Author's response to reviews

Title: Return-to-work of sick-listed workers without an employment contract - what works?

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Version: 3 Date: 17 May 2009

Author's response to reviews: see over
Dear Editor,

On behalf of all authors, I submit our revised manuscript: “Return-to-work of sick-listed workers without an employment contract – what works?”
We thank the reviewers for their comments. In line with most suggestions made by the reviewers we have revised our manuscript, which is enclosed. We have answered the comments and questions point-by-point. The numbered response to the reviewers’ comments, including the changes that have been made, is also enclosed.
We hope that our revised manuscript will now be accepted for publication.

Finally, I hereby declare that this manuscript is not published in any journal or other citable form and is not under consideration by any other journal.

On behalf of all co-authors,

With kind regards,

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Response to the comments of the reviewers of the manuscript entitled: “Return-to-work of sick-listed workers without an employment contract – what works?”

Response to the comments of reviewer #1:

We want to thank reviewer #1 for the comments and suggestions.

First, with regard to the two comments related to the overall manuscript: We understand that descriptive analysis is less interesting compared to the results of the loglinear multiple regression analysis of occupational health care (OHC) interventions and return-to-work (RTW). However, as stated by reviewer #1, the focus on the vulnerable group of ‘non-standard’ workers is also internationally important. To date, the number of studies concerning these workers are limited. Therefore, in our opinion examining and presenting characteristics of these sick-listed workers are of scientific value. It can contribute to knowledge, i.e. insight into this target group and current OHC practise, needed for the development of adequate, i.e. tailor-made, occupational health care to optimize vocational rehabilitation and RTW of the vulnerable workers with flexible labour agreements.

Next, we agree that this study may also be of interest in the field of health services research. Initially, this manuscript was submitted to BMC Health Services Research. However, the editor concerned suggested to submit the manuscript to BMC Public Health instead. In his opinion our study suited better the scope of public health research. In our opinion, both research areas are relevant. From both a scientific and a social perspective we believe that more attention is needed for adequate OHC for the ‘non-standard’ working population.

Major compulsory revisions

1. Reviewer #1 advises us to include a more detailed description of the OHC by the Social Security Agency (SSA) for sick-listed workers without an employment contract. We agree that providing more contextual information is important to understand better the peculiarities of the Dutch system and the target group in our study. Therefore more information was added to the introduction (see page 5 and 6 of the revised manuscript).

2. For reviewer #1 the outcome variable raises two questions:
   a. Did the questionnaire address separately work with a contract and work without a contract, or was employment in any job classified as RTW? Although the questionnaire did ask about return-to-work (RTW) in work with and without an employment contract (e.g. temporary agency work), for the results working in any type of job was classified as RTW. This explanation was added to the questionnaire paragraph in the methods section (see page 8 of the revised manuscript).
   b. Is it possible to construct a three class outcome: RTW, non-RTW non-sick-listed, non-RTW sick-listed?

We agree that, considering our target group, this would be a relevant division of the RTW outcome. The questionnaire used in this study was modelled after a questionnaire to examine OHC among employees. When looking at sick-listed employees recovery in principle coincides with actual RTW. However for workers without an employment contract, due to the absence of a workplace to return to, recovery can occur without actual RTW. Unfortunately, no information regarding recovery with and without actual RTW was asked in the questionnaire. Therefore with the collected data the proposed three class outcome division cannot be made.
3. Reviewer #1 asks which population data are used for the non-response analysis? The sample of 3,500 persons was random drawn by the SSA from a total population of 14,854 workers without an employment contract, who had reported sick between the first of August and the end of October of 2003 and were at baseline at least 13 weeks sick-listed. On basis of the population data, provided by the SSA, we looked at the possibility of selectivity of the response (N=1077). There were no important relative differences between the response data used in this study and the available population data as provided by the SSA. Therefore, we concluded that the non-response didn't harm the reliability of the data used in this study. We understand that this was confusing in the manuscript, so we changed this in the revised version (see page 7 and 13).

4. For reviewer #1 it is unclear whether the low rate of OHC interventions is due to a low rate of visits or due to deficient practise of OHC professionals. This should be clarified/discussed. Reviewer #1 raises a vital point. Based on our study it is not possible to determine what contributed most to the found low rate of OHC interventions. However, it is possible that the low rate of OHC interventions in this study may be (partly) explained by a low rate of visits. Summoning to consulting hours was reported by only 54% of the respondents. Since all respondents were at least 13 weeks sick-listed they all should have been invited to visit the insurance physician at least once. From this perspective it is likely that a low rate of OHC visits contributed to the low rate of applied OHC interventions. On the other hand, OHC interventions such as making of a RTW action plan, and discussing actual starting with work again are obligatory in the Netherlands, as mentioned in the manuscript. When looking at these interventions, they were reported by 19% and 28% of the respondents respectively. If a low rate of visits to the SSA would be the main reason for the low rate of OHC interventions found, the rates found for obligatory OHC interventions should be closer to the reported rate for visits. Since this was not the case, it is likely that a low rate of visits to the SSA is only part of the explanation. Insufficient OHC practise by the professionals of the SSA also seems to be an important factor. We have added this explanation to the discussion of the revised manuscript (see page 15).

