Author's response to reviews

Title: Post traumatic stress symptoms and heart rate variability in Bihar flood survivors following yoga: a randomized controlled study

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Author's response to reviews: see over
Referee 1:

Reviewer’s report

Comment 1: The authors have done an excellent job of responding to the issue I raised in the initial review. They have adequately acknowledged and discussed the main limitations of their study: the small size of the study and the short duration. These reflect the conditions in rural disaster areas where provisions for refugees are inadequate. Communication is difficult, and population are moving and may not be in one location long enough to be followed for more than a few days.

Comment 2: Despite the obstacles and limitations, this is an interesting and valuable study that contributes to the field of interventions in the aftermath of complex disasters, an area sorely in need of more study.

Comment 3: No further revisions are recommended.

Referee 2:

Comment 1: The manuscript is much improved with the current revisions. Only these discretionary revisions remain:

Discretionary Revisions

Comment 6: Another limitation of the study is the lack of a control group that controls for attention. For example, did the yoga participants feel better just because someone spent time with them in the classes? Please discuss in the discussion section.

Response 6: There was a control group. The 22 participants were randomized as a yoga and wait list control group. We regret if this was not clearly mentioned.

Reviewer’s Response: It would seem that the authors have misunderstood the original comment. I will clarify. The question relates to the issue of the nature of the control group. In the current study a wait-list no treatment control was used. This leaves the study open to the criticism that it wasn’t yoga that produced the improvements in sadness, but rather just the time spent with participants (1 hour per day). An attention control is one in which this effect is accounted for.
For example, an attention control group could receive education sessions matched to time that the intervention group spends in yoga classes. In this sort of design, the issue of “attention” is accounted for and any improvements achieved by the yoga group are more likely to be truly due to yoga rather than “attention”.

I have decided that this comment is better placed as a discretionary revision rather than compulsory, and it is the choice of the author whether they want to include a discussion of a lack of attention control in their study in their final manuscript.

REVISED RESPONSE TO COMMENT 1:

As pointed by the reviewer it is recognized that the interaction with, and attention from an instructor has certain psychological benefits (Delbanco, 1993). The fact that the control group did not have such interaction with an instructor means that the effects seen in the yoga group (i.e., decreased sadness) and those seen in the control group (i.e., increased anxiety), could also be related to the additional attention received by the yoga group which the control group did not get. We have added this point to the revised discussion along with the relevant reference, as follows:

> In the Discussion ‘Apart from this, the fact that the control group did not have interaction with an instructor could have contributed to their increased anxiety levels, as additional care and attention given by an instructor or healer are known to have psychological benefits [29].’


Comment 2: Comment 8: Please discuss how the specific pranayama, asana routine impacted on your findings.

Response 8: We have attempted to explain how the different yoga practices may have influenced the variables assessed. ‘Previous studies have shown that the practice of yoga postures interspersed with relaxation while supine reduced sympathetic nervous activity more than a comparable period of supine rest alone (Sarang, Telles 2006). Also the same combination of
postures and supine rest delayed the latencies of certain evoked potential components which are generated in the cerebral cortex (Subramanya, Telles, 2009). Apart from this, an hour of practicing yoga postures increased the levels of the inhibitory neurotransmitter gamma-aminobutyric acid (GABA) compared to an equal duration of time spent reading (Streeter, Jensen, Perlmutter, Cabral, Tian, Terhune, Ciraulo, Renshaw, 2007). The individual effects of separate asanas have not been worked out.

Apart from yoga postures, loosening exercises (sithilikarna vyayama) were shown to increase flexibility and reduce musculoskeletal discomfort in professional computer users (Telles, Dash, Naveen, 2009). The effects of yoga breathing practices have been assessed more individually. High frequency yoga breathing (kapalabhati) has been shown to increase the low frequency power of heart rate variability suggested an increase in sympathetic nervous system activity (Lepicovská, Dostálek, Kovárová, 1990). In contrast alternate nostril breathing breathing (anulom-vilom pranayam; Raghuraj, Telles, 2008) reduced the systolic, diastolic, and mean pressure values suggestive of lower sympathetic nervous system activity hence there may have been overall effect of yoga voluntarily regulated breathing (pranayama) on the sympathetic nervous system activity in participants which may be the reason why there was no change in the heart rate variability.

References:

This comment relates to the pranayama and asana routine in the author’s study. In the comment, I am asking if the particular routine used in the author’s study had any specific implication on the study results. Do the authors think any of the pranayama or asana practices they chose to include
in their study impacted on the findings? For example, do the authors think kapalabhati is the key ingredient in their yoga routine that explains the improvement in sadness?

**REVISED RESPONSE TO COMMENT 2:**

This question is difficult to give an accurate response to, as there have been no studies assessing the effects of any of the practices used, individually, on sadness or anxiety.

Our observation has been that kapalabhati practice (at the breath rate of 1Hz; kept up for 15 minutes, with a 1 minute rest period after every 5 minutes), does elevate the mood. However at present there is no systematic study which has shown this and we cannot mention observations. Such studies (i.e., specific effects of individual practices) are being carried out by us and elsewhere and the results should be available after further research.

However in the absence of such information we have added the following sentence to the discussion:

At present it is not possible to specify which specific yoga practice may be responsible for a particular effect (e.g., reducing sadness or preventing the yoga practitioners from feeling more anxious than they did at the beginning of the week, which was seen in the non yoga/control group).

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**Referee 3:**

**Comment 1:** I think that the majority of issues raised are tackled by the authors.

However, I have one serious and major concern with respect to comment 10 (reviewer 3). The results of the ANOVA show no significant effects between groups and time (see also result section). That means that conclusions regarding efficacy of the yoga program are not justified. The authors, however, do suggest efficacy of the program in their abstract and discussion section, which is misleading.
Response 1:

There were no significant differences based on the repeated measures analysis of variance between Groups, Times of assessment or interaction between the two. Hence follow-up post-hoc tests were not done as they would have no validity (Mitchell & Jolley, 2007).

We proceeded to analyze the data using (i) separate t-tests for paired data for pre-post comparisons and subjected the resultant p values to Bonferroni correction (by dividing the p value by the number of comparisons, which was 2 in this case), and (ii) independent t-tests to compare the pre data of the two groups.

This has been mentioned in the Results as follows:

There was a significant reduction in sadness in the yoga group (p<0.05 based on a pre-post comparison with a paired t test; here p = 0.021; and after Bonferroni correction for 2 paired comparisons, p = .042) and a significant increase in anxiety in the control group (p<0.05 based on a pre-post comparison with a paired t test; here p = 0.023; and after Bonferroni correction for 2 paired comparisons, p = .046).

Despite the Bonferroni correction/adjustment this analysis is not as rigorous as an ANOVA and has a greater risk of a Type I error. This has been mentioned in the Discussion as follows:

‘The small effect size contributed to no significant changes with the repeated measures analysis of variance, hence though definitely less rigorous, t-tests were performed, which is a serious limitation in interpreting the findings.’

Also, we have mentioned that the findings were based on paired t-tests in the (i) abstract, (ii) in the Results, (iii) the Discussion (as mentioned above), and in the (iv) Conclusion.

Reference: