Reviewer’s report

Title: Prediction of posttraumatic stress disorder among adult in flood district

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Reviewer: Anke Ehlers

Reviewer’s report:

This study assessed an impressively large sample of flood survivors in rural China 2 years after the event for PTSD. Degree of exposure, demographic characteristics and descriptors of current mood were also assessed with the goal to develop a brief screener for PTSD. The study addresses an important area of research, and the authors are to be congratulated on obtaining such a large apparently representative sample. The cross-validation of the results is an important strength of the study.

However, I think that the impact of this paper could be much improved if the authors revised their analysis and developed a much simpler screener. I also think that the scope of the literature review and discussion needs to be extended in several ways as specified below.

1. The goal of the study is to develop a "practical screening method with application value". I think that the impact of the paper and the practicality of application could be much improved if the number of variables was reduced. For example, the authors could combine the 8 exposure variables (5 to 12) to one scale of degree of exposure. Similarly, the 4 symptom variables (13 - 16) could presumably be combined into a scale. Alternatively, the authors could select a few of the most predictive items to form the scales. Overall I am wondering whether items that did not increase PTSD risk by much could be dropped to make the scale more economical?

Furthermore, the formula from the logistic regression would be quite difficult to score. I think this would limit the practical use. I wonder whether it would be possible to develop a simpler score such as risk points (that are related to Odds ratios) for presence or absence of certain variables that could be simply added up such as

Female = 1 point; injured = 2 points; severe flood = 4 points

2. I do not think 72% sensitivity is acceptable for a screening instrument. A lower cut-off seems more appropriate for screening purposes.

3. There is no rationale for the selection of the predictors. Previous work finding evidence for the role of the predictors in other populations should be cited. The items for the screener questionnaire need to be described in the method section. Were they selected from a larger group of questions. How were the mood questions chosen? Furthermore, the discussion should link the findings on sex
and age differences, degree of exposure to previous research.

4. The authors will need to justify why the screener did not include self-reported PTSD symptoms, which are good predictors of PTSD diagnosis. There is a range of such screener questionnaires with good predictive validity (see review by Brewin, C.R, Journal of Traumatic Stress, Journal of Traumatic Stress. Vol.18(1), Feb 2005, pp. 53-62; see also the comparison of various symptom combinations Ehring et al., Journal of Nervous and Mental Disease. Vol.195(12), Dec 2007, pp. 1004-1012.).

Prediction on the basis of PTSD symptoms usually yields a better sensitivity and specificity than the instruments suggested here. Thus, the authors will need to argue why it may be more practical/acceptable etc to use the scale they suggest. One possible reason I could imagine is that it the screener could be used to predict who should receive more detailed screening for PTSD. If this is a valid concern in a large population to be surveyed, this would suggest that the authors concentrate on demographics and exposure variables that could be determined objectively.

5. In the discussion, the authors will need to acknowledge that they failed to include psychological several predictors which may have increased the predictive value of the questionnaire (see for example the meta-analyses by Brewin, Chris R et al., Journal of Consulting and Clinical Psychology. Vol.68(5), Oct 2000, pp. 748-766. and Ozer, E. et al., Psychological Bulletin. Vol.129(1), Jan 2003, pp. 52-73.).

McDonnell and colleagues have recently published such a 10-item screener in Journal of Consulting and Clinical Psychology. Vol.76(6), Dec 2008, pp. 923-932. It has a sensitivity of .83 and specificity of .84

6. In the discussion, I think it will be noteworthy to comment on the finding that the concept of PTSD appeared to apply to the rural Chinese population (and possible limitations that the authors observed). There is controversy about whether the concept of PTSD is valid in other cultures, and the data appear relevant. See for example: Mezey, Gillian; Robbins, Ian. BMJ: British Medical Journal. Vol.323(7312), Sep 2001, pp. 561-563.

7. The cross-sectional nature of the study needs to be acknowledged as a limitation. For example, items 13 to 16 could be a consequence of having PTSD and may not work prospectively.

Smaller points:

p. 2: PTSD is the dependent, not the independent variable

p.5/6: The description of the DSM-IV criteria can be much reduced. It would be sufficient to say that the cut-off was 2 for each symptoms and that the DSM-IV algorithm was used. It would be important to report inter-rater reliability.

Table 1: some of the items are incomplete
Table 2: x4 is missing

**Level of interest:** An article of importance in its field

**Quality of written English:** Needs some language corrections before being published

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

**Declaration of competing interests:**

I declare that I have no competing interests