5. Reviewer #1 notes that the findings in our study cannot be used to argue that OHC interventions promote RTW. Causality of the found associations needs to be investigated with other study designs. We agree that the results of this study cannot be used to state that OHC interventions promote RTW. Other study designs are needed to investigate the causality of the associations found in our study. Therefore we left the statement ‘receiving less occupational health care interventions could have influenced RTW’ out of the discussion in the revised manuscript (see page 15).

6. Reviewer #1 notes that the chapter ‘Policy implications’ in the discussion should be rewritten, because presentation of current and future research plans cannot be considered as policy implications. We understand from the comment of reviewer #1 that the title ‘Policy implications’ in the discussion was confusing since it did not match the content of the chapter concerned. Although in our opinion the content of current and new research programs
can be seen as part of policy agreements, we agree with reviewer #1 that the scope of policy with regard to OHC and RTW of the vulnerable working population is broader. From this perspective, the title of the chapter does not cover its content. Instead of rewriting the chapter, we have chosen to change the title. Because our main goal of this part was to discuss implications with regard to current and new research. In the revised manuscript this chapter is called: ‘Research challenges for present and future’ (see page 18 of the revised manuscript).

Minor comments

1. Reviewer #1 notes that in the abstract abbreviations (RTW, OHC) are used without explanation. We corrected this in the revised manuscript (see page 2).

2. For reviewer #1 it is not clear what type of worker means (in methods paragraph of abstract)?
   We agree that this is confusing without an explanation. The workers in the cohort were divided in the following categories: temporary agency workers, unemployed workers, and remaining workers without an employment contract. This explanation has been added to methods paragraph of the abstract in the revised manuscript (see page 2).

3. For reviewer #1 the focus of the first sentence of the introduction is unclear, the syntax is to complex.
   To make the opening sentence of the introduction clearer we have changed it. See page 4 of the revised manuscript.

4. Reviewer #1 notes that the paragraph about temporary agency work does not belong to the Social Security System chapter. We agree and this has been addressed in the revised manuscript. The paragraph about temporary agency work is now presented as a separate chapter (see page 6).

5. The first sentence of the section Methods (“This study was conducted from May 2004 until June 2004”) is included in the text below (see page 7 of the revised manuscript).

6. Reviewer #1 comments that the abbreviation TNO (in the section Methods) should be explained. TNO stands for the Netherlands Organisation for Applied Scientific Research. This explanation has been added to the revised manuscript (see page 7).

7. Reviewer #1 notes that table legends may not include abbreviations. This is corrected in the revised manuscript (see page 30).

8. Reviewer #1 notes that in figure 1 the number of ‘remaining workers’ grows from 300 to 301 after response and asks how this is explained?
   The explanation for this is the following: the random sample of 300 clients were drawn from the SSA database out of all clients registered as ‘remaining worker’. After receiving the questionnaires the subgroups were formed, i.e. redivided, based on the type of worker as reported by the clients. Of the 1179 respondents 301 reported being a ‘remaining worker’, i.e. not a temporary agency worker or an unemployed worker (the explanation for this is that a client initially registered as a temporary agency worker or an unemployed worker by the SSA had become a ‘remaining worker’ at the time of receiving the questionnaire). Next, the respondents who reported having a full
disability pension or an employment contract were excluded. This resulted in 235 remaining workers without an employment contract. In the revised manuscript an explanation regarding the sample and the response was added to the section methods (see page 7 and 8) and the following change was made in figure 1: the subgroup after drawing the random sample and after response is named ‘remaining workers’. After analysing this subgroup is named ‘remaining workers without employment contract’.

9. For reviewer #1 the classification of p-values with stars in table 4 is unnecessary. This point has been addressed in the revised manuscript (see page 34).

Response to the comments of reviewer #2:

We want to thank reviewer #2 for the comments and suggestions.

Minor essential revisions

1. Reviewer #2 comments that in the section methods of the abstract it has to be clear that the data are cross-sectional. This explanation has been assed to the abstract (see page 2).

2. Reviewer #2 notes that there are a number of studies that have looked at occupational health care RTW interventions and we did not review these. Although our study is specific to the Dutch system and workers without a contract, it is difficult for reviewer #2 to see how it compares to other interventions and what novel contribution our study is making. As already mentioned in the discussion, studies with a focus on OHC and RTW of the vulnerable group of sick-listed workers without an employment contract are rare in the international literature. However, we agree that adding more information about other studies regarding occupational health care interventions and RTW is a valuable addition to the manuscript. This has been addressed in the revised manuscript (see page 17).

