Author's response to reviews

Title: Work-related psychosocial events as triggers of sick leave - results from a Swedish case-crossover study.

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Author's response to reviews: see over
Re: Manuscript ID 5521901204115356
"Work-related psychosocial events as triggers of sick leave – results from a Swedish case-crossover study" written by Hanna Hultin, Johan Hallqvist, Kristina Alexanderson, Gun Johansson, Christina Lindholm, Ingvar Lundberg and Jette Möller.

Dear Editor,

thank you for considering our paper for publication and giving us the opportunity to revise our manuscript. We have now revised the manuscript and addressed the comments made by the reviewers. The enclosed document contains both the comments made by the reviewers and our answers (printed in italics).

Reviewer 1 questions whether the assumptions of the case-crossover design are met in our study, mainly based on concerns regarding the duration of effect and the risk of recall bias. We have brought the case-crossover design into a new research area, and with such an attempt follow some unexpected experiences. However, we want to clearly state that the assumptions of the case-crossover design are met: The psychosocial events studied are uncommon and marked events occurring at specific points in time, with an effect that is clearly transient. We acknowledge that there may be long-term effects of psychosocial events on the risk of sick leave, however these are not within scope of this study and also not suitable to study using a case-crossover design. We argue that any long-term effect of psychosocial events on sick leave, in our study, is a question of effect-modification rather than an argument against the use of the design. We further have detailed information on exposure in a period of two weeks before sick leave which we use in two different types of analyses yielding similar results.

Quite aside from this concern, the reviewers’ comments have prompted us to make some changes in the manuscript:

- We have included a section about background factors, in which the sources of the different items used in the descriptive analyses are more thoroughly described.
- We have included a discussion on the assumptions regarding the length of the hazard periods and on what effects such assumptions may have.
- We have elaborated the discussion on the risk of reverse causation and confounding from illness and described in detail how these potential problems were handled.
- We have pointed out the possible long-term effect of exposure to psychosocial events and that such an effect may modify the presented trigger effect.

Detailed answers to all reviewer comments follow below. All changes to the manuscript have been made with “track changes”.

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To:
Doctor Fehmidah Munir
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BMC Public Health
We would like to thank the reviewers for their thorough work and insightful comments. If you have additional comments or questions, please do not hesitate to contact us.

Best regards,
Hanna Hultin, on behalf of the authors

**Reviewer's report**
**Reviewer:** Petra Koopmans

This paper describes a relevant question: whether work-related psychosocial events are triggers of sick leave. The use of a case-crossover design in sickness absence research is new, and interesting. However, I wonder whether the assumptions of a case-crossover design in this case are met. Moreover, a causal interpretation is not justified in this study.

**Major Compulsory Revisions**

General and methodological remarks

1. A case-crossover design is used for transient or intermittent exposures. Important assumptions in a case-crossover design include that the exposure should be brief, the time between exposure and event onset should be short and there is little carry-over effect on the exposure. Therefore, this study design can’t be used, for example, to evaluate an intervention which is anticipated to cause cumulative changes in the participants, or to study on the progression of a chronic disease. I wonder whether psychosocial factors can be considered as brief and transient exposures. I do expect cumulative and carry-over effects in for example the relationship with superiors and colleagues, bullying and tasks for which you are not skilled enough or unpleasant work tasks.

Yes, you are right, a cumulative effect cannot be studied, however long-term effects may have acute parts to it that is suitable to be studied with a case-crossover design. Exposures studied should have an intermittent or at least transient effect, but the exposure in itself neither has to be intermittent nor brief. For example, it is possible to study the effect of death of a spouse, in which the crossover from unexposed to exposed status is unidirectional and irreversible. (Maclure M, Mittleman MA. Should we use a case-crossover design? Annu Rev Public Health. 2000;21:193-221.) The exposures we have studied are fairly uncommon events for most of the respondents. The mean percentage of exposed workdays during the previous two months varied between 0.2% (for exposure to discrimination, bullying and harassment) to 15% (for to a very pressured work situation). Furthermore, the respondents appeared to have no problems reporting them as discrete events.
There may be a long-term effect of recurrent exposure to psychosocial events which may exercise its own effect on sick-leave quite apart from the reported trigger effects. In what direction such a long-term effect may go is not for us to tell. It is also possible that the long-term effect modifies the trigger effect, but it does not make the reported acute effect invalid. We have added a section (Discussion, ninth paragraph, 19th line) to point out the possible effect modification from long-term effects of exposure.

2. In a case-crossover design what a case was doing at the time of an acute event is compared with what the case would have been doing usually. In practice, case-crossover designs are limited by the information available on each case’s ‘usual’ behaviour. Extracting such information requires in-depth questioning. The telephone interview on the first day of sick leave may bias the results through recall bias (maybe those who report ill are more aware of psychosocial events just before the sick-leave and less of events more distant in time) or response bias (respondents answering questions in the way they think the interviewer wants them to answer). In this context, I would also be interested whether the interviewers were aware of the hypotheses of the study, and if yes, how this could this have influenced the results?

Yes, we agree that misclassification of exposure, caused by recall bias, is an important problem to consider. Although the events studied here are rare they are also unpleasant, and therefore easy to remember, at least in the perspective of the two weeks under investigation here. As is commonly done, we used two different control periods (the last or last two workdays before the case period and the usual frequency over the previous two weeks), and similar effect estimates are usually regarded as an argument against important exposure misclassification. This is mentioned in the manuscript (Discussion, third paragraph, fourth line).

The interviewers were informed of the general hypotheses for the study. As pointed out in the discussion section (third paragraph, fourth line) neither the interviewers nor the respondents were informed of the specific induction periods, instead they were instructed to pay equal attention to each specified time period.

For response bias to falsely produce the reported over-risks, the respondents would have to over-report exposure in the case period or to underreport exposures in the control period. We find it unlikely that social desirability would lead the respondents to over-report exposure to conflicts, unpleasant work tasks and the likes. On the contrary when asked in open questions, at the beginning and the end of the interview, whether there were any circumstance part from the reported health problem that influenced their
decision to report sick, most respondents answered no. There is paragraph concerning this in the discussion section (third paragraph, 17th line).

3. The length of the hazard period is very important. If the duration is overestimated, many “false exposures” will become “exposures”. If the duration is underestimated, some of the “true exposures” will be excluded. When either of these occur, the association between the event and exposure can’t be evaluated correctly. Why did the authors choose for a hazard period of one and two days respectively?

Thank you for bringing up this interesting and important issue. We based our hypothesis on the Illness Flexibility Model, presented by Johansson and Lundberg, and according to this model the different concepts, specifically adjustment latitude, absence requirements and absence incentives concern circumstances present at the time of or shortly before decision-making. The model states that the induction periods are expected to be short. (Johansson G. The Illness Flexibility Model and Sickness Absence [Thesis]. Stockholm: Karolinska Institutet; 2007 & Johansson G, Lundberg I. Components of the illness flexibility model as explanations of socioeconomic differences in sickness absence. Int J Health Serv. 2009;39(1):123-38.) We were limited to days as shorter time periods were anticipated to raise misclassification. We therefore made the assumption that the hazard period should be at least one day for all exposures. In the data collection we aimed to include several types of control information to assure different lengths of hazard-periods could be tested however, the minimum length of time periods being one workday.

There is a risk that we have used a case period that is shorter or longer than the true hazard period. In both these cases it would lead to an underestimation of the true effect. A discussion on this has been added to the discussion section of the revised manuscript (fifth paragraph, first line).

4. It is possible that the psychosocial pressure increases when an employee is bound to report sick. Thus the (actual or perceived) psychosocial problems increase when an employee is preparing to report sick and tries to oversee the consequences. In that case the decision to report sick causes or plays a role in the work-related psychosocial events in stead of the other way around.

The main concern in case-crossover analyses is the potential for a discrepancy in reporting exposure between the case- and control periods. This possible problem has in essence several different parts to it; First, being ill or feeling symptoms of illness can make an individual more or less prone to, for instance, engage in conflicts with colleagues. Based on our study we cannot conclude which is more likely than the other. Secondly, if making the decision to report sick while at work, communicating this decision may in itself cause psychosocial events. Thirdly, one might perceive situations as
more problematic or pressured when ill. In this last situation, the main discrepancy between the case and control information is then the presence of illness and a case of confounding from illness might arise.

All three circumstances imply that the respondent felt ill and/or had decided to report sick when exposed to the psychosocial events. To investigate this we performed alternative analyses stratifying sick-leave spells into whether the respondent did or did not report experiencing illness symptoms prior to the first sick leave day. As written in the third paragraph of the results section, both results indicate increased odds ratios. We have now further elaborated on these issues in the discussion (seventh paragraph, first line).

5. In the document the authors speak of ‘reporting sick when ill’. I would prefer to use the term ‘reporting sick’ instead. Because the authors did not investigate whether the employees actually were ill or whether the sick leave was due to this illness, except for a self-report interview at the first day of sick-leave.

Our research question concerns the interplay between ill health and non-medical factors. Illness can be defined as the ill health a person identifies themselves with and it is commonly measured with self-reports. All respondents reported during the interview that they were or had been ill, that they had reduced work ability due to illness and that this was the reason (or one reason) for reporting sick. Furthermore, Sweden has an eight-day self-certification rule regarding sick leave, which implies that during these days the assessment of whether or not the work ability is reduced to the extent that sick leave is necessary is actually based on the individually experienced illness and not objective measures of disease or disease symptoms.

We acknowledge that there may be some misclassification in our measure of illness, i.e. persons are on sick leave without being ill, but report being ill in the interview. However, we do not see this as a major problem as we expect such individuals would decline being interviewed rather than attend in an interview which lasted between 45 and 90 minutes. Since our aim has not been to study the triggers of faked sick leave we do not consider this selection to be of major concern.

However, we admit that we cannot with the current design distinguish between the effect of psychosocial events in interplay with illness and the possible independent effect of psychosocial events on sick leave. We have added a section about this in the discussion (tenth paragraph, first line).

Minor Essential Revisions

Background
1. The authors state in the background that “Several of the non-medical factors may operate in a short period before the decision making. The induction period before the effect comes manifest is short and such factors may, therefore, be viewed as triggers”. Is there a proof or reference for this statement?

We regard it as an innate part of the research question that the exposures we aimed to study (conflicts, work tasks etc.) varies over time, and that they are also likely to have an immediate and transient effect.

As stated above (under comment number 3) we made theoretical assumptions based on the Illness Flexibility Model. It states that the induction period for most the concepts in the model are assumed to be short, an assumption which is consequently tested through the use of a case-crossover design. We have added a reference to Johansson and Lundberg in the revised manuscript. (Johansson G. The Illness Flexibility Model and Sickness Absence [Thesis]. Stockholm: Karolinska Institutet; 2007 & Johansson G, Lundberg I. Components of the illness flexibility model as explanations of socioeconomic differences in sickness absence. Int J Health Serv. 2009;39(1):123-38.)

Methods
Data sources
2. How did the interviewers introduce themselves and what said was about confidentiality? Or, in other words, could the employees have had the idea that their answers would be communicated to their employer?

Several steps were made to ensure the participants that their answers would not fall in the wrong hands:

The union representatives at all workplaces were included in the meetings with the project leaders and after information they accepted that the workers were approached for invitation to participation

All workers were invited to information meetings regarding the project where concerns and questions was addressed and answered.

An introduction letter was enclosed to the postal baseline questionnaire. The letter clearly explained both how individual information from the questionnaire eventual future interviews, would treated, that is not be communicated to outside the project administration and that feedback to the working places would solely be on aggregated level.

The workplaces contributed with lists of employees but were not informed about who participated. Instead, they reported all sick-leave spells of all employees to the project, where the project only interviewed those who had confirmed participation through written informed consent.
At each interview, the interviewer took the time to both thoroughly introduce themselves and the project and to remind the respondent that their answers were confidential. The participants could decline at any stage of the process after being approached with the baseline questionnaire.

