximum likelihood. Standard errors in parentheses. \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Under the assumption of no duration-dependence in the baseline hazard, this effect on the hazard is also the effect on the duration. However, the baseline hazard is not constant (see Figure A 2 in Appendix 1). In order to get the effects on the duration, we proceeded as follows. First, we non-parametrically estimate the hazard rate for the controls. Second, we estimate the corresponding hazard rate for the treatment group by simply multiplying the baseline estimate with the coefficient received from the Cox regression model. Third, we estimate the survival functions for the control and treatment groups and compare them. The average sickness-spell duration for an individual in the control group is 378 days. The sum of the differences during this period corresponds to 110 days, giving a percentage effect of about 29 percent (109/378).<sup>14</sup> This is a somewhat larger effect than the one received in section 6. Unfortunately, there is no (as far as we know) possibility to test whether this difference in estimates is statistically significant.

## 8 Monitoring and/or new information

The estimated effect consists of two potential components: increased monitoring through the call and new information through the meeting. These two components probably have different implications for future sickness absence; increased monitoring may affect behaviour temporarily but not significantly in the longer term, while (new) information may affect take-up rates more permanently.

<sup>14</sup> The size of this effect turned out to be the same as the effect on the baseline hazard (the hazard of receiving the call early).

The analysis so far has focused on the combined effect on the probability to leave the ongoing sick-spell. Focusing on the ongoing spell does not allow us to identify separately the effect of the meeting. The reason is that the meeting took place about one month after the randomization and only those who still were on sick leave at this point in time were eligible for the meeting (some individuals had ended their spells, either due to the potential “threat” or simply because of improved health).<sup>15</sup> This implies that those who attended the meeting on average had longer sick-spells than those who did not. Thus, simply including a time varying dummy variable – that takes the value zero before the meeting and one after the meeting – in a hazard regression model would give a biased estimate of the causal effect of the meeting on the hazard from sickness absence. However, we can use the exogenous variation in when the call was sent as an instrument for attendance and estimate the effect of the meeting on sickness absence prevalence. Before a more thorough discussion on this, we present, in Table 5, some descriptive statistics on those who attended the meeting and on those who did not.

According to Table 5, there is no strong selection into the meeting with respect to observable characteristics. There is only one statistically significant difference: fewer individuals with a mental disorder participated in the meeting. Since mental disorder was a conceivable reason to not attend at the meeting, this is no surprise. More important is the fact that – despite the obligation to show up at the meeting – only 30 percent ( $79/(193+79)$ ) of all the individuals who were actually called attended a meeting. This low number suggests that a significant part of the effect stems from simply receiving the call, i.e., a “threat” effect.

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<sup>15</sup> 25 control individuals had ended their sick spells before they received the call.

Table 5 Descriptive statistics by attendance on the meeting status

	Not participated			Participated		
	Mean	St. dev.	N	Mean	St. dev.	N
Female	0.55	0.50	193	0.48	0.50	79
Age	43.02	11.74	193	43.77	11.11	79
Immigrant	0.26	0.44	193	0.28	0.45	79
Post upper sec. education	0.19	0.39	193	0.25	0.44	79
Married	0.37	0.48	193	0.38	0.49	79
Divorced	0.17	0.38	193	0.14	0.35	79
Number of children	0.62	0.99	193	0.73	0.97	79
Earned income (SEK 100,000/year)	1.43	2.78	193	1.18	1.11	79
Mental diagnosis	0.33**	0.47	193	0.22	0.41	79
Musculoskeletal disorder	0.20	0.40	193	0.28	0.45	79

Notes: \* stat. sign. at 10 percent level, \*\* stat. sign. at 5 percent level and \*\*\* stat. sign. at 1 percent level. Significance levels are based on standard t-test. St. dev. is standard deviation.

