PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Economic volatility in childhood and subsequent adolescent mental health problems: A longitudinal population-based study of adolescents
AUTHORS	Bøe, Tormod; Skogen, Jens; Sivertsen, Børge; Hysing, Mari; Petrie, Keith; Dearing, Eric; Zachrisson, Henrik

VERSION 1 - REVIEW

REVIEWER	Jenny M. Cundiff, PhD. Texas Tech University United States
REVIEW RETURNED	19-Apr-2017

GENERAL COMMENTS	This is an interesting and well-written paper. Just a few suggestions for edits and points of discussion: 1. The introduction is very clear and well-written. There are a few copy-editing errors - mostly verb tense. 2. The participant section should include information about how representative the final sample was. There is brief mention of this in the abstract, and it seems worth reiterating that only half of the potential population chose to participate. 3. Why define “at-risk-of-poverty” as you have? Similarly, given the data you have available, is it possible to simply use poverty or income in order to define the groups? 4. There are of course serious limitations of self-report of mental health problems, and these should be acknowledged. 5. For the symptoms of general mental health scale: you report on the reliability; is there any evidence for the validity of these self-report scales? 6. Are symptoms of hyperactivity and inattention measured twice (two difference scales)? If so, why? 7. The lines in figure 1 could be more clearly distinguished from one another. 8. Given conclusions, the title seems misleading.

REVIEWER	Dermot O'Reilly Queen's University Belfast Northern Ireland
REVIEW RETURNED	07-May-2017

GENERAL COMMENTS	Thank you for asking me to review this interesting paper. I thoroughly enjoyed reading it and would recommend publication pending a few minor revisions.

	My small number of substantive comments are as follows… Background: • This is an excellent introduction which was a pleasure to read. • I think a little more exposition of theory might be helpful where the authors spell out what they anticipate finding based on theory. • I would like to understand why the authors thought that poverty would influence the prevalence of ADHD. Methods: • There is no adjustment for changes in marital status which is one of the recognised ways of changing economic circumstances…though this limitation is picked up in the discussion section. • I may have missed it but are all children in Norway expected to stay in school until 19 or is this a selected group with a disproportionate number of those in poverty leaving formal education at an early stage? • Was there an adjustment for overall level of income? I wonder if transition into and out of poverty was a proxy for generally low income? The 'starting position' may be also important as one could imagine that the transition from relative affluence to poverty might be more devastating than a similar change from someone who started in relative poverty. • It would have been interesting to include a counterfactual in the analysis (such as the absence of an effect on a common physical condition) as the 'specificity' of the effect would have given greater assurance that this was a real phenomenon. • Finally the focus is based on ages 16-19 but the definition of early and late poverty is based on calendar years. I wonder if this might have introduced an element of misclassification.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Jenny M. Cundiff, PhD.
Texas Tech University, United States
Please state any competing interests or state 'None declared': None declared.

Please leave your comments for the authors below
This is an interesting and well-written paper. Just a few suggestions for edits and points of discussion:

1. The introduction is very clear and well-written. There are a few copy-editing errors - mostly verb tense.

Thank you. We have read through the manuscript and corrected the errors we were able to identify.

2. The participant section should include information about how representative the final sample was. There is brief mention of this in the abstract, and it seems worth reiterating that only half of the potential population chose to participate.

The reviewer and editor note an important issue. In fact, the sample was not representative given the high attrition. As such, we have added more detail to the limitations section on this issue (page 19):

Attrition from the study could affect generalizability, with a response rate of about 53% and with

adolescents in schools overrepresented. Unfortunately, non-participation in survey research is on the rise [66], and non-response is found related to better socioeconomic status [67], i.e., official data shows that in 2012, 92% of all adolescents in Norway aged 16-19 attend high school [68], compared to 98% in the current study. Previous research from the former waves of the Bergen Child Study (a longitudinal study nested within youth@hordaland), has also identified psychological problems as a predictor for non-participation [69]. As the current sample may be skewed towards better socioeconomic status and psychological health, the results may be a conservative estimate of the number of adolescents growing up in poor families and their associated mental health problems.

3. Why define “at-risk-of-poverty” as you have? Similarly, given the data you have available, is it possible to simply use poverty or income in order to define the groups?

We agree that the term “at-risk-of poverty” may not have been optimal. It is conventional to choose a threshold of between 60% of the median income in the population as an indicator of relative poverty. The 60% threshold such as we have used, for example, is used in the UK, across the EU, in the OECD and by UNICEF to measure poverty. As suggested by the reviewer, we have changed this term to “relative poverty,” throughout the manuscript and in Methods (page 9):

From this measure of family income, we calculated the proportion of adolescents in relative poverty, defined as having an equivalised household income below 60% of the equivalised national median income for that particular year (e.g. to calculate relative poverty proportions for 2004 we used the median income for 2004) [40,41].

