Author’s response to reviews

Title: Trauma exposure, PTSD and psychotic-like symptoms in post-conflict Timor Leste: an epidemiological survey

Authors:

Ian Soosay (i.soosay@auckland.ac.nz)
Derrick Silove (d.silove@unsw.edu.au)
Catherine Bateman-Steel (c.bateman-steel@unsw.edu.au)
Zachary Steel (z.steel@unsw.edu.au)
Paul Bebbington (rejupbe@ucl.ac.uk)
Peter B Jones (pbi21@cam.ac.uk)
Tien Chey (t.chey@unsw.edu.au)
Lorraine Ivancic (l.ivancic@unsw.edu.au)
Claire Marnane (c.marnane@unsw.edu.au)

Version: 2 Date: 30 November 2012

Author’s response to reviews: see over
Reviewer: Samantha Outcalt

Discretionary Revisions
1. Introductory paragraph of Background would be clarified by brief definition of ‘psychotic-like symptoms’, in particular differentiating how these symptoms may be distinct from psychotic disorders...

We have now included in the first paragraph:
The relatively high rates of psychotic-like experiences at a community-wide level suggests a distinction between these phenomena and clinical psychotic disorders whose prevalence rates are much lower. A diagnosis of a psychotic disorder requires the simultaneous occurrence and persistence of symptoms, whereas persons experiencing isolated symptoms appear to be more common and are less likely to attract clinical attention (Dominguez, 2011; Hanssen, Bak, Bijl, Vollebergh, & van Os, 2005).

2. The last two sentences of the third paragraph of the Background seem a bit disjointed. A smoother transition between the two thoughts may be helpful for the reader.

This has been attended to in the overall revision.

3. The third sentence in the Measures section is awkwardly worded and difficult to parse.

This section has been reworded to improve clarity.

4. While reading the Training and Procedure section, I found myself wondering how the Timorese community workers were recruited onto the research team.

We have now indicated that they were recruited though a network of NGOs – media advertising was still in its infancy in Timor at the time.

5. I am struggling with the issue of “paranoia” among a population who has experienced significant atrocities, such as the sample used in your study. It seems that a degree of suspicion and distrust would not only be normative, but perhaps even adaptive, and I am not certain that it would be accurately classified as psychotic-like paranoia.

We have expanded consideration of the normative aspect of these reactions in the discussion (paragraph 4) as follows:

It seems likely that exposure to prolonged persecution and conflict can impact on the individual’s sense of trust and security in a manner that may be adaptive. In a minority, however, these responses may create or magnify a susceptibility to paranoia and other psychotic-like symptoms, particularly when ongoing communal
divisions continue to foster fear and suspicion. The challenge is to define more clearly the boundaries between normative responses of mistrust and pathological reactions wherein suspicion and fear are transformed into frank and potentially disabling psychotic-like symptoms.

**Minor Essential Revisions**

1. In the last paragraph of the Background, the study is introduced in terms of what was done (e.g., “we examined...”), and this would be strengthened if it were framed explicitly as a research question with specific hypotheses.

   *We have reframed this section into hypotheses as follows:*

   *We hypothesized that psychotic-like symptoms would be common and associated with exposure to the cumulative traumas of conflict. We also postulated that PTSD would mediate the relationship between trauma and psychotic-like symptoms.*

2. The last paragraph of the Measures section refers to “a widely used measure of disability”. Please inform the reader which measure this is.

   *We now indicate that the measure has been used in two national mental health surveys in neighbouring Australia and provide the reference (Sanderson K & G., 2003).*

3. The Statistical Analyses section needs to more clearly indicate that the trauma count refers to number of categories of trauma, rather than number of traumatic incidents. This becomes clear later in the paper, but also needs to be mentioned here.

   *This nomenclature now is used consistently throughout the paper and a more precise account of the derivation of the categories of PTEs is given in the measures section.*

4. The limitations need to mention the exclusion of rape in the data. Although this was mentioned in the Methods section, it needs to be acknowledged that the data presented in this study likely underestimate the degree of trauma exposure since rape was not included in the analyses.