By regressing IM on an assignment dummy (and all other controls used earlier), we learn that there is a 32 percent increased probability to attend the meeting if the call was received early in comparison with later (see Table 6, column 1). Hence, the exogenous variation in when the call was sent can be used as an instrument for the meeting. However, a caveat is the potential “treat effect” since it also implies a direct effect on sickness prevalence. Thus, we need to assume that a potential “threat” affects the hazard rate at the time of the call, only and, hence, not sickness prevalence in the long term.

The last randomization was performed in November 2006. The control individuals from this randomization received their delayed call in the end of December 2006 implying that a potential “threat” affected their sickness absence behavior in the beginning of 2007. We can observe our study group to the end of 2007 and we estimate the effect of the meeting on sickness absence prevalence the first day in each month in 2007. Thus, if the last randomization (call) induced a “threat effect”, the two stage least squares (2SLS) estimator is biased when applied for outcomes in the beginning of 2007.

Important to note is that if the hazard rates (both from sickness absence and into sickness absence) are heterogeneous, the 2SLS estimator identifies a local average treatment effect (LATE)<sup>16</sup>. This is the treatment effect for the compliers – the treatment effect for those who attended the meeting if they were called early but who would not if they were called with delay. In Appendix 2, we discuss this in more detail.

<sup>16</sup> See e.g. Angrist and Krueger (1999) for a formal discussion of LATE-parameters.

The estimated parameters from the first step, reduced form (RF) and the 2SLS estimators are displayed in Table 6. We present the results without (Panel A) and with control variables (Panel B). The RF and 2SLS estimates are replicated for each month in 2007. First, note that the estimates are not sensitive to the inclusion of the control variables. Hence controlling for covariates is not essential for the inferences why the LATE is, basically, non-parametrically identified.<sup>17</sup> The RF and 2SLS parameters are imprecisely estimated but the point-estimates indicate an informative pattern. In order to illustrate this, we have in plotted the 2SLS estimates (with 95 percent confidence intervals) for each month in 2007. Except from the first quarter, all estimates are negative. An explanation for the initial positive estimates is that the control group in the last randomization reacted on the “threat” in the beginning of 2007 and, hence, did not renew their certificate in the first quarter. In April, there is a statistically significant negative effect. However, from May onwards the point estimates monotonously decreases toward zero. The interpretation is that the meeting reduced sickness absence by 80 percent in April but by only 10 percent in the end of the year. Our conclusion is that attendance at the meeting did matter, at least for the compliers. However, since the estimated effects declined with time, we believe that the meeting affected individual behavior in about the same manner as the threat, namely through increased monitoring. Thus, this program has strong short-run effects but no effects in a longer perspective.

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<sup>17</sup> Due to the small sample size, we refrain from using the non-parametric estimator suggested by Frölich (2007).

Table 6 Estimates from the first step (OLS) and reduced form and two stage least squares (2SLS) estimation on the effects of the meeting on sickness prevalence each month in 2007

First step	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
<b>Panel A: No control variables</b>												
	<i>Reduced form</i>											
0.320 (0.064)***	0.028 (0.115)	-0.029 (0.116)	-0.043 (0.116)	-0.135 (0.063)**	-0.120 (0.061)*	-0.084 (0.061)	-0.069 (0.060)	-0.055 (0.060)	-0.083 (0.060)	-0.054 (0.060)	-0.024 (0.060)	-0.017 (0.060)
	<i>2SLS</i>											
	0.142 (0.435)	-0.073 (0.444)	-0.126 (0.448)	-0.476 (0.260)*	-0.418 (0.250)*	-0.278 (0.243)	-0.222 (0.239)	-0.167 (0.236)	-0.277 (0.241)	-0.163 (0.235)	-0.050 (0.230)	-0.022 (0.229)
<b>Panel B: All control variables</b>												
	<i>Reduced form</i>											
0.316 (0.067)***	0.084 (0.118)	0.033 (0.119)	0.018 (0.119)	-0.124 (0.065)*	-0.111 (0.063)*	-0.079 (0.063)	-0.070 (0.062)	-0.054 (0.062)	-0.071 (0.062)	-0.040 (0.062)	-0.013 (0.062)	-0.002 (0.062)
	<i>2SLS</i>											
	0.378 (0.445)	0.183 (0.446)	0.127 (0.448)	-0.443 (0.266)*	-0.394 (0.255)	-0.268 (0.247)	-0.232 (0.244)	-0.169 (0.241)	-0.237 (0.244)	-0.115 (0.235)	-0.008 (0.235)	0.036 (0.234)
<b>Share on sick-leave</b>												
Overall	0.73	0.65	0.63	0.52	0.50	0.49	0.49	0.50	0.48	0.47	0.45	0.45
Called early	0.74	0.64	0.60	0.45	0.44	0.45	0.45	0.47	0.44	0.44	0.44	0.44
Called late	0.71	0.67	0.65	0.59	0.56	0.54	0.52	0.53	0.52	0.49	0.46	0.46

