

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Sickness absence as a predictor of disability retirement in different occupational classes: a register-based study of a working-age cohort in Finland in 2007–2014
AUTHORS	Salonen, Laura; Blomgren, Jenni; Laaksonen, Mikko; Niemelä, Mikko

VERSION 1 – REVIEW

REVIEWER	Thomas Clausen National Research Centre for the Working Environment
REVIEW RETURNED	02-Dec-2017

GENERAL COMMENTS	<p>It has been a pleasure to review this manuscript!</p> <p>The authors must be commended for thoroughly preparing the manuscript prior to submission, which makes the review process delightfully easy!</p> <p>I only have few minor comments that must be addressed in a revision of the manuscript.</p> <ol style="list-style-type: none">1. The authors need to clarify the predictor variable further. As I understand, sickness benefits are paid after a ten consecutive working days with sickness absence and that the sick listed person must have absence certified by a medical doctor. As I understand, the predictor variable is specified as registered sickness absence starting in the year of 2005. This should be made absolutely clear in the methods section.2. As I read Table 1, the median number of sickness absence days is very high in all job groups. So, is it correctly understood that the median number of absence days varies between 31 and 60 days for members of the five job groups?3. Results are very interesting! I think the authors could do more justice to the results in the discussion section – for instance by discussing more thoroughly that a significantly elevated risk for DR is found for all types of sickness absence periods.4. Finally, it is a remarkable strength of the study that it solely is based on registered information. However, as the authors correctly points out this leaves us with little knowledge on the importance of various work characteristics in the observed association. In the light of the differences observed between job groups the different configurations of physical and psychosocial work characteristics could be discussed on the basis of evidence from previous findings. <p>I wish the authors the best of luck with the revision of the present</p>
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	manuscript.
REVIEWER	Cécile Boot VU University Medical Center Amsterdam, the Netherlands
REVIEW RETURNED	18-Dec-2017
GENERAL COMMENTS	<p>This is an interesting paper on an excellent data source. I have 2 key issues that require attention:</p> <ul style="list-style-type: none"> - It is not clear what this paper adds to our understanding of sickness absence and disability retirement - Differences between occupational groups are not tested, and confidence intervals are largely overlapping, so the conclusion about differences between occupational classes is not supported by the data - The authors treat diagnosis categories as effect modifier (stratified analyses) as well as confounder (figure 3). A clear conceptual model for the hypothesized role of diagnosis is missing. <p>Major remarks:</p> <p>Please provide a stronger rationale for what this study adds to the existing literature. It is widely known that long term sickness absence predicts disability retirement. What does this paper add to our knowledge?</p> <p>Please provide a rationale for men/women differences in the introduction and make this more clear in the objective of the study. Please provide more information about how a person outside employment can receive a disability retirement? Sickness absence should have taken place from a situation in which a person was employed, given your definition ‘income losses caused by work incapacity’?</p> <p>Why did you choose to include sickness absence starting in 2005 and disability pensions from 2007? What happens to persons who start their sickness absence in January 2005, and were entitled for disability retirement from January 2006? Where they excluded?</p> <p>Please provide a clear definition of the population of workers. Employee (working for an employer) is different from worker (including self-employment); from Figure 1 it becomes clear that self employed persons are included. Please be careful with terminology to avoid confusion (persons/workers/employees/self employed).</p> <p>Were 824,915 persons dropped from the sample due to missing information? That is about half of the sample! Please provide more information about exclusion of persons vs. missing information, e.g., in a flow chart.</p> <p>What is the rationale for the categories of length of sickness absence?</p> <p>Please provide more information about the categories of occupational classes (upper, lower, manual, non manual), or give at least a reference, as this can not be reproduced with the present information.</p> <p>Results: please avoid reference to differences between diagnostic groups, as the confidence intervals largely overlap. This implies that the difference is not statistically significant</p> <p>Given the large sample size, 99% confidence intervals would be more appropriate.</p> <p>I do not see the added value of adjusting for diagnosis, given the fact that stratified analyses were chosen. Figure 3 is redundant.</p> <p>Discussion: the diagnosis categories are very broad, and perceived limitations in work are not known. This should be discussed as a limitation to the study.</p> <p>P12/line 53: I do not consider missing data of 824,915 participants</p>

	(as written in the methods section) as 'very little missing information'. Please explain. Appendix 2: should 2.9 be 2.90? and 1.50 instead of 1.5?
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VERSION 1 – AUTHOR RESPONSE

Editor's comments to author:

1. Please replace '1.7 million Finns' with the country in the title. Please also make sure you add in the word 'cohort'.

Thank you for the suggestion. We have now revised the title as suggested.