4. There are of course serious limitations of self-report of mental health problems, and these should be acknowledged.

We agree with this comment. Adolescents may give accurate and reliable reports of their mental health problems when assessed with methods that are appropriately targeted to their age [1]. However, we still agree that sole reliance on self-reported data is a limitation, and we now acknowledge this in limitations: (p 19):

Information about mental health outcomes is self-reported. Although adolescents may provide accurate information about their own mental health [65], there would have been stronger support for the results had measures such as clinical evaluations been available.

5. For the symptoms of general mental health scale: you report on the reliability; is there any evidence for the validity of these self-report scales?

We have now added information about the validity of the self-report scales in the Methods section (p. 9-11). Several references were added, demonstrating the validity of the SDQ [2], SMFQ [3], and ASRS [4].

6. Are symptoms of hyperactivity and inattention measured twice (two difference scales)? If so, why?

Symptoms of hyperactivity and inattention are measured in the Strengths and Difficulties Questionnaire and using the ASRS. The measure in the SDQ, is very brief, only containing two items on hyperactivity/impulsivity and three items on inattention. The age-group in the current sample is also towards the upper end of the age-norms for the SDQ (although it has been found to work fairly well also in this age group; [5]). We therefore wanted to include an additional more detailed instrument – the ASRS – which has been specifically developed to measure ADHD and has been used in this age group. We have now added this information on p. 11:

This additional measure of ADHD was included as symptoms of ADHD are only briefly measured by the SDQ, and the age-range in the current sample is at the upper end of the norms of the SDQ.

7. The lines in figure 1 could be more clearly distinguished from one another.

The lines in figure 1 has now been revised for clarity.

8. Given conclusions, the title seems misleading.

The title has been revised, (also to comply with changes asked for by the Editor), and now reads: Economic volatility in childhood and subsequent adolescent mental health problems. A longitudinal population-based study of adolescents

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Reviewer: 2
Dermot O'Reilly
Queen's University Belfast, Northern Ireland
Please state any competing interests or state 'None declared': None declared

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Please leave your comments for the authors below

Thank you for asking me to review this interesting paper. I thoroughly enjoyed reading it and would recommend publication pending a few minor revisions.

My small number of substantive comments are as follows…

Background:
• This is an excellent introduction which was a pleasure to read.

Thank you for this comment.

• I think a little more exposition of theory might be helpful where the authors spell out what they anticipate finding based on theory.

We have now expanded the theoretical foundations for our anticipated findings on page 6:

Different mental health problems debut at different ages [13], and investigating the co-occurrence of risk factors such as poverty with potentially vulnerable periods may provide more insight into mediating mechanisms. Poverty in early (pre-school) childhood has been emphasized as especially detrimental to development [14,15], as it is a crucial time for brain development and may disrupt the many core cognitive and social competencies are acquired at this time. In contrast, others have highlighted middle childhood [16] and early adolescence as particularly sensitive periods [11]. Middle childhood is, for most, when formal schooling starts. This period also provides the foundation for further developments in adolescence and adulthood [17-19]. Support has been found for the importance of early childhood [15,20], and recent studies also underscore the importance of experiencing poverty in middle childhood and in early adolescence, especially in relation to development of internalizing and externalizing mental health problems [21,22]. Besides the demonstration of timing effects, these studies also demonstrated the detrimental effect of long-term, or repeated exposure to poverty [21,22].

• I would like to understand why the authors thought that poverty would influence the prevalence of ADHD.

Several studies indicate that most mental health problems, and ADHD is no exception, are correlated with socioeconomic conditions. There are several papers (some of which we reference on page 17) showing that ADHD is more common among poor segments of the population (also some using genetically informed designs, e.g., [6]). Our expectations were based on readings of the previous literature, and our findings were therefore somewhat unexpected in light of those studies, which we emphasize on p. 17.

A curious finding was seen for symptoms of hyperactivity/inattention, measured with the SDQ. Here the never poor group had lower scores relative to those moving into or out of poverty, but not relative to the chronically poor group. Participants in the chronically poor group also had lower scores relative to those in the transient poverty groups. A similar trend was also observed when symptoms were measured with the ASRS. Previous studies do suggest that ADHD is associated with a range of socioeconomic indicators, including poor economic well-being and poverty [36,57,58], and as such, this unexpected finding warrants further investigation before firm conclusions can be reached.

Methods:
• There is no adjustment for changes in marital status which is one of the recognised ways of changing economic circumstances…though this limitation is picked up in the discussion section.