Notes: standard error within parentheses, \*\* statistical significant at the 5 percent level, \* statistical significant at the 10 percent level.

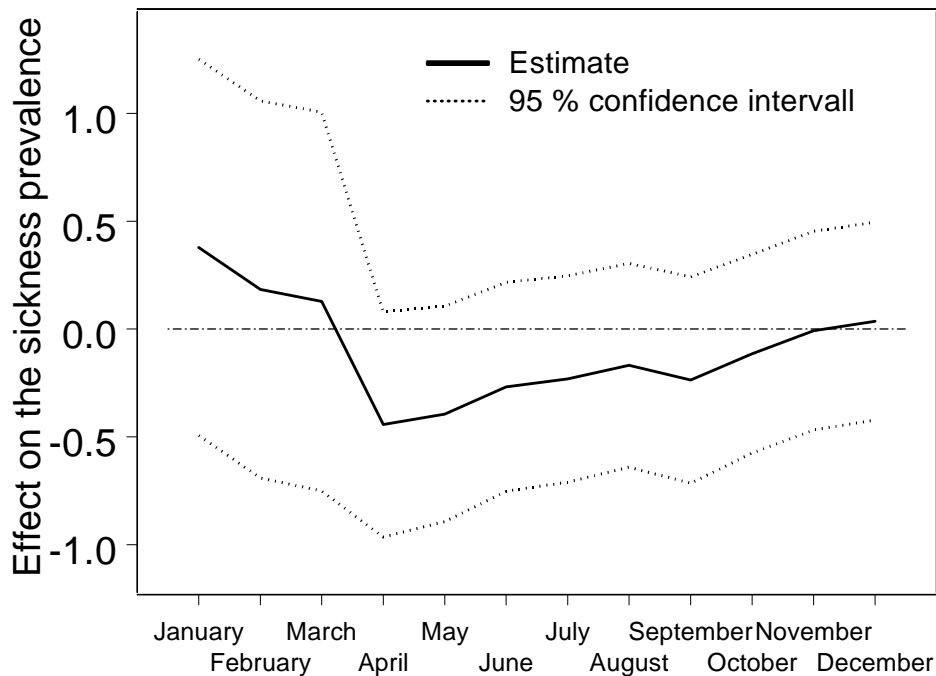


Figure 2 IV estimates of the effect of attending the meeting on the probability of being on sick-leaving each month in 2007

## 9 Concluding discussion

Calling sick-listed individuals without permanent employment (who are weakly established on the labor market) to information meetings about the rights and duties associated with the insurance significantly reduces the length of their sickness absence. Our result, based on a social experiment, suggests that a call would lead to a reduction of the length by, at least, 20 percent on average.

The information program may have affected behavior in two ways: through increased monitoring induced by the call and through the information obtained at the meeting. The low attendance rate at the meetings (30 percent) suggests that a significant part of the effect stems from a “threat”. However, in order to learn more of the behavioral mechanisms we also estimate the importance of the information meeting on sickness prevalence each month in 2007. The result from the estimations suggests that the



meeting also played a role, at least for the compliers (those who attended the meeting when called early but who would not have attended the meeting if called with delay). The estimated effect of the meeting monotonously decreased over time, suggesting strong short-run effects but no effects in a longer perspective. Our conclusion is that lack of knowledge about the rights and duties associated with the insurance is not a likely reason for why this group of individuals was on sickness benefit.

