Reviewer's report

Title: Psychological Trauma and Evidence for Enhanced Vulnerability for PTSD through Previous Trauma among West Nile Refugees

Version: 1 Date: 29 March 2004
Reviewer: Rita Rosner

Reviewer's report:

General

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

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

1. Page 2, sentence 2: “The introduction …” needs to be rewritten.
2. Page 5: “…suggest….” Instead of “suggests”
3. Page 8: “ … in the past year display AN even …” instead of …and...
4. Page 9: Plural or singular? “typically, even a severe single traumatic event produces …

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Discretionary Revisions (which the author can choose to ignore)

General comment: This is an interesting manuscript that examines the impact of multiple traumatization on PTSD in West Nile refugees. The study describes a circumscribed but important issue and it appears to have been conducted carefully.

1. Abstract: The conclusion is not really connected to the results section and reaches far beyond the scope of the paper.

2. Background:
a) The background information could be more succinct and more closely related to the manuscripts main subject. For example reference to DSM-III (I guess this is meant in sentence two) is not necessary. But if this sentence remains in the manuscript the DSM-version and reference has to be included.
b) A missing point in the background section is the discussion of intercultural aspects of PTSD. This is from my point of view one of the biggest contributions of the study, since we have only a very limited number of studies from Africa.
c) When risk and protection factors are discussed, then the authors should mention as well peritraumatic and posttraumatic factors such as social support and life stress after trauma. Although the paper focuses on pre-traumatic factors and only on events, it should be mentioned that there are some difficulties to call them – in the special situation of multiple trauma – pre-trauma factors. After each traumatic event follow psychosocial consequences (loss of home, family etc.) which lead to a different entrance into the new traumatic event.
d) Dose-response assumptions are probably the best tested ones in research on PTSD and in the special case of multiple trauma it is – to my knowledge – unusual to find find no relationship (see text “failing to confirm a relationship”. Page 4). Rather the correlations are small like .2 , but still very consistent and in the case of war trauma most correlations are around .4. Thus the recommendation is to write this section more precisely.
e) Cumulative trauma was estimated by assessing the number of different traumatic events. Why used the authors not the absolute number (5-times shot at, three times wounded etc)? Is there a reason for choosing this type of estimation?

3. Method: It might be helpful to use more subsections in the method part, such as “Sample description”, “Measures” etc.
a) The sampling design should be explained in more detail.
b) Is the sample somehow representative?
c) 75% of participants were women. How come? Are all men dead or soldiers? Having a sample consisting of a majority of women leads very likely to higher prevalence of PTSD. In many samples the PTSD-prevalences in women are twice as high as those for men.
d) Please add some information about responder rates
e) CIDI and SRQ have to be explained and references have to be mentioned.
f) Validating the PDS with the CIDI is actually a very problematic procedure. Did you have a validated version of CIDI for this region?

4. Results:
a) The correlation given in the first part of the results section is probably a Pearson correlation. Yet as events are ordinal and the distribution is skewed I wonder whether the use of a rank correlation coefficient would not be the adequate coefficient.

5. Discussion:
a) Parts of the study have obviously been published before and are mentioned in the text. For a better understanding the quotation of prevalences (see first sentence of the discussion section) needs to be given earlier, for example in the section on sample description. Again given that there are 75% women in the sample, prevalences should be given gender specific.
b) Understanding avoidance criteria as unspecific depressive items is a very interesting observation. Did you verify that observation with your data? Maybe PTSD is not as interculturally valid as we all assume, and the avoidance symptoms are not as strong in this culture?
c) In the discussion about the "psychobiological breaking point" the very probable cooccurrence of traumatic events and major stressors such as loss of home, bad nutrition etc. is neglected. Maybe the 100% of PTSD in the high-event group are not only a consequence of traumatic events but of loss of resources as well. In summary a discussion of living conditions in the region and for this particular sample might be helpful for a better understanding.

6. Conclusions:
Although I agree on a personal level with the conclusions, the conclusions do not follow from the results of this paper. The connection should be more succinct.

7. References:
In my version of the manuscript there are two references at the beginning of the reference section which obviously belong to another manuscript on a completely different subject.

What next?: Accept after minor essential revisions

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

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

Statistical review: No

Declaration of competing interests:

None