

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	The welfare burden of adolescent anxiety and depression: A prospective study of 7500 young Norwegians and their families - the HUNT study
AUTHORS	Pape, Kristine ; Bjorngaard, Johan; Holmen, Turid; Krokstad, Steinar

VERSION 1 - REVIEW

REVIEWER	Dr Max Henderson Senior Lecturer Kings College London Institute of Psychiatry UK I declare i have no competing interests.
REVIEW RETURNED	12-Sep-2012

THE STUDY	I think the article would benefit from some English language fine tuning. The most obvious example is the phrase "benefit reception" rather than "benefit receipt".
GENERAL COMMENTS	<p>This is an interesting addition to the small but important literature that sets occupational function within the context of the lifecourse. The authors have made use of the well known HUNT study and linked this to Norwegian registry data. The novel element is the exposure in childhood with follow up in adulthood, the recognition of the role of shared environment and the potential for an association with parental mental ill health. These elements are I believe well constructed and clearly described.</p> <p>There are some elements which I think can be improved upon. The language issue is noted above and is i think important. Secondly in the abstract and in the results the authors describe the impact of a 1 point change in SCL-5 score, but in the table these scores are just dichotomised to High/Low. If as I believe, the authors think there is interest in the effect of single point change then the results should include more detail on SCL scores. It would also be helpful to know at what age individuals got their benefits. Table 2 is a novel and helpful way of looking at the relationship between child and adult morbidity. it would be helpful to have an N for each group - i suspect the size of the groups drops as one goes down the table which will of course have a bearing on the width of the 95% confidence intervals. i think the discussion would benefit from some additional consideration of pathways and influences. the authors have considered the role of parental mental ill health but not what the occupational impact of this ill health was. One might anticipate that a greater influence would be observed from a parent who was sat depressed AND off work than one who was depressed and stayed in their job. Conversely the authors have considered the role of</p>

	parental education but not the possibility that educational attainment might be a mediator of the relationship between childhood symptoms and adult outcomes. These should be considered in the discussion at least.
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REVIEWER	Lauren Franz MBChB, MPH Child Psychiatry and Global Health Fellow Duke University Medical Center Durham, North Carolina, USA I have no competing interests
REVIEW RETURNED	24-Sep-2012

GENERAL COMMENTS	<p>One of the most important conclusions to draw from this study, and one that warrants greater emphasis in the discussion section of the paper, is the importance of a family oriented approach to mental health assessment and treatment across the lifespan. Although this approach is common in the early childhood period, where there is a greater acknowledgement and appreciation of the importance of the family unit in both the assessment and treatment of emotional and behavioral disorders, in adolescence, there is a greater emphasis on the individual in both symptom report and treatment approach. Data from this paper can reiterate the importance of the family unit in a comprehensive mental health assessment and treatment approach throughout childhood and adolescence. The argument could also be made for adult mental health providers to expand their assessment and treatment approach beyond their individual patient and consider screening children in the family unit for anxiety and depressive disorders.</p> <p>I would argue that the statement made by the authors 'we believe that we have demonstrated a universal vulnerability in adolescents regardless of context' is perhaps a bit strong, and needs some reworking. Although it is thought that anxiety and depression are leading causes of global disability and disease burden, there are vast areas of the world where we simply have no data on the epidemiology of anxiety and depressive disorders in childhood and adolescence. The social, economic, and cultural context in which diseases occur undoubtedly effects their presentation and prevalence, and we have to be mindful of this when we make statements about universal vulnerability.</p> <p>Although the following sentence was included in the discussion section 'other mental health diseases and more general personal traits such as childhood temperament and intellectual abilities are individual factors that may be of importance, though these were not assessed in our study': I feel that this point warrants greater discussion as it is potentially a very important limitation of the study.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer Dr Max Henderson

1. I think the article would benefit from some English language fine tuning. The most obvious example is the phrase "benefit reception" rather than "benefit receipt".

Response:

We have had the manuscript proof read and changed the indicated term.

2. Secondly in the abstract and in the results the authors describe the impact of a 1 point change in SCL-5 score, but in the table these scores are just dichotomised to High/Low. If as I believe, the authors think there is interest in the effect of single point change then the results should include more detail on SCL scores.

Response:

We have included some more information on the SCL-5 score in the abstract (under Methods, page 3) and methods section (Methods/Statistical methods – page 11). We also changed Table 1 and 2 in order to include more information on the SCL-5 score and to improve consistency. The text (Results/Adolescent symptoms of anxiety and depression – page 14) was slightly modified so as not to duplicate information from Table 2.

3. It would also be helpful to know at what age individuals got their benefits.

Response:

We included a figure (Figure 2) which shows the proportion of the cohort receiving benefits according to age in the follow-up period.

4. Table 2 is a novel and helpful way of looking at the relationship between child and adult morbidity. it would be helpful to have an N for each group - i suspect the size of the groups drops as one goes down the table which will of course have a bearing on the width of the 95% confidence intervals.

Response:

N of symptom load groups (for complete-case) is included in the new Table 1. Table 2 is based on imputed data.

5. I think the discussion would benefit from some additional consideration of pathways and influences. the authors have considered the role of parental mental ill health but not what the occupational impact of this ill health was. One might anticipate that a greater influence would be observed from a parent who was sat depressed AND off work than one who was depressed and stayed in their job.

Conversely the authors have considered the role of parental education but not the possibility that educational attainment might be a mediator of the relationship between childhood symptoms and adult outcomes. These should be considered in the discussion at least.

Response:

We agree that these are important factors and have added some considerations in the discussions chapter regarding both the importance of adolescent's own educational attainment as an intermediate factor (Discussion/Interpretation of findings – lines 3-7, page 20) and the role of parental work status (Discussion/Interpretation of findings, second paragraph, page 21-22). In the sibling comparison this factor is, to a certain degree, controlled for.

Reviewer Lauren Franz

1. One of the most important conclusions to draw from this study, and one that warrants greater emphasis in the discussion section of the paper, is the importance of a family oriented approach to mental health assessment and treatment across the lifespan.....

Response:

Our study does indeed suggest the importance of a family oriented approach, and in accordance with the reviewer's comment, we have tried to put some more emphasis on this in the manuscript. (Discussion/Implications and conclusions– lines 3-10).

2. I would argue that the statement made by the authors 'we believe that we have demonstrated a universal vulnerability in adolescents regardless of context' is perhaps a bit strong, and needs some reworking.....

Response:

We agree with the reviewer and have moderated the statement of external validity and "universal vulnerability" (Discussion/Strengths and limitations – last sentence, page 19).

3. Although the following sentence was included in the discussion section 'other mental health diseases and more general personal traits such as childhood temperament and intellectual abilities are individual factors that may be of importance, though these were not assessed in our study': I feel that this point warrants greater discussion as it is potentially a very important limitation of the study.

Response:

We agree that the lack of comprehensive information on other mental disorders is a limitation of the study. We have therefore included a comment on this (Discussion/Strengths and limitations – lines 8-11, page 18). In addition, we have given more attention to the potential confounding effects of other mental disorders in the discussion, and made a distinction between this and "more general personal traits" (Discussion/Interpretation of findings – pages 20 and 21).