The large combined effect on the hazard should be interpreted with the target group for this study in mind. Individuals in this group were in most cases unemployed, temporarily employed or involuntarily part-time employed. During the last years there have been institutional changes aimed to harmonize the sickness and the unemployment insurance. However, there are still economic incentives for being sick-listed rather than unemployed. The large estimated suggests that moral hazard is a problem among unemployed sick-listed individuals also in the present institutional setting.

For guiding policies aiming to reduce moral hazard we need better knowledge about the behavioral mechanisms associated with the usage of the sickness insurance. This study shows that it is possible to shorten the length of sickness absence with quite small measures. Calling sick-listed individuals to an information meeting is both inexpensive and an easy to implement. However information meeting where the intentions with the insurance are discussed does not seem to change individuals' long-term behavior.

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## Appendix 1

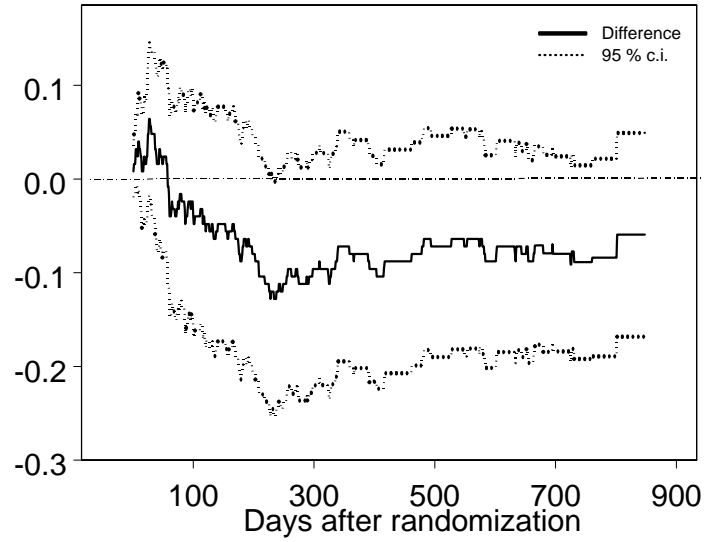


Figure A 1 Difference in survival functions between treatment- and control on matched sample