2. Please include any relevant quantitative results in the abstract results section.

Thank you for this suggestion. We have now revised the abstract and added some quantitative results as suggested (Abstract, results).

Reviewers' comments to author:

Reviewer 1

1. The authors need to clarify the predictor variable further. As I understand, sickness benefits are paid after a ten consecutive working days with sickness absence and that the sick listed person must have absence certified by a medical doctor. As I understand, the predictor variable is specified as registered sickness absence starting in the year of 2005. This should be made absolutely clear in the methods section.

Thank you for these remarks. The reviewer has understood correctly the way in which the predictor variable, the sickness absence days, was measured. We have now further clarified the description of the predictor variable (Methods, Measurement of sickness absence, 1st paragraph)

2. As I read Table 1, the median number of sickness absence days is very high in all job groups. So, is it correctly understood that the median number of absence days varies between 31 and 60 days for members of the five job groups?

The reviewer has understood correctly about the median number of the sickness absence days. The reason for the seemingly high median numbers is that they were calculated among those who had at least one new sickness allowance spell that started in 2005. We have now clarified this in footnote 1 to Table 1. It is to be remembered that our analyses focus on rather long sickness absences measured through receipt of sickness allowance. Thus, as the waiting time for sickness allowance is 10 working days, all those who had received any sickness allowance had had more than 10 sickness absence days. On the other hand, if also those with no new sickness absence spells in 2005 would have been included in the calculation of medians (the majority of the study population: 91% of men and 88% of women), the median number would have been zero in all categories of socio-demographic explanatory variables.

3. Results are very interesting! I think the authors could do more justice to the results in the discussion section – for instance by discussing more thoroughly that a significantly elevated risk for DR is found for all types of sickness absence periods.

Thank you for this suggestion. We have now added more discussion on the results concerning all types of sickness absence periods (Discussion, Main findings, 1st paragraph)

4. Finally, it is a remarkable strength of the study that it solely is based on registered information. However, as the authors correctly points out this leaves us with little knowledge on the importance of various work characteristics in the observed association. In the light of the differences observed between job groups the different configurations of physical and psychosocial work characteristics could be discussed on the basis of evidence from previous finding.

Thank you for these remarks. We agree with the reviewer that different work characteristics may explain some of the occupational class differences on the association between the length of sickness absence and the risk of disability retirement. Some of these factors are discussed in the “Interpretation of the results” section, and we have further added some clarifying points in the manuscript (Discussion, Interpretation of the results (paragraph 2) and Methodological considerations).

Reviewer 2

1. It is not clear what this paper adds to our understanding of sickness absence and disability retirement.

We have now further emphasized what this manuscript adds to the understanding of the associations between sickness absence and disability retirement (Introduction, paragraphs 3–4). The main contribution of this manuscript is the examination of occupational class differences in the association between sickness absence and disability retirement. To our knowledge, there are no previous studies on this matter. A deeper understanding of the process of weakening work ability in different occupational groups is needed in order to be able to better focus the measures targeted against permanent disability.

2. Differences between occupational groups are not tested, and confidence intervals are largely overlapping, so the conclusion about differences between occupational classes is not supported by the data.

Thank you for these remarks. We agree that in the figures presented in the manuscript, the confidence intervals seem to largely overlap between occupational classes. However, differences in the association between the length of sickness absence spells and disability retirement may be better seen in Appendix tables 1 and 2, which show that confidence intervals between occupational classes do not always overlap, thus the occupational class differences are clear. Furthermore, we have now added results of Wald tests for statistical significance of interactions between the length sickness absence and occupational class to the manuscript (see Methods, Statistical methods and Results (paragraph 3), and Appendix tables).

3. The authors treat diagnosis categories as effect modifier (stratified analyses) as well as confounder (figure 3). A clear conceptual model for the hypothesized role of diagnosis is missing.

Thank you for this point. In our study, we had three research questions. Diagnosis of sickness absence was used in research questions 2 and 3. The research question 2 examines how the length of sickness absence due to different diagnostic groups predicts disability retirement in different occupational classes. Thus, in this stratified analysis, diagnosis was used as an effect modifier. The research question 3 asks whether the differences in the diagnoses of sickness absences explain the occupational class differences in the association between the length of sickness

absence and disability retirement. In this research question, diagnosis is used as a potential confounder. We have now clarified the dual role of diagnosis in our research questions (Introduction, Paragraph 4).

4. Please provide a stronger rationale for what this study adds to the existing literature. It is widely known that long term sickness absence predicts disability retirement. What does this paper add to our knowledge?

Please see our reply above (reviewer 2, point 1). We have now added a stronger rationale for our study (Introduction, paragraph 3–4).

5. Please provide a rationale for men/women differences in the introduction and make this more clear in the objective of the study.