Exposures
3. How were the questions combined into one measure called ‘problems in the relationship with superior’ and ‘problems in the relationship with colleagues’? When one questions was coded as yes, then the combined score was coded as yes?

Yes, this is properly understood. We have tried to clarify this in the manuscript (methods section, ninth paragraph, sixth line).

Statistical analyses
4. “The odds ratios can be considered as estimates of the relative risk”. This holds only for uncommon events (< 5%).

What we mean is that the odds ratios can be considered as estimates of the incidence rate ratios, nothing else. This has now been clarified in the revised manuscript (methods section, 18th paragraph, first line).

5. Were the uncertain exposures coded as exposed or as non-exposed?

We agree that the description regarding the use of uncertain exposures is somewhat unclear in the manuscript. Thank you for pointing this out.

When the respondents were unable to make a certain statement regarding which specific workday they were exposed, special codes were used to indicate that. It was not a question about the respondent doubting that exposure had taken place just an uncertainty of which day it occurred.

In all analyses of matched control periods we considered such uncertain exposures as exposed.

For basis of the usual frequency analyses, the estimation of usual frequencies for the exposures “discriminated, bullied or harassed”, “unpleasant work tasks” and “pressured work situation” the number of exposed work days are counted and the specific workday is not of interest. However, when calculating the usual frequencies for the exposures which are based on more than one interview question, the uncertain exposures are not used, since the unknown timing of the exposure event during the two week period does not allow for adding the number of exposed days over several questions (i.e. if the respondent thought that the Monday or Tuesday of a certain week was exposed according to one question and that the Tuesday or Wednesday may have been exposed according to another
question, it would be impossible to know if one or two days should be added to the usual frequency). This might have led to an underestimation of the usual frequency for these analyses. For clarification we have elaborated the section concerning how the uncertain exposures were used the revised manuscript version (methods section, 19th paragraph, first line).

6. An estimated sick leave incidence rate was calculated. Over which period this rate was calculated?

The rate was calculated over the total follow-up period, which varied between 3 to 12 months for different workplaces. This has been clarified in the revised manuscript (results section, first paragraph, first line).

Results:
7. “The median self-reported work ability did not differ between exposed and unexposed cases”. Which kind of exposure is meant here, and over which period?

The median self-reported work ability was assessed at time of sick leave and did not differ significantly between sick-leave spells which were exposed and unexposed in the case period to any of the respective exposures under study. We have clarified this in the revised manuscript (first paragraph, ninth line).

8. Figure 1 Usual frequency A) Add ‘excluding the case period’ (as in C)

No, this will not be correct, since in 1b the two-week period prior to sick-leave by definition does not include the case period (the first sick-leave day).

9. Page 10 last sentence: “We performed separate analyses for these exposures, only including cases that did not report illness symptoms before the first sick leave day, which also resulted in increased OR’s. This speaks against exposure being a consequence of illness”. Were the OR’s significant? The conclusion is more appropriate in the discussion.

The mentioned sub-analyses implied excluding 76% of the cases, and the ORs were not statistically significant. This has been clarified in the manuscript (results, third paragraph, eleventh line).

We agree that the conclusion is more appropriate in the discussion, however in the revised manuscript we have deleted the last sentence and instead added a more elaborate discussion on reverse causation in the discussion section (seventh paragraph, first line).
10. Table 3. I suppose the numbers of exposed/unexposed shown in table 3 are the numbers in the case periods. Can the authors add the numbers of exposed/unexposed in the control periods?

*We have added this information in the tables, but to keep the simplicity and readability of the table we have chosen to put the information on number of exposed in case and matched control periods as a footnote of the table.*

Discussion

11. “Confounding from selecting control periods without illness is not a problem, since there is no strong reason to believe that exposure is associated with illness”. I do not understand this sentence.