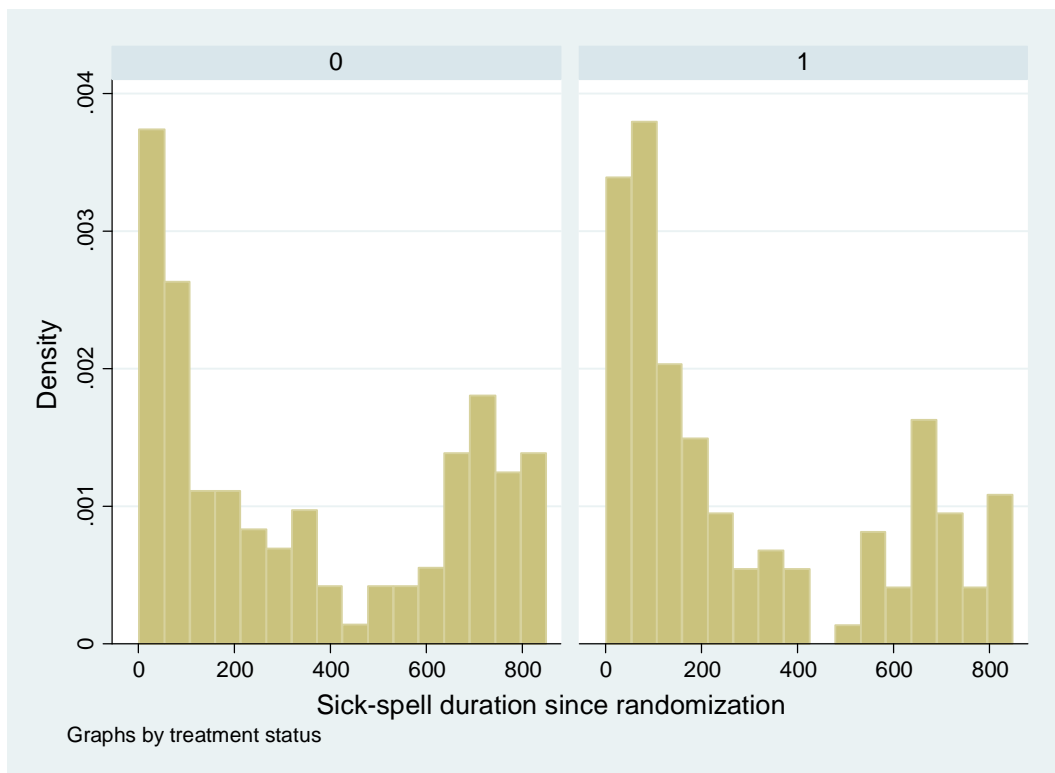


Figure A 2 Histogram of sick-spells durations since randomization by treatment group

## Appendix 2: The problem of identifying the effect of the meeting

Let  $Y_c(1)$  be the potential prevalence of sickness in calendar time  $c$  if in *IM* (that is *IM* is set to 1) and let  $Y_c(0)$  be the potential prevalence of sickness in calendar time  $c$  if *not* in *IM* (that is *IM* is set to 0). For a given individual only one of these potential outcomes are observed. The observed prevalence for individuals in a population eligible of *IM* can hence be written as:

$$Y_c = Y_c(0) + IM(Y_c(1) - Y_c(0)). \quad A.1$$

The potential prevalence of sickness absence in calendar time  $c$  depends on the potential duration in the sickness and the non-sickness state, respectively. For convenience, assume no duration dependence from the sickness state into the non-sickness state or vice versa. Then the probability distribution of sickness prevalence is:

$$\Pr(Y_c(j) = 1) = \lambda(j)^{not} \times (1 / \lambda(j)^{sick}) = \pi_j, j = 0, 1 \quad A.2$$

Here  $\lambda(1)^{not}$  and  $\lambda(0)^{not}$  is the incidence into sickness (i.e. hazard from the non-sickness) if an individual attended *IM* and if he/she did not, respectively, and  $\lambda(1)^{sick}$  and  $\lambda(0)^{sick}$  are the corresponding hazards from sickness (into non-sickness).

Let the prevalence in state  $j$  be additively separable in the population mean,  $\bar{\pi}_j$ , individual  $i$ 's difference, from this mean,  $\pi_{ij}$ , and an idiosyncratic error term  $\eta_{ij}$ . That is:

$$Y_{ic}(j) = \bar{\pi}_j + \pi_{ij} + \eta_{ij}, i = 1, \dots, n. \quad A.2$$

We assume that the individual differences in expectation is zero, that is  $E(\pi_{ij}) = 0, j = 0, 1$ . The average treatment effect is, hence, equal to:

$$E(\bar{\pi}_1 + \pi_{i1} - (\bar{\pi}_0 + \pi_{i0})) = \bar{\pi}_1 - \bar{\pi}_0 = \Delta\pi,$$

By using equation A.1, the regression of prevalence in calendar time  $c$  on  $IM$  can be written as:

$$Y_{ic} = \pi_0 + IM_i \Delta\pi + \xi_i \tag{A.3}$$

where  $\xi_i = IM_i((\pi_{i1} - \pi_{i0}) + (\eta_{i1} - \eta_{i0})) + \pi_{i0} + \eta_{i0}$ .