Thank you for these remarks. In our understanding, it is important to perform the analyses separately for men and women, since numerous studies have found gender differences in both sickness absence and disability retirement (see, for example, Gjesdal et al., 2011; Streud 2014; Sumanen et al., 2015). Women tend to have more often sickness absence days than men but men have longer sickness absence spells; however, gender differences are smaller in disability retirements. Furthermore, occupational class distributions are known to be very different among men and women. We have now added a stronger emphasis on the need for gender stratification in the manuscript (Methods, Other covariates).

6. Please provide more information about how a person outside employment can receive a disability retirement? Sickness absence should have taken place from a situation in which a person was employed, given your definition 'income losses caused by work incapacity'?

Thank you for these remarks. In Finland, all those aged 16–64 years with reduced working capacity can be granted a disability pension if they meet certain criteria of long-term decrease in work ability (see <http://www.kela.fi/web/en/disability-pension-and-rehabilitation-subsidy-eligibility>). This includes the unemployed if their working capacity is deemed to be sufficiently reduced. Similarly, all Finns aged 16–67 years may be entitled to sickness allowance if their working capacity is reduced short term and they meet the general medical criteria. Thus, sickness allowance may be granted also to the unemployed, students, pensioners, those on job alternation leave etc. (see <http://www.kela.fi/web/en/sickness-allowance-who-can-claim>).

We think our formulation that sickness allowance is paid to compensate for income losses may have been misleading in this respect. Thus, we have somewhat rephrased this and clarified the description of the measurement of sickness absence in our manuscript (Methods, Measurement of sickness absence, paragraph 1).

7. Why did you choose to include sickness absence starting in 2005 and disability pensions from 2007? What happens to persons who start their sickness absence in January 2005, and were entitled for disability retirement from January 2006? Where they excluded?

We had two main reasons for leaving a 'gap year' between the measurements of sickness absence and disability retirement. First, the majority of disability retirees are granted the pension after one year of sickness absence. In our analyses, we followed all sickness absence spells that started in 2005 to their end, and a large part of these spells ended in 2006. Including year 2006 in our follow-up would therefore have been problematic. Thus, those who transferred to disability retirement in 2006 were excluded from the analyses. This is in accordance with the definition of the study population: working-age persons who were not on any pensions at the end of 2006. Lastly, the use of this kind of 'gap

year' is conventional and commonly used in this field of study (see Alexanderson et al. 2012; Haukenes et al. 2011; Lund et al. 2008). We have now clarified the exclusion of disability pensioners in 2006 in the manuscript (Methods, Measurement of occupational class).

8. Please provide a clear definition of the population of workers. Employee (working for an employer) is different from worker (including self-employment); from Figure 1 it becomes clear that self employed persons are included. Please be careful with terminology to avoid confusion (persons/workers/employees/self employed).

Thank you for these remarks. Occupational social class was classified into manual workers, lower non-manual employees, upper non-manual employees, self-employed (including self-employed and owners of companies with salaried employees), and those outside employment (mainly long-term unemployed, students, unknown and missing). This classification is derived from the Statistics Finland (see Statistics Finland, 1989 reference number 26 in the manuscript), as referred to in the manuscript. We have now clarified the description of self-employed in the manuscript (Methods, Measurement of occupational class) and checked the manuscript throughout in order to ensure clarity of expression concerning the occupational classes.

9. Were 824,915 persons dropped from the sample due to missing information? That is about half of the sample! Please provide more information about exclusion of persons vs. missing information, e.g., in a flow chart.

Thank you for bringing this to our attention. This number was accidentally left in the text from earlier phases of data construction, and depicts the number of persons not at all belonging to our study cohort. We have now corrected this in the manuscript (Methods, Other covariates).

10. What is the rationale for the categories of length of sickness absence?

We aimed to group sickness absence days into meaningful categories and thus used categories largely corresponding to full months. The more detailed rationale for the length categories partly arose from the data. Most people experiencing sickness absence have reasonably short sickness absence spells and only few have long spells (see Table 1). In order to reach groups large enough for the analyses, we defined categories of shorter spells with narrower limits and categories of longer spells with wider limits. The number of participants would have been very small in the long spells groups if those categories would have been narrower.

11. Please provide more information about the categories of occupational classes (upper, lower, manual, non manual), or give at least a reference, as this can not be reproduced with the present information.

Please see our answer to reviewer # 2's point 8 above.

12. Results: please avoid reference to differences between diagnostic groups, as the confidence intervals largely overlap. This implies that the difference is not statistically significant.