   *We now include mention of rape in the limitations section.*

5. The use of the term “outcomes” in the second sentence of the Conclusions is inaccurate. Causality cannot be determined by the design of this study. The word “outcomes” should be replaced by “correlates”. 
We have removed any use of terms that might imply causality – and also caution against drawing causal inferences in the discussion.

Major Compulsory Revisions
1. The measures used in the study should be described in more detail. For one, the instruments need to be appropriately cited (e.g., Bebbington & Nayani, 1995 was strikingly omitted). Additionally, some information about scoring procedures should be included. It would have been helpful to see a brief description of PSQ criteria here rather than being referred to another paper. Additionally, after reading the manuscript, I am left wondering how PTSD diagnoses were established. Are DSM criteria for PTSD used in the HTQ? Have the diagnostic criteria been modified to reflect this population? What does a community cut-off of 2.0 mean? Is the HTQ being used to measure both trauma exposure and identify PTSD diagnoses? A paragraph with more detail about the HTQ could eliminate this confusion.

We have amplified the description of the measures, particularly the Harvard Trauma Questionnaire in order to clarify all these issues.

2. The PTSD prevalence (5%) is shockingly low, given the degree of trauma experienced by these participants.

We note that the HTQ is the most widely used measure of PTSD across cultures – it has been shown to be robust in measuring PTSD symptoms. The rates for PTSD are low but not excessively low given the range of prevalence rates recorded across post-conflict countries worldwide (Steel et al., 2009). We note in the discussion that the conditions that pertained in Timor at the time of the study corresponded with those known to be associated with lower PTSD rates – the source of the terror has been removed (in this instance, the Indonesian military), peace had been restored by the UN and substantial time had elapsed since the cessation of conflict. In addition, Timor Leste had gained its independence. As such, the sense of security was likely to have been restored, an important factor in hastening recovery from PTSD.

3. The limitations need to more explicitly address the fact that the base rate of psychotic-like symptoms in pre-conflict era is not known.

This point now is acknowledged in the limitations section.

Reviewer: Kristine Chapleau
MAJOR COMPULSORY REVISIONS
Introduction
1) p. 5: Authors need to clearly state hypotheses for relationships between psychotic symptoms and the other variables of interest (trauma, demographics, PTSD).

We have stated hypotheses in the Aims section.
Method

2) Psychosis Screening Questionnaire (PSQ): Provide a brief description of determining PSQ positive cases (not just the citation). The authors cited reference #20 as justification for how to determine positive PSQ cases; however, that reference article does not make any mention of the PSQ or how it is scored.

We now have been explicit in defining the criterion for assigning PSQ screen-positive cases and have remained consistent in the terminology and the application of this categorical assignment throughout.

3) Harvard Trauma Questionnaire (HTQ): Authors described how the number of different traumas was measured but not how PTSD was assessed. Please describe how many items were on this portion of the survey, the scale used, and how it was scored. The authors report 2.0 for cutoff as per reference #21; however, the cutoff reported in this reference was 2.5, not 2.0.

We have now provided a comprehensive account of the measure, indicating that there are separate sections for assessing PTSD and potentially traumatic events. We provide the correct reference for using the 2.0 community cut-off (Silove et al., 2007).

4) Additionally, is there any reason not to leave PTSD symptom severity and # of different traumas as continuous variables? Of the two, I would prefer that # of traumas be kept as a continuous variable.

We explain why we analyse PTEs categorically – the dose-response relationship is rarely linear – more often, higher levels of trauma exposure have an exponential relationship with mental disorder.

5) Trauma count should be discussed in HTQ subsection for measures, not in Statistical Analyses section.

We have made this adjustment.

6) Demographics: Was age categorized in the survey or were categories created for data analysis? Again, I’m wondering if Age could be left as a continuous variable.