The problem of estimating  $\Delta\pi$  with ordinary least squares is that  $\xi_i$  contains the individual difference from the population mean,  $\pi_{i0}$ , which also determines the probability to attend  $IM$ . In order to illustrate this, we can, without loss of generality, let the hazard rate from sickness at duration  $\tau$  at the time of the experiment,  $\lambda_i(\tau)$ , be equal to the hazard rate if not attending  $IM$  after the experiment, that is  $\lambda_i(\tau) = \lambda_i(0)^{sick}$ . The duration in sickness absence at the time of the experiment  $T_i$  for individual  $i$  can then be expressed as:

$$T_i = \exp(-\int_0^\infty \lambda_i(0)^{sick} d\tau) \varepsilon_i, i = 1, \dots, n, \tag{A.4}$$

where  $\varepsilon_i$  is an idiosyncratic error. Let  $t_i$  be the time of  $IM$  for individual  $i$  in asynchrone time (i.e. measured at the duration), then the treatment assignment is defined as:

$$IM_i = I(T_i > t_i), i = 1, \dots, n, \tag{A.5}$$

where  $I(\cdot)$  is the indicator function which takes value one when the expression is true. Since  $T_i$  is a function of  $\lambda_i(0)^{sick}$ , the assignment given in equation (A.5) depends on  $\pi_{i0}$  (see A.2 and A.3). Hence:



$$Y_{ci}(0) \not\perp IM_i$$

It is reasonable to assume that individuals with high incidence also have low hazard rates, that is  $\text{Cov}(\lambda_i^{not}(j), \lambda_i(j)^{sick}) < 0$ . An implication of this assumption is that those who attended *IM* also have high prevalence, that is  $\pi_{i0}$  is high for those who attended *IM*. In this situation, an OLS estimator of the average treatment effect of the treated would be downward biased.

### **Instrumental variables estimator and local average treatment effect**

Let  $IM(1)$  and  $IM(0)$  be the assignment into *IM* if called early ( $Z = 1$ ) and late ( $Z = 0$ ).

Then the observed assignments is:

$$IM = IM(0) + Z(IM(1) - IM(0)).$$

Since  $Z$  is randomized it is clear that those with  $Z = 1$  and  $Z = 0$  have the same distributions, that is:

$$(IM(1), IM(0)) \perp Z$$

In order to estimate the treatment effects of the treated using an instrumental variables estimator,<sup>18</sup> the potential prevalence distributions also needs to be independent of  $Z$ , that is:

$$(Y_c(1), Y_c(0)) \perp Z$$

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<sup>18</sup> Note that under homogenous treatment effects, a mean independence assumption is sufficient.

This will hold if any potential threat effect from the call only has a short term effect on the hazard in the duration in which the call was sent. Note that we can write equation A.3 as:

$$Y_{ic} = \pi_0 + IM_i \Delta \pi_i + \nu_{i0}, i = 1, \dots, n \quad \text{A.3}$$

where  $\Delta \pi_i = \bar{\pi}_1 - \bar{\pi}_0 + \pi_{i0} - \pi_{i0} = \Delta \bar{\pi} + \pi_{i0} - \pi_{i0}$  and  $\nu_i = IM_i(\eta_{i1} - \eta_{i0}) + \pi_{i0} + \eta_{i0}$ . In our setting, the individuals who remain in the sickness state until they receive *IM* will, in general, have higher  $\pi_{i0}$  (i.e. lower  $\lambda_i^{sick}(j)$ ) than the population in general implying that the two stage least squares (2SLS) estimator does not estimate the average treatment effect. However the conventional 2SLS estimator will estimate the treatment effect for the compliers (i.e. for those who attend *IM* if called early but not if called late) if there are no defiers (a defier is an individual who attend *IM* if called late but not if called early). For more details on this, see e.g. Angrist and Krueger (1999).

Finally, note that the assumption of no defiers is weak in our setting. Since the *IM* is obligatory, the only reason for not attending is to leave the sickness state. If there is a threat effect and under the assumption that this threat effect is the same irrespectively of when the call is received, these individual are, in this set up, denoted never takers. Hence, the compliers are determined by the idiosyncratic error  $\varepsilon_i$  in A.4; those with a large  $\varepsilon_i$  flow out before the *meeting* if called late but attend the meeting if called early.

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