Author's response to reviews

Title: Reading and writing difficulties in adolescence and risk of health related work exclusion: a cohort study

Authors:

Kristine Pape (kristine.pape@ntnu.no)
Johan H Bjørngaard (johan.h.bjorngaard@ntnu.no)
Steinar Westin (steinar.westin@ntnu.no)
Turid L Holmen (turid.lingaas.holmen@ntnu.no)
Steinar Krokstad (steinar.krokstad@ntnu.no)

Version: 2 Date: 30 May 2011

Author's response to reviews:

Trondheim, May 30th. 2011

Dear editor, dear referees;

Thank you for giving us the opportunity to resubmit a second revised version of the manuscript. We highly appreciated the thorough review from the two reviewers.

The most important change in our manuscript is the re-writing of the background chapter. We did this in order to meet some of the main objections of the referees, mainly regarding issue no. 1 from referee 1 (theoretical background) and no. 3 from referee 2 (specification of the model). In general, we have also introduced a more consistent nomenclature/language and a more structured discussion. The manuscript has already undergone language editing, although at an earlier stage. If a new editing is requested we will perform it immediately.

Referee Sigrun Olafsdottir:

Major Compulsory Revisions

1. The authors do not provide a strong theoretical background, which I understand is appropriate for a public health journal. However, they could make a stronger case for why it is important to focus on health related work exclusion, and especially what are the mechanisms that link reading and writing disabilities to work exclusion. It is also somewhat unclear what is of ultimate interest, and again, those are all related, but the paper focuses on medical conditions, yet the dependent variable is health related benefits. It would be beneficial to establish a stronger link between health outcomes, work exclusion and health benefits, particularly having a more systematic language of what is ultimately of interest, and then why it is of interest. The background could be more focused on what is the key point of the study, what is the link between reading and writing disabilities
and health, what are the things that the authors will test, and why we should care about this topic.

Comment / changes: Background section is re-written, trying to elaborate on these issues and clarify our aims.

2. The discussion is somewhat unsystematic and would benefit from some more integration of ideas

Comment / changes: The discussion has been divided into sections to make it more systematic, and some parts are re-written or structured.

I am also sceptical of whether the findings from one region in Norway can really be generalized to most European countries, especially if they do not apply to young people living in large cities in Norway. The authors should also elaborate on why the homogeneity of their sample is a strength of the study.

Comment: This is of course an important objection, which is often discussed when using smaller population samples from countries like Norway and also more generally in social epidemiology. Data from the HUNT study cover about 80% of a total county population and are in general representative for the Norwegian population. This is shown for both trends in sick leave and disability pensioning and trends in cause specific mortality.

Changes: We have tried to treat these issues in a more balanced way in the discussion – in the last part of the “strengths and limitations”. (see for example page 14).

Minor Essential Revisions

1. It is somewhat unclear what the authors are interested in. The title mentions health related work exclusion, yet the abstract focuses on health related social insurance benefits. Those are obviously linked, yet it would be clearer to use the same language, or at least make the link. We are basically interested in health related social insurance benefits, as they indicate health related work exclusion.

Changes: We have tried to better explain the objectives of the study and to use a more consistent language throughout the manuscript. The title of the paper is also changed in order to better mirror these objectives.

2. The discussion on who was excluded from the sample (pg. 5) is somewhat unclear (starting with, A total of 101 out of 163 individuals). This would benefit from re-writing.

Changes: This sentence has been re-written (see page 5 in the methods section)

It also seems like the authors are referring to multivariate analysis, rather than multivariable analysis (first paragraph, page 7).

Comment / Changes: We have used the term multivariable analysis when referring to the analyses with one end-point and several independent measures
The authors also measure family socioeconomic status with education, which is too limited of a variable to capture socioeconomic status.

Changes: We agree and have therefore changed “socioeconomic status” to “education” in the text.

Referee Torbjørn Torsheim:

Major Compulsory Revisions

1. The main weakness of the study is the measurement and conceptualisation of reading and writing difficulties. While the current measurement likely captures some true variance, there might be substantial measurement error. The clinical validity is questionable, and misclassification is probable. While it is easy to understand that clinical classification is non-feasable in a large epidemiological study, clearly some assessment of measurement accuracy is needed. On p.12 the authors discuss the measurement, asserting that the current measure obtained a base rate that is similar to other studies. Obtaining a plausible base rate is valuable, but clearly insufficient to demonstrate the quality of the indicators on the level of individual respondents. The authors should also pay attention to the problems of combining reading difficulties and writing difficulties together, as these might occur in isolation.