3. Reviewer #2 asks to outline any ethics procedure/approval in the section Methods. Also reviewer #2 asks to explain what TNO means.

First, regarding the ethics procedure/approval, this study was commissioned by the Dutch Ministry of Social Affairs and Employment and conducted by the Netherlands Organisation for Applied Scientific Research (TNO). The sample was random drawn by the Dutch Social Security Agency (SSA) from their database. This was clarified in the methods section of the revised manuscript (see page 7). And second, TNO is the Netherlands Organisation for Applied Scientific Research. This was also clarified in the revised manuscript (see page 7).

4. Reviewer #2 notes that we carried out loglinear regression analysis, but in the manuscript we refer to the analysis as ‘multiple regression analysis’. Reviewer #2 finds this misleading and asks to adjust this and refer to loglinear analysis. We understand that referral to our analysis was not consistent throughout the manuscript. This was corrected in the revised version.

Next, reviewer #2 asks for more information regarding the interaction effects, namely What interaction effects? How were these entered into the model? And where are they reported? Reviewer #2 also wants to know what confounding variables were entered?
Are these the same as the background variables? Finally, reviewer #2 comments that parameters of the model (e.g. it is a good model?) should be reported.

In the first step of building the model the possible determinants were selected one by one for significance. The determinants in our model were the five examined occupational health care interventions. The possible confounders were the measured demographic characteristics, i.e. the background variables. Next, these possible confounders were added to the model one by one. If a possible confounder altered the beta coefficient of one of the selected determinants with 10% or more, this confounder entered the model. This resulted in five confounding variables to enter the model, namely type of worker, age, and perceived health (past, present and future). Next, we looked if there were relevant interactions between these confounders and the determinants in our model, i.e. the examined occupational health care interventions. No interaction terms were included in the end model, since none of the studied relevant interactions between confounders and interventions were significant. To clarify we reported the confounding variables included in the model on the bottom of table 4 (see page 34 of the revised manuscript) and added an explanation to the revised manuscript (see page 12). 

Finally, we can report that the Nagelkerke R Square of our model is 0.364; i.e. the explained variance with the determinants in our model is 36%.

5. Reviewer #2 argues that in the results section ‘Baseline characteristics of the cohort’ it should be clear whether only observations are reported or analysis was carried out. In answer to this comment, we can explain that in table 1 the analysed frequencies for gender, age, type of worker, and level of education are presented. This explanation was added to the revised manuscript (see page 10).

6. Reviewer #2 comments that the results section ‘Perceived health and RTW at 7-9 months after the start of sick leave’ is purely descriptive and suggests reducing this section. As reviewer #2 notes this description of the results was one of our study objectives. However, we agree that we can tighten this section. This has been addressed in the revised document (see page 11 and 12). 

Next, reviewer #2 asks to enter N for each reported percentage in the results section ‘content and frequency of the applied occupational health care interventions’. In the revised manuscript we have entered N for each reported percentage. Although these numbers can be derived from the table, we understand that adding this to the text increases the readability. The range of missing values for the reported occupational health care interventions is in our opinion acceptable, i.e. between 3.1% and 4.6%. Therefore, these are not included in the reported percentages and N-values in the results section.

Finally, reviewer #2 comments that an appendix outlining what each of the examined OHC interventions consist of would be useful. We agree that more contextual information regarding the examined OHC interventions in our study is a valuable addition to the manuscript. This has been addressed in the revised manuscript (see page 9 and figure 2).

7. Reviewer #2 comments that the main objective of our study does not come across strongly enough in the discussion. And suggests starting with reiterating our main objective and discussing how the RTW figure for non contracted workers compares to sick-listed employees with similar illnesses. Next, discussing the results of the
loglinear analysis and where appropriate discussing these findings in relation to the literature review on occupational health interventions.

We have taken into account the valuable suggestions made by reviewer #2 and rewrote part of the discussion. In the revised manuscript we repeated our main objective and main findings in the first part of the discussion (see page 14 of the revised manuscript). With regard to a comparison with the RTW figure of sick-listed employees, we added this to the discussion (page 14 and 15). At 7-9 months after the start of sick leave only 19% of the workers without an employment contract had returned to work (7% partially and 12% completely) compared to 81% (31% partially and 50% completely) of the employees (Ybema et al., 2004). However, we have to remark that the observed difference in RTW rate cannot be explained with certainty based on our study. Other study designs are needed to address this question. Finally, findings of a literature review on OHC interventions and RTW was added to the discussion (see page 17 of the revised manuscript).

Discretionary revisions

8. Reviewer #2 suggests to change the description of the perceived health status from ‘even bad’ to ‘poor’ or ‘fairly poor’. We have changed this throughout the manuscript, including the abstract.