Reviewer's report

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

Version: 2 Date: 14 October 2004

Reviewer: Joop TVM de Jong

Reviewer's report:

General

1. In the new version the authors again mention that they used a multi-stage sampling design (without adding the reference in the text). In their cover letter they refer to two publications of Karunakara et al, which are not made available, and are not published yet. One of the two papers is in press in the African Health Sciences journal which is difficult to access.

Therefore, this reviewer is not able to find out whether their description of the sampling procedure in these papers is adequate.

The authors do provide a description of their multi-stage sampling design in their cover letter. However, in the opinion of this reviewer their description is not precise enough to replicate their study. In other words, the authors should as yet provide a clear replicable description in the methods section of this manuscript.

The same applies to their training of interviewers that sounds interesting and worthwhile to summarize in the methods section.

2 Both reviewer 3 and 1 questioned the validity of the PDS. With regard to the question of reviewer 3 the authors refer to their answer to reviewer 1. This reviewer (1) still does not understand why the authors used an instrument that has only been used in the west instead of the PTSD section of the CIDI that has been used in a number of studies in African and Asian cultures. Without expressing exaggerate doubts about the universality of the PTSD construct, this reviewer still does not understand how the authors validated the PDS. The latest version of the paper does not mention a (cultural) validation procedure. Using an instrument that has only been used in the west for "the diagnosis and severity" without validation is what was meant with a 'worrisome choice'. Without validation it is not justified to mention prevalence figures because the rates are probably skewed. The authors could circumvent this discussion by just mentioning PDS sumscores - as they did in table 1 - or PTSD symptoms instead of disorder which would not essentially alter the outcome of their study.

The authors further mention that "the SRQ was used to examine the convergent validity (a construct the reviewer does not seem to be familiar with...)". They admit that the SRQ "is not perfect, that no instrument is, and that it is one of the instruments that has been developed for Subsaharan African populations". A brief reaction. First, the SRQ in a number of studies in developing countries, eg by the WHO, has shown to have a cut-off point indicating psychopathology that varies from 2 to 13. Moreover, the SRC does not pretend to provide a diagnosis which indeed makes this instrument a weak choice. Second, the PDS pretends to assess a PTSD DIAGNOSIS while the SRQ assesses anxiety and depression SYMPTOMS or SIGNS. In such a case one cannot speak of convergent validity. Maybe the authors would like to examine DIVERGENT or CONCURRENT validity? Since data are not
provided one wonders whether it makes sense to mention the SRQ at all.

Once these two issues have been dealt with adequately, this reviewer agrees with publication.