We agree that the results concerning differences between diagnostic groups are only indicative. In the analyses performed separately for different diagnostic groups, and supported by the results shown in appendix tables 1 and 2, there was an indication that the length of sickness absence due to mental and behavioural disorders predicted disability retirement more strongly than the length of sickness absence due to other diagnostic groups. This was the rationale why we tested the confounding effect of the diagnostic groups in Figure 3.

We have now modified the interpretation of the results concerning the diagnoses of sickness absence (See "what this study adds?" -box, Results (paragraph 5) and Discussion, paragraph 1 and 3).

13. Given the large sample size, 99% confidence intervals would be more appropriate.

Thank you for this suggestion. However, due to general convention and comparability with related studies, we think that 95% confidence intervals are better suited for our analyses. 95% confidence intervals have been used also in previous large-scale register-based studies (for example, Wang et al., 2014; Jansson & Alexanderson, 2013; Lund et al., 2008; Kivimäki et al., 2007; Gjesdal & Bratberg, 2003.)

(Wang, M., Alexanderson, K., Runeson, B.m Head, J., Melchior, M., Perski, A & Mittendorfer-Rutz, E. Are all-cause and diagnosis-specific sickness absence, and sick-leave duration risk indicators for suicidal behaviour? A nationwide register-based cohort study of 4.9 million inhabitants of Sweden. Occupational and Environmental Medicine. 2014 Jan;71(1):12–20.)

14. I do not see the added value of adjusting for diagnosis, given the fact that stratified analyses were chosen. Figure 3 is redundant.

Please see above (reviewer # 2, point 3) our explanation for the research questions. In research question 2, diagnosis was used as an effect modifier and in research question 3 as a potential confounder. Figure 3 answers to the research question 3. Therefore, in light of our aims, we do not think that the figure is redundant.

15. Discussion: the diagnosis categories are very broad, and perceived limitations in work are not known. This should be discussed as a limitation to the study.

Thank you for this suggestion. We agree that the diagnostic groups are broad, and also, as reviewer # 1 noted, that the workplace characteristics are not controlled for. We have now added a note on these limitations to the manuscript (Discussion, Methodological considerations).

16. P12/line 53: I do not consider missing data of 824,915 participants (as written in the methods section) as 'very little missing information'. Please explain.

Please see our answer to the reviewer's point 10 above.

17. Appendix 2: should 2.9 be 2.90? and 1.50 instead of 1.5?

Thank you for bringing this to our attention. We have now corrected the numbers in the appendix table 2.

VERSION 2 – REVIEW

REVIEWER	Cecile Boot VU University medical center Amsterdam, the Netherlands
REVIEW RETURNED	09-Feb-2018
GENERAL COMMENTS	I am happy with the revisions made.
REVIEWER	Thomas Clausen National Research Centre for the Working Environment, Denmark
REVIEW RETURNED	09-Feb-2018

GENERAL COMMENTS	<p>To the authors, Thank you for this revised manuscript. I am happy with the revisions that the authors have made to the first draft, but after re-reading the manuscript one new concern has arisen that I feel that the authors could address.</p> <p>Results are presented both in figures and in appendix tables. The authors focus much on the results pertaining to upper non-manual workers, and while the results of this group are interesting results on other job groups also merit comments in results section.</p> <p>Also, the authors should clarify the analytical strategy for the analyses of interaction reported in the appendix. It seems that upper non manual workers were treated as reference, but do the results of the interaction analysis apply for all lengths of sickness absence or do the only apply for the +180 day-period?</p> <p>That was it. I hope you find these comments useful and wish you the best of luck with this important research.</p>
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VERSION 2 – AUTHOR RESPONSE

Reviewer’s comments to author:

Results are presented both in figures and in appendix tables. The authors focus much on the results pertaining to upper non-manual workers, and while the results of this group are interesting results on other job groups also merit comments in results section.

Answer: Thank you for this suggestion. We have now added more results on other occupational classes in addition to upper non-manual employees in the results section (see Results, paragraph 5).

Also, the authors should clarify the analytical strategy for the analyses of interaction reported in the appendix. It seems that upper non manual workers were treated as reference, but do the results of the interaction analysis apply for all lengths of sickness absence or do the only apply for the +180 day-period?

Answer: Thank you for this comment. The interaction analysis has been made by including the length of sickness absence in the interaction model, thus the analysis does not apply only for the over 180 days long sickness allowance periods but takes into account all five categories of the variable depicting the length of sickness absence. In addition to existing description of the interaction test found in the statistical methods section and in the third paragraph of the results section, we have now added further clarification of the description of the interaction test (see Statistical methods).

VERSION 3 – REVIEW

REVIEWER	Thomas Clausen NAational Research Centre for the Working Environment
REVIEW RETURNED	19-Mar-2018
GENERAL COMMENTS	I am happy with the revisions made by the authors and recommend that the paper should be accepted for publication.

