Reviewer’s report

Title: Explanations for female excess morbidity in adolescence: evidence from a school-based cohort in the West of Scotland

Version: 1 Date: 26 March 2007

Reviewer: Torbjorn Torsheim

Reviewer’s report:

General
The question posed by the authors is central and well defined. Several authors have addressed explanations for gender differences in adolescent health, but the study fills a need for accumulation of findings in this area, especially by focusing on a broad set of factors. The research questions are clearly formulated. The sample is sufficiently large and the measurements follow well-established formats. The results indicate gender differences in all ‘mediating factors. In the presence of statistical control for mediating factors there was still strong gender differences in the outcomes. It is concluded that other factors need to be included to account for such differences.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. Introduction. The manuscript would improve if a clearer statement of mechanisms was included. The authors consistently refer to associations, but without stating the mechanism. To me it is not clear why BMI should lead to more headaches in girls. There should be a clearer statement as to why self-esteem might explain gender differences in health. After all, the correlation between self-esteem and depressed mood tend to be very high, which could reflect conceptual confounding between these constructs.

2. Measurement. For some of the scales, notably the Masculinity scale, the operationalization is based on limited information. It is quite understandable that space limitations put constraints on the number of items included, but the authors should address whether this has any impact on the validity of the scale used to measure masculinity and femininity. In general, authors should include more about measurement validity and reliability, since these are important issues for making inferences from the data.

3. BMI comparison across gender is problematic, since any given BMI would reflect different body composition for boys and girls. An alternative would be to use standard age and sex adjusted classification of overweight and obesity.

4. Sampling. The sample is school-based, but it is not clear whether the sample was clustered. A clearer description of the sample is needed to consider whether a design effect might be present. The analysis does not seem to take into account potential design effects. Such design effects might be ignorable, but should be evaluated.

5. Based on interaction analysis, the authors modelled gender differences in subcategories of potentially mediating influences. This resulted in separate analysis of gender differences in dizziness among non-smokers and smokers. While such a procedure could be justified in terms of achieving homogeneity in odds (statistical reasons), it is questionable whether this procedure makes sense conceptually. From apriori knowledge, and the introduction part, I would not expect a different pattern of gender differences among smokers and non-smokers. This procedure seems to be data-driven, with the danger of capitalising on chance. The authors need to justify their approach clearly.

6. Table 3 include a computation of " % female excess explained". This could be misleading, since the logistic OR is a nonlinear transformation of the log odds. An OR drop from 3 to 2.5 and a further reduction from 2.5 to 2 is not on a linear scale.

7. The discussion is clearly written, but highly descriptive. Main results are mentioned and emphasised, but not discussed at any length. A clearer contextualisation of the findings would improve the manuscript. For example, on p.13 the authors note that among females, masculinity was positively associated with depressive mood but among males it was negatively associated. According to the authors this finding runs counter to other studies. Some reflection about how this finding should be interpreted, and in what respect it seems deviates form other studies should be included.
8. Much of the discussion deals with the lack of explanation of gender differences. Clearly gender differences, as other differences in health, are multi determined. If we in addition consider measurement error, there is every reason to expect that one will not be able to explain gender differences completely. The impact of measurement error needs to be clearly stated.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

The term ‘depression’ is used throughout the manuscript, but the measurement instrument deals with depressive mood. Depression is a clinical term while depressive mood describes variation in affective state.

Discretionary Revisions (which the author can choose to ignore)

The authors consistently write ‘Explain’ with a hyphen. Although it is good to express some awareness about the limitations related to explanation, the usage of the term can still be qualified in a simple sentence, rather than hyphenating it on every occasion. (e.g. We use the term "explanation" in a strictly statistical sense of the word.)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

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

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests.