Author’s response to reviews

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

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Author’s response to reviews: see over
Dear Editor,

Thank you very much for your letter of September 23, 2009 inviting us to submit a revised version of the above mentioned manuscript. We have made changes according to the reviewers’ comments. Below are our specific responses to the reviewers’ comments.

Reviewer: Anke Ehlers

Comment 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

Response 1:
Yes, it is a good suggestion. We have combined the X7-X12 into X7, and combined X13-X16 into X8. As a result, the number of variables included in model decreased from 13 to 7. In addition, a simpler risk score model was created for prediction.

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

Response 2:
We agree with the reviewer. If one wants to find as more potential patients as possible, one can select a lower cutoff value. We list different cutoff value and corresponding sensitivity and specificity in Table 3, public health workers can choose the optional cutoff values in their PTSD prediction.

Comment 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.

Response 3:
The risk factors of PTSD considered in this study is based on the professional judgement and literatures. We described the method for selection of risk factor in methods section (page 7 line 15-18) and discussed it in page 11 line 20-25.

Comment 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.

Response 4:
The purpose of this study is to develop a prediction model for the rapid identification of high risk of PTSD after flood, not for screening of early PTSD patients. Therefore, only some demographic characteristic($x_1,x_2,x_3$), type of flood ($x_5$), severity of flood ($x_6$), flood experience($x_7$) and mental status before flood ($x_8$) were included in our predictive model, without the early symptom of PTSD.
Comment 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.

Response 5:
As mentioned above, our model is used to predict and not to screen. So the early symptoms of PTSD have not been included in our model. We have discussed the question from page 10 line 27 to page 11 line 2.

Comment 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.

Response 6:
Indeed, as the reviewer has mentioned, there is controversy about whether the concept of PTSD is valid in different cultural backgrounds. But, the questionnaire for PTSD had been tested in Chinese populations, which was proved valid and reproducible. We noted the question in page 7 line 1-3.

Comment 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.

Response 7:
Yes, I agree with the reviewer’ comment. But we specified the item 13 to 16 as mental status before flood, which must occur before flood and before PTSD. We defined the item in page 8 line 1.

Comment 8:
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

Response 8:
Corrected: PTSD is the dependent.
We have predigested the description of the DSM-IV criteria.
some incomplete items in Table 1 have been added.
Variable $X_4$ didn’t enter prediction model.

**Reviewer:** Cengiz Kiliç  
**Comment 1:**  
Language: The manuscript needs to be thoroughly revised for grammatical errors.  
**Response 1:**  
Thanks for your suggestion. Our manuscript has been thoroughly revised for grammatical errors by a Canadian professor.

**Comment 2:**  
Sample: how do they know the residents are similar in terms of economic and educational levels. How do they define severity of flood? Who defines %50, %75 etc.?  
**Response 2:**  
The catchment area is a small area, with similar natural and social environment. Most of residents are farmers and have low educational levels (showed in Table 1), which were described in page 5 line 5-7. Severity of flood and its standard was set up by the Chinese flood management authority (page 5 line 26-27).

**Comment 3:**  
How many interviewers? How long did assessment take? How long did data collection last? How were the subjects contacted? Where were they interviewed? Any privacy problems? How many refused? What was done to find those who were not at home during the visit? How were the missing data completed, by re-visits or by phone?  
**Response 3:**  
Yes, we have added some information mentioned in your comment 3 in page 6 line 2-13 and page 8 line 25-27.

**Comment 4:**  
On page 5, the authors should make it clear they are describing their questionnaire, not DSM-IV criteria. And what is a ?structured? questionnaire? Is it administered by interviewers? Did responders fill it themselves? Or was it read out to them and responses recorded? It is not necessary to give the 17 symptoms in detail. How was duration of symptom ascertained? Did interviewers ask about it for every positive item?  
**Response 4:**  
Yes, I agree with your suggestion. We have deleted the 17 symptoms of PTSD in method description. Interviewers asked the duration of every positive symptoms, all the positive items meet the criterion E of PTSD (the disturbance lasting more than 1 month) (see from page 6 line 27 to page 7 line 1). “Structured questionnaire” means a questionnaire with several possible answers (closed) in our paper. We have deleted the “structured” in order not to result in misunderstanding. The questionnaire was administered by interviewers (stated in page 6 line 2-6).
Comment 5:
What is the reason for including items 13-16 among the predictors? Why isn't loss of property or financial/material damage included?

Response 5:
In fact, we included many flood experience and flood hazard variables in initial stage, such as loss of property or financial/material damage, but only item 13-16 had statistic significance in mono-variable analysis. So we only consider the item 13-16 (see page 11 line 20-25).

Comment 6:
Why is there no information about the trauma experiences of the sample? How many dead, injured etc. What about the subjective experience (i.e fear) during the flood.

Response 6:
We have added some information about the flood in page 4 line 22-25.

Comment 7:
Results: The authors state that the two groups were almost the same in terms of study measures. Does this mean the differences were not significant? There is no mention of this in the text or in the table.

Response 7:
It means that there was no significant difference in baseline characteristics and PTSD rates (P>0.05) between the two study groups, showed in page 9 line 3-5 and Table 1.

Comment 8:
It is not clear from the text, where the cutoff value comes from. Is it derived from a total score? If not, what is the practical use? How can any other researcher make use of it?

Response 8:
We specified the cutoff value in page 9 line 20 and the use of our model in page 9 line 23-25.

Comment 9:
The authors state in discussion that all data were collected by blind method? How is this possible, and why is it necessary?

Response 9:
The blind method means that interviewers did not know who was PTSD or not at the time of interviewing. We described the method and its benefit in page 11 line 17-19.

We believe that we have adequately responded to the reviewers and hope that our paper is acceptable for publication in BMC now.

Sincerely,

Peng Huang