Changed marital status is indeed a prominent factor that influences economic circumstances in a household. Unfortunately, longitudinal data on marital status were not available, and this was acknowledged in limitations as pointed out by the reviewer (p. 18-19):

The main aim of this study was to investigate associations between trajectories of low income and mental health, and as such, we did not assess the factors associated with the actual trajectories themselves. There are several reasons why families experience fluctuations in income, such as changes in parental work affiliation or education levels, and structural changes in the family like divorce or reconstitution [7]. Due to lack of historic information about such events, factors causally associated with different trajectories were not explicitly investigated in the current study.

• I may have missed it but are all children in Norway expected to stay in school until 19 or is this a selected group with a disproportionate number of those in poverty leaving formal education at an early stage?

Official data show that in 2012, 92% of all adolescents in Norway aged 16-18 attended high school, compared to 98% in the current study. As such, care should be take when generalizing to all adolescents, as this sample comprised largely high school students. We now acknowledge this in limitations (p 19):

Attrition from the study could affect generalizability, with a response rate of about 53% and with adolescents in schools overrepresented. Unfortunately, non-participation in survey research is on the rise [66], and non-response is found related to better socioeconomic status [67], i.e., official data shows that in 2012, 92% of all adolescents in Norway aged 16-19 attend high school [68], compared to 98% in the current study. Previous research from the former waves of the Bergen Child Study (a longitudinal study nested within youth@hordaland), has also identified psychological problems as a predictor for non-participation [69]. As the current sample may be skewed towards better socioeconomic status and psychological health, the results may be a conservative estimate of the number of adolescents growing up in poor families and their associated mental health problems.

• Was there an adjustment for overall level of income? I wonder if transition into and out of poverty

was a proxy for generally low income? The ‘starting position’ may be also important as one could imagine that the transition from relative affluence to poverty might be more devastating than a similar change from someone who started in relative poverty.

The reviewer brings up several interesting points about how the initial level of income could have implications for the impact of changing economic circumstances. The methods used in the current paper does not answer these, and this has now been acknowledged in the limitations section (p. 20):

Still, the method does not adjust for overall level of income, and it does not give an indication of the magnitude of the economic transition (e.g., moving from affluence to poverty may have greater consequences that moving from relative poverty to poverty).

• It would have been interesting to include a counterfactual in the analysis (such as the absence of an effect on a common physical condition) as the ‘specificity’ of the effect would have given greater assurance that this was a real phenomenon.

The inclusion of a counterfactual is an interesting idea. As the current survey focused on mental health, we did not obtain information about physical conditions that were suitably common. On a side-note, we would also expect that most common physical conditions would also be influenced by socioeconomic factors such as economic circumstances [8], so were not certain that using common physical conditions would be an adequate counterfactual.

• Finally, the focus is based on ages 16-19 but the definition of early and late poverty is based on calendar years. I wonder if this might have introduced an element of misclassification.

This discrepancy between age and calendar years was accounted for in the analyses. We agree that this can be somewhat confusing, and we therefore included the age-ranges as well as the calendar year in the description of the x-axis in Figure 1 to illustrate that a single year of income corresponds to different age-groups in the sample.

References

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8 Chen E, Matthews KA, Boyce WT. Socioeconomic differences in children's health: how and why do

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VERSION 2 – REVIEW

REVIEWER	Jenny M. Cundiff, Ph.D. Texas Tech University United States
REVIEW RETURNED	22-Jun-2017

GENERAL COMMENTS	Glad to see the authors resubmitted. On second review, some significant concerns remain: 1. The participant section does not describe attrition from the original study/data collection or compare the analyzed sample to the original population sampled. This should be described in detail as it is important to understand who was not included, why, and what it may mean for results in this sample and this set of analyses specifically. Discussing this in the limitations section without description and context given in the participant section is not adequate. 2. Reasons given in the response for using the “under 60%” threshold should be incorporated into the manuscript (e.g., UNICEF guideline for measuring poverty, etc). This helps give a little more context for your choice as SES variables are measured in so many ways 3. The self-report data is limited not only by the fact that it is self-report but by the brief nature of the measures themselves. This is common in such large studies but these limitations should be described more fully. 4. Why not simply stick with the better measure of ADHD instead of keeping to outcome measures of the same construct? 5. Reviewer 2’s point about adjust for overall level of income is a very good one. Perhaps rather than poverty or moving in and out of poverty, results are due to lower income. This is a potential alternative explanation for the findings that can be tested in the data set and so I recommend it be statistically examined rather than stated as a limitation (since the limitation can be addressed).

VERSION 2 – AUTHOR RESPONSE

The reviewer had some further concerns. Our responses follow below.