*This connects to your 4th comment and we agree that is unclearly written. We have now added a discussion on reverse causation, confounding from illness and exposure/perceived exposure in the discussion section (seventh paragraph, first line) and the statement is no longer in the manuscript.*

12. “We expect the triggers studied here to mainly have an effect in the continuum slightly above no work ability and slightly below full work ability”. Please explain this, because it is not clear.

*We regard no work ability as an absolute criterion for not working. Sick leave in case of full work ability means cheating. In between, the reduced work ability due to illness implies a relative indication for sick leave and the decision might also be influenced by non-medical circumstances.*

13. “Nevertheless, our results suggest that the work ability of the included cases at least to some part was situated in the continuum were the trigger factors could have an effect”. I do not understand this sentence and I do not see how this can be concluded from the results.

*In our study, the self-reported health disorders when reporting sick mainly regarded different types of acute infections and the estimated work ability when reporting sick was low. However, we mean that since we see increased risks of sick leave for some of the exposures, the work ability of some of the included cases must have been large enough to allow for other factors to play a role.*

Conclusions

14. ‘when experiencing ill health’. By formulating it this way it seems that all sickness absence is due to ill health. But this needs not to be the case.

*According to the Swedish Sick Leave Act, sickness benefit is granted individuals who have a reduced work ability due to ill health.* (Swedish Sick Pay Act (1991:1074) Available at: [http://www.riksdagen.se/webbnav/index.aspx?nid=3911&bet=1991:1047]) *We agree that*
not all individuals who report sick would qualify if these criteria were checked objectively. However, our research question does not concern triggers of malingering.

We consider sick leave, in the same way as illness, to have a multi-causal background. Ill health which leads to reduced work ability is one major component, but other factors can be of importance too. The degree of ill health (measured as degree of reduced work ability) can be regarded as the dose of exposure to ill health, following the logic that the higher the dose the fewer other exposures are needed to fill out the “causal pie”.

We admit that our measure of illness may have its’ flaws. But we think that participation is less likely if cheating. Please also see the answer to comment 5 for a further discussion.

15. “However, the results imply that psychosocial work-environmental factors appear not only to affect sick leave through health, but also through sick-leave behavior”. I am not sure how this conclusion can be drawn from this study. The methods applied do not allow conclusions about causal mechanisms or mediator variables.

We do think that the case-crossover design applied in this study grants us to draw such a conclusion, although with the stated limitations. However, to tone down the language somewhat we have changed the sentence to “However, the results suggest that psychosocial work-environmental factors appear have a short-term effect on individuals’ decision to report sick”.

Discretionary Revisions
Abstract
Background
Different work-related psychosocial events. I do not understand the word ‘different’ in this context. The word ‘different’ can be removed.

Thank you for pointing that out, we have changed manuscript accordingly.
Reviewer's report

Reviewer: Martin Lindhardt Nielsen

Not being familiar with the case-crossover methodology, I felt quite impatient until I reached the detailed explanation in the methods section on page 8. The expression “earlier work history” in the abstract slightly confused my understanding. Maybe the “hint” at this point could be clearer.

Thank you for pointing that out. We have changed the method section in the abstract to improve clarity.

Details:

Page 1: The names of institutions are given partly in Swedish, partly in English.

Thanks, we have corrected this in the revised manuscript.

P 3: the background paragraph is a bit implicite.

It is not clear to us what part of the background that is implicit. We have gone through the text and made some changes to enhance clarity. Please be more specific if further is needed.

P 5, study sample: The categories “manufacturing”, “health care” and “white collar work” are not really compatible – “administration” or “office work” would fit better with the two first categories. The population description could be clearer: How many employees were sampled in total, and how many were absent for what reasons? What does “sick leave since > 30 days” mean?

Thank you for pointing that out, we have changed the sector name “white collar work” to “office work” as suggested.

Human resource personnel at the different workplaces selected all employees who fulfilled the inclusion criteria at their respective workplace. In total, 3149 individuals were identified and after detailed corrections on inclusion criteria, 3020 individuals were considered eligible and were invited for participation. Employees not meeting the inclusion criteria, for example if they were currently on sick leave since more than 30 days, were not invited for participation. This has been clarified in the revised manuscript (methods, fourth paragraph, first line).

P 6, data sources: what is “a 3-12 month follow-up”? The period from 3 to 12 months after baseline; a variable follow-up between 3 and 12 months for different individuals, or something else?
The period of follow up varied from 3 to 12 months between the different workplaces, and the expression “a 3-12 month follow-up” aims to that. This has been clarified in the revised manuscript (methods, fifth paragraph, third line).

P 7: I wonder if “row” has the same meaning as the original Swedish word used in the questionnaire. According to my dictionary, a row is a particularly noisy or even violent argument or quarrel. This is hopefully rare at the workplace. Is this really what the participants were asked? Similarly, is “brushed aside” the precise expression, or would “ignored” be better/more generally understandable?

After consulting native English speakers we have decided to change the word “row” to “quarrel”. However, we believe the expression “brushed aside” together with “disregarded” make an adequate translation of the Swedish term which we have used in the interview.

P 9, last paragraph: …codes do indicate… “do” should probably be “to”. …cases were the respondents reported….“were” should probably be “where”.

Thank you for pointing this out. We have changed the manuscript accordingly.

P10, results: the duration of follow-up is still not clear.

We have clarified this in the methods section of the revised manuscript (fifth paragraph, third line).

Last line: “This speaks against exposure being a consequence of illness”. Do you mean exposure or/and perceived exposure?

We meant exposure. However, this sentence has been removed from the revised manuscript. Instead we have included a more elaborate discussion regarding reverse causation in the discussion section (seventh paragraph, first line).

P 13, “there is no strong reason to believe that exposure is associated with illness”. Associated in what way? Exposure may cause illness, although it is not the hypothesis in this study. And illness may affect the perception and thereby reporting of exposure (particularly mental health problems could increase the perception of trouble at the workplace).

We admit that this part of the sentence is unclearly written. In this case we meant that given the sub-analyses we made, there is no strong reason to believe that exposure may cause illness. However, we have now added a
discussion on reverse causation, confounding from illness and exposure/perceived exposure in the discussion section (seventh paragraph, first line) and the statement is no longer in the manuscript.

P 14: In the list of psychosocial factors that increase the risk of sickness absence, the authors does not mention decision authority that have been the most consistent predictor in reviews and single studies.

We did not aim to list all psychosocial factors that have been shown to increase the risk of sick leave, but rather to connect the specific psychosocial events that we have studied to previous studies of similar factors. As important and valid as we think the association between decision authority and sick leave is, we do not easily see how it is comparable to the psychosocial events we have studied. However, it might act as an effect modifier and further studies are needed to explore such a relationship.

P 15: line 2, affect should probably be effect line 11; were should probably be where
P 18, ref 9: the word “star” does not belong in the article title.

We have changed the manuscript according to the three suggestions above.

P 20, figure 1 seem to be missing?

It was our understanding that all figures should be uploaded separately. If this is not the case, or if the figures should both be in the manuscript file and uploaded separately we will of course change this.

table 1: The question and answers on self-rated health seem to differ somewhat from SF36 that is more or less standard in this field, but it is not stated in the methods section, where the question comes from (or the other questions).

This question is based on SF-1, the first item of the Swedish version of SF36, however slightly modified to differentiate it from another baseline question regarding the respondents’ health status at the time of filling in the questionnaire. In the revised version of the manuscript we elaborate further on the origin of the baseline and interview questions (methods, 14th paragraph, first line).

P 22: content missing?

No content is missing, only misplaced page break. This has been corrected in the revised manuscript.

P 23: new table/part of table 1?
Please see answer to the previous comment. This is part of table 1. We have corrected it in the revised manuscript.

P 24: “Health disorder” should probably be “health condition”, “health problem” or just “disorder”.

We agree with the reviewer’s suggestion and have changed the term to “health problem” in the revised manuscript.

P 28: Figure 1 has no legend to explain the differences between 1a. and 1b., etc.

This has now been added in the manuscript.