Comment: This is an important limitation in our study, and finding an appropriate measure of reading and writing difficulties has proposed a major challenge. Even if the original questionnaire included several questions on reading and writing, the questions were not validated and could be exposed to measurement error—especially for those with weak reading skills. These limitations in our data were decisive for both the conceptualisation and choice of measure in our study. When analysing the data, we performed sensitivity analyses, using other measures of RWD, including strictly self-reported problems and separate measures for reading and writing problems. The RWD group composition differed according to the choice of measure, but the association between RWD and welfare dependence did not. The measure used in the analyses was chosen as we considered this the most simple and objective.

Changes: We have tried to be more precise about these topics in the manuscript in all sections – and treated these issues in particular in the middle part of the “strengths and limitations” section in the discussion chapter.

2. The fact that reading and writing difficulties were assessed in students belonging to different age cohorts warrants more attention, as strategies for dealing with RW might differ substantially across school grades and thus threaten the comparability of RWD across age groups. This is underscored by the observation that 70% of the RWD group was middle school attendees, whereas only 50% of the non-RWD group was middle school attendees. Controlling for age might help to remove selection effects, but does not remove
lack of comparability.

Comment: All the analyses were originally performed with an additional school level (middle school or secondary school) and RWD interaction. Reporting results for each school level separately was considered appropriate, but was omitted as it was no support for statistical interaction and the analyses lost strength when dividing the cohort according to both sex and school level.

Changes: We have included a comment in the methods and results chapter, reporting on RWD and school level interaction (page 10 and 11).

3. Potentially mediating factors should receive a more explicit treatment throughout the text. In the introduction section, as well as in the methods section, the authors do not differentiate between confounders and mediating factors, while the causal status of these two classes of variables clearly differs. The discussion section includes a treatment of mediating factors, and rival explanations for the association, including working ability, mental health and completed education. The analysis plan seems to focus on isolating the effects of mental health, since mental health is added in a separate block. However, the rationale for testing mediation procedure was not clearly stated.

Comment: Our aim was to assess the relationship between RWD and welfare dependence. In order to do so, we made a conceptual model, taking into consideration potential confounders. Existing literature on the relation between dyslexia/reading skills and mental disorders are divergent when concluding on the direction of these associations, and in our model mental health problems both represented a potential confounder and effect mediator. This was the reason why we chose to include mental health issues in a separate model. Exploring mediating factors was not our aim, and our modeling did not include such analyses. In the discussion chapter we discuss why adolescents in the RWD group seem more vulnerable to welfare dependence, focusing on education and mental health issues.

Changes: Model specification is re-written in the background (third paragraph), and in the method section (under analysis). The causal pathways are discussed in a separate section in the discussion, as issues needing further attention and analytical approaches.

4. The choice of analysis needs further explanation. In figure 1 an age by RWD effect is depicted, with increasing differences across age. The figure implies a longitudinal trend, yet the modelling approach did seem to include a longitudinal dependent measures. Rather, a cross-sectional logistic model on any (?) long term sickness leave during ages 24 to 28 was specified. The author should provide arguments to support their chosen analytical approach, and why the window of ages 24 to 28 was selected. Further, it is unclear why siblings were analysed separately.

Comment: When performing the analyses we used several dependent measures, including longitudinal assessments, as sensitivity analyses (GEE models using
each year (age) during follow-up). We wanted to make results easy to interpret for the reader, and a simpler logistic end-point was chosen in order to present the results as predicted risks. The window of ages 24 to 28 was used as it provided enough cases of benefit receipt and at the same time assessed the more long-term effects.

Changes: We have reported results from the GEE analyses in the results chapter, also treating the potential effect measure modification with time (see page 11).

5. The concluding arguments should be more balanced. Based on relatively weak associations, the importance of targeting reading and writing difficulties seems overstated, especially since the study does not identify plausible pathways that mediate the impact of RWD.

Comment: We believe that the findings of our study are important, as these issues have not been previously treated using a modern epidemiological approach and in a family context. Despite this, assumptions on such associations are often assumed in the public sphere. The next step will be to study the pathways that mediate the impact of RWD, and for this other types of studies using other analytical approaches are needed. We do however believe that the associations found in our study are more than strong enough to advocate attention to reading and writing difficulties and other factors that make young people vulnerable to work exclusion.

Regards,

Kristine Pape, MD
Norwegian University of Science and Technology
Department of Public Health and General Practice
Postboks 8905, MTFS
7491 Trondheim
Norway
Telephone: private +47 95 11 79 20, work +47 73 59 88 76, Fax: +47 73 59 75 77.
E-mail: kristine.pape@ntnu.no