1. The participant section does not describe attrition from the original study/data collection or compare the analyzed sample to the original population sampled. This should be described in detail as it is important to understand who was not included, why, and what it may mean for results in this sample and this set of analyses specifically. Discussing this in the limitations section without description and context given in the participant section is not adequate.

We agree that this could be added to the participant section. Rather than comparing with the original study/data or original population (which is not comparable due to the design of the study) we have compared the sample with the population from official statistics on school participation which was available for comparisons. We also added some information from a previous publication comparing the mean GPA for mandatory courses in high school in the sample and in the population as an indication of representativeness of the sample. We have made the following changes to the manuscript:

Added to Procedure and participants (pp. 8-9):
Official data shows that in 2012, 92% of all adolescents in Norway aged 16-19 attended high school [38], compared to 98% in the current sample. The GPA in current sample was comparable to the national GPA, but somewhat lower than the GPA in Hordaland country [39].

Changes to Limitations:
We have replaced the word “attrition” with “non-participation” in the limitations section (p. 19).

2. Reasons given in the response for using the “under 60%” threshold should be incorporated into the manuscript (e.g., UNICEF guideline for measuring poverty, etc). This helps give a little more context for your choice as SES variables are measured in so many ways

We have now added the following to the measurement section (page 9): “This criteria corresponds to the criteria used in income inequality statistics in the European Union [39,40].”

3. The self-report data is limited not only by the fact that it is self-report but by the brief nature of the measures themselves. This is common in such large studies but these limitations should be described more fully.

Information about the number of items in each of the scales (SDQ = 25; SMFQ = 13; ASRS = 18) are now reported in the methods (pp. 10-11). We have also added the following to the limitations section (p. 19): “Information about mental health outcomes is self-reported using relatively brief measures. Although adolescents may provide accurate information about their own mental health [66], there would have been stronger support for the results had more comprehensive measures such as clinical evaluations been available.”

4. Why not simply stick with the better measure of ADHD instead of keeping to outcome measures of the same construct?

The SDQ is an instrument that consists of all the five subscales used in the paper. Rather than reporting on only some of these subscales, we decided to include the SDQ in its entirety. However, we agree that the better measure is to be preferred, and have a change in the manuscript to clarify that we rely more strongly on the findings from the ASRS in the interpretation and discussion of the findings (p. 17): “A curious finding was seen for symptoms of hyperactivity/inattention, measured with the ASRS. Here the ‘never poor’ group had lower scores relative to those moving into or out of poverty, but not relative to the ‘chronically poor’ group. Participants in the ‘chronically poor’ group also had lower scores relative to those in the transient poverty groups. A similar trend was also observed when symptoms were measured with the SDQ. Previous studies do suggest that ADHD is associated with a range of socioeconomic indicators, including poor economic well-being and poverty [32,61,62], and as such, this unexpected finding warrants further investigation before firm conclusions can be reached.”

5. Reviewer 2’s point about adjust for overall level of income is a very good one. Perhaps rather than poverty or moving in and out of poverty, results are due to lower income. This is a potential alternative explanation for the findings that can be tested in the data set and so I recommend it be statistically examined rather than stated as a limitation (since the limitation can be addressed).

We agree that the association between low income and mental health is also of importance, and this association has been studied extensively. Studies on the association between low income and mental health were reviewed in the introduction (i.e., second paragraph starting on page 5). In line with more recent studies nuancing this association (see e.g., page 6 of the introduction) the aim of the current

study was to specifically investigate the transitory patterns of economic circumstances over time, and to consider the effects of duration, timing and sequencing of exposure to relative poverty. Therefore, rather than adjusting for low income, in using the latent class approach the group of participants that had a consistently low income relative to the rest of the sample is captured in the chronically poor group, whereas those who have low incomes early or later in childhood are in the “moving out of”- and “moving into” poverty groups respectively.

We agree that this was not adequately explained in the limitations section, and this has now been revised to: “Finally, latent class analysis is probabilistic in nature, and the uncertainty of classification into the latent classes is expressed in the probability scale of being in either of the poverty trajectories at any given time during the period for which income information was available. As such, the classes may deviate from the results obtained if observed data had been used instead. However, the pattern of results was largely replicated in robustness checks using observed data, and the classes were associated with relevant socioeconomic factors in a meaningful way, increasing our confidence that the latent class approach has captured meaningful patterns in the income data. Still, the method does not give an indication of the magnitude of the economic transition (e.g., moving from affluence to poverty may have greater consequences than moving from relative poverty to poverty).”

VERSION 3 – REVIEW

REVIEWER	Jenny M. Cundiff, Ph.D. Texas Tech University United States of America
REVIEW RETURNED	06-Jul-2017

The reviewer completed the checklist but made no further comments.