Reviewer's report

Title: An investigation of factors identified at birth in relation to anxiety and depression in old age. The Hordaland Health Study

Version: 2 Date: 28 February 2013

Reviewer: Daniela Goncalves

Reviewer's report:

This is an interesting study that might provide further information about a topic that has been seldom considered. I have some concerns, however, regarding its methodology, which I would like the authors to address. It is my understanding that by replying to these comments the authors will be able to clarify some questions that are not so straightforward in the current version of the manuscript, overall improving its readability and conclusions.

Major concerns

• Overall, it is not clear to me how this study was conducted. The authors mention in the methods that the HUSK is a cohort study, but in the abstract it is referred to as a cross-sectional survey sample of community residents. These studies would include different designs. It would also be helpful to be explicit about the original study sample here, including non-participation rates and to what extent the original sample was representative of Bergen’s inhabitants (considering this has been ascertained already).
• The authors use 2 outcome variables and 36 potential explanatory variables; thus, 72 possible associations are being considered, which means that if adopting a conservative position, only one of all the considered results would be considered significant (case-level anxiety associated with mother’s condition after birth, with p=0.001). Could the authors please comment on why they decided not to apply any type of correction to their analysis, thanking into account the considerably high amount of variables analysed?
• Conclusions (p.12): the authors mention that “although there was no clear evidence of mediation by educational attainment (…)”; how was this ascertained by their analysis?
• For this study, the authors had access to birth records data for 14% of the original sample (which, as previously stated, is not even described, e.g., how high was the initial non-response, amongst other relevant aspects); of these, they lost further 15% of the potential sample due to missing data (which, as below stated, it would be relevant to comment). This is a remarkably small sample, which the authors acknowledge but justify by saying that previous studies retrieved data for proportions as low as 2-5%; whereas that is true, the reference they have provided also presents plenty of studies where the authors were able to trace down as much as 60% of the original sample, often averaging 40%. These are substantially different numbers, which will have an impact upon the
generalisability of results, and I believe further thought should be put into it. Does this study really shed further light upon the relationship between early factors and later mental health problems, or was it just considerably underpowered, thus the lack of significant results where other studies have found so?

• Also related with my previous point, the authors state that “in these studies it has been argued that as long as comparisons are made within the identified subgroups, it is unlikely for substantial bias to be introduced in the tested associations”. This has been advocated not by further studies but by a particular study, by Martyn and colleagues, where the authors were able to retrieve data for 66-77% of their original sample, totalling 13,000+ participants. Again, the comparisons between both studies are limited by the sample size, which obviously also constrains assertions about the results and subsequent implications.

Minor concerns

• For the study population (p.5): could the authors please explain what “all residents…of a previously established cohort” means?

• Still related with study description, authors mention that the records from one hospital were retrieved/consulted; is it possible to describe how many hospitals existed in Bergen at the time, and whether any attempts were made to consult records from those?

• Regarding the values chosen for establishing caseness: it is not clear to what extent self-reports developed for other groups swiftly apply to older populations (e.g., http://www.ncbi.nlm.nih.gov/pubmed/18074254), and I would recommend the authors to, at least, acknowledge it. Furthermore, the same paper they cite (Flint & Rifat) seems to suggest a cut-off point of 11 (p.122), which should also be addressed.

• Cohort context (p.7): interesting information, but probably better placed in the introduction?

• Related with cohort context, is it possible for the authors, using the information available re parental characteristics, to place this “traced” cohort within 1925/1927 Bergen’s society?

• Statistical analyses (p.7): 15% of the “traced” participants did not have a valid HADS; this seems to be a considerably high number, would the authors be able to explain a bit more?

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
'I declare that I have no competing interests'