

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	The association between childhood cognitive ability and adult long term sickness absence in 3 British birth cohorts: a cohort study
AUTHORS	Max Henderson, Marcus Richards, Stephen Stansfeld and Matthew Hotopf

VERSION 1 - REVIEW

REVIEWER	Tom Sensky Emeritus Professor of Psychological Medicine Imperial College London Consultant Psychiatrist I have no competing interests to declare.
REVIEW RETURNED	07/02/2012

GENERAL COMMENTS	<p>1 The subject of this paper is topical, and the paper offers interesting and original data. Overall, the text is well written and easy to follow. The aims and the hypotheses of the study are clearly stated, and the results and tables are appropriate.</p> <p>2 In the introduction, it would probably be better to replace references to specific UK benefits with more generic descriptive terms, since the specific benefits change over time. For example, incapacity benefit is currently being replaced.</p> <p>3 The authors note (p6) that although the sample from the 1946 cohort was broadly representative of the population from which the cohort was taken, some variables pertinent to the study were overrepresented among non-responders. This implies that the sample assessed was possibly biased. If this is correct, this should be commented on when considering the weaknesses of the study. The other two cohorts are not described in the same detail, but references are cited to more detailed descriptions. If the samples from these cohorts were also possibly selective, then this should also be noted in the discussion. Similarly, does the 59% response rate from the 1970 British Cohort Study represent a potentially selective sample?</p> <p>4 In describing the "risk set", the authors note that analyses were restricted to those who describe themselves as being employed or in full-time education in the sweep immediately before that used in the present study. It would be helpful to know this interval, for each of the cohorts.</p> <p>5 The authors note that the results, shown in Tables 1 and 2, are similar for all three cohorts. However, differences remain, and some discussion of these would be helpful. For example, what the authors</p>
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	<p>consider might account for the failure to replicate the statistically significant findings from the 1958 cohort in the other two cohorts? There is a brief discussion of some differences between the cohorts (p12), but more would be helpful.</p> <p>6 Educational attainment, adult social class, and depression are considered to be mediators rather than confounders of the relationship between cognition and long term sickness absence (p13). However, while this is plausible, the authors offer no explanation or justification.</p>
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REVIEWER	Jenny Head Reader in Medical and Social Statistics Department of Epidemiology and Public Health
REVIEW RETURNED	15/02/2012

GENERAL COMMENTS	<p>This is an interesting paper which addresses a little researched area, namely does childhood cognitive ability predict adult long term sick leave and if so, are the associations mediated through educational attainment, adult social class or adult mental ill health. Data from three large national birth cohorts are used to address these research questions.</p> <p>I have the following suggestions which the authors might like to consider:</p> <ol style="list-style-type: none"> 1. The authors refer to results from an earlier study which showed that emotional and behavioural difficulties in childhood predict long term sickness in adult life (Henderson et al, 2009). Given that childhood illness may be related to both adult ill health and sickness absence behaviour, it would be good to test whether associations between cognitive ability and adult sick leave remain after adjusting for childhood illness. 2. The authors consider the role of depression in adulthood as this is the leading reason for long term sick leave. But given that sick leave is a strong predictor of mortality, it would be interesting to add chronic physical disease as a potential mediator. 3. It would be good to be consistent in terminology throughout the paper eg in places the authors say IQ rather than cognitive ability. 4. Page 9, risk set. More information needs to be provided on the participants excluded from the analysis because they were not in full time work, education or caring for a family in the previous sweep. Specifically, how many years ago was the previous sweep, how many people and what proportion were excluded from each cohort and how many of the excluded were on long term sick leave, and if available, how many had never worked. 4. Page 9 , statistical analysis. Was multiple imputation carried out for all variables including the outcome or was the multiple imputation for the explanatory variables only? This should be stated in the paper. 5. Page 16, Table 2. The confidence interval is missing for one of the results in final column. Also, it would be good to add numbers analysed for the 3 cohorts.
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	Thank you for giving me the opportunity to review this interesting paper.
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VERSION 1 – AUTHOR RESPONSE

Professor Sensky

1. Thank you
2. The term 'health-related benefits' has been used to replace IB.
3. A sentence acknowledging issues regarding non-participation has been included in the discussion section (weaknesses).
4. A clause indicating the year of the previous sweep has been added to the end of the 'risk set' paragraph
5. There are a range of reasons why, despite the notable similarity between the cohorts, some differences may remain. We have noted that power issues may be at least partly responsible for the issues regarding statistical significance; the size of the total 1946 cohort is smaller than the others, and the prevalence of the outcome in the young population represented by the 1970 cohort was very small (~1%). Other factors at play include the different way cognitive ability was measured (mentioned in the methods), the different way the outcome was measured in the 1946 cohort (mentioned in the methods) and chance.
6. We acknowledge that the distinction between confounding and mediating factors is not always easy to make; of course there is no statistical test for confounding. Our main point here is that cognitive function predates the other variables and it is at least plausible that the others are on the causal pathway between cognitive function and IB receipt. This causal pathway is likely to be convoluted! We further acknowledge that it is possible that there is an element of confounding and we had hoped that this is implicit in our measured language ("...predominantly as confounders..." and "...in part explained by"). To further emphasise this we have added a final sentence highlighting the complexity.

Dr Head

1. With the existing data we have used for this study (1) and (2) are beyond the scope of this analysis. We fully accept the possibility of a role for physical health across the life course. We have included a line in the discussion to this end and think this would make an excellent follow on study.
2. As above
3. We agree. Thank you. IQ has been changed to 'cognitive function' throughout.
4. The date of the previous sweep has been included in the risk set paragraph together with the proportion of participants removed by the restriction. The results are
1946: 23% excluded of whom 27% (vs 5.5%) on HRBs
1958: 7% excluded of whom 15% (vs 2.9%) on HRBs
1970: 11% excluded whom 13% (vs 1%) on HRBs.
We do not have data on "never worked".
5. Multiple imputation was carried out for all variables including the outcome. This has been added to the methods section.
6. Thanks for spotting the pasting error. The missing figure has been added and Ns for all cohorts added to Table 1.
We believe these amendments improve the paper and look forward to hearing from you in due course.

Yours faithfully

Dr Max Henderson