We do not believe that this change will make any difference to the results. In logistic regression analysis it is easier to interpret odds ratios if they are all rendered in categorical form (otherwise, to the uninitiated, the odds ratios can appear to be surprisingly “low” for dimensional variables).

Results
7) Include chi-square analyses comparing demographic differences in # of traumas and PTSD.

This has been done.

8) Coding: We believe that our approach to coding and analysis falls well within the tradition of the general traumatology field, that is, by deriving categories and comparing higher values with the lowest value as the reference point. While the reviewers’ suggestions are of interest, the approach is not standard in the field of epidemiology or traumatology where, as indicated, it is the convention to use the lowest category as the reference point to derive odds ratios and confidence intervals. It needs to be noted that there is no evidence that low or moderate levels of trauma experiences are “normative”, so that the use of mean scores as the reference point could be misleading. Using a categorical approach, we allow multi-degree modelling to examine the association of variables at different levels in relation to the predictor variable. We have been careful to ensure that the categories have sufficient numbers in each grouping to ensure statistical power. We note that the confidence intervals for the univariate and multivariate logistic regression models are all sufficient to allow interpretation, indicating the adequacy of power.

9) It may be interesting to test the reverse model in which PSQ (right arrow) trauma (right arrow) PTSD to determine which model better fits the data.

We did not mean to imply that psychotic-like symptoms were likely to lead to trauma exposure and we have modified that passage. There is no theoretical or clinical reason to expect that psychotic-like symptoms would mediate the effects of trauma in relation to PTSD and therefore there are no grounds for testing that model – it would not be interpretable in the context of current understanding of these phenomena.

10) Include Nagelkerke’s R-squared for logistic regression and model.

There is considerable debate in the literature about the merits of presenting R-squared analogues to assess the overall strength of association in logistic regression models. Nagelkerke’s R-squared is not invariant to base rate and does not allow consistent interpretation between studies. The measure has not been recommended as an overall measure of association by commentators in the field especially for multivariable logistic models and for this reason we have not reported this statistic (Allen & Le, 2008; Menard, 2000).

Discussion:
11) p. 12, para. 2: “A striking finding of our study was the strength of the association between trauma exposure and psychotic-like symptoms.
We have changed this section in accordance with this concern. We have not emphasized the higher trauma category, simply the overall association with trauma.

MINOR ESSENTIAL REVISIONS
Improving the organization of the methods and results would improve the quality of the paper.

We have revised as indicated.

12) Demographic data should be presented in Participants section, not the Results section.

The presentation of these details varies in scientific papers and we prefer presenting these data in the Results section.

13) Create subsections for measures.

We have attended.

14) p. 9, para. 3: OR is listed as 2.1%

Thank you, we have altered.

15) p. 2 Abstract: Move analysis plan to Results section and remove stats from Results section.

Again, this is a matter of variation in the style of reporting.

16) Gender should be changed to Sex throughout the paper.

Thank you – we have attended.

Reviewer's report
Reviewer: Amanda Wickett-Curtis

Discretionary revisions
1) The question seems to be clearly defined although the absence of formal diagnostic assessment of PTSD as a diagnostic category seems to limit the ability to draw conclusions based upon diagnosis (e.g. ptsd) as a SCID was not used.

We note that the HTQ was compared with the SCID in a convergence study (see Measures section).
2) There is lack of emphasis in discussion about the heterogeneity of psychotic disorders. For example, reference #12 is cited to support examination of these ideas although Scott et al. (2007) examined one facet of psychosis (delusions) versus numerous factors assessed in this study. More attention could be paid to this distinction in the limitations section of the discussion.

*We make reference to this in the Discussion when comparing rates of psychotic-like symptoms across studies.*

3) A methodological limitation should be noted and involves predisposing factors to PTSD/Complex PTSD and Psychosis involving attachment and early personality variables.

*We make reference to these issues in the limitations section.*

References


