Author's response to reviews

Title: Physical and psychosocial benefits of yoga in cancer patients and survivors, a systematic review and meta-analysis of randomized controlled trials.

Authors:

Laurien M Buffart (l.buffart@vumc.nl)
Jannique G van Uffelen (janique.van.uffelen@monash.edu)
Ingrid I Riphagen (Ingrid.i.riphagen@ntnu.no)
Johannes Brug (j.brug@vumc.nl)
Willem van Mechelen (w.vanmechelen@vumc.nl)
Wendy J Brown (wbrown@hms.uq.edu.au)
Mai J Chinapaw (m.chinapaw@vumc.nl)

Version: 2 Date: 7 August 2012

Author's response to reviews: see over
Dear editor,

We would like to thank the reviewers for critically reviewing our manuscript. Please find our point-to-point response to the reviewers below. We hope that we have satisfactorily improved our manuscript according to the reviewers’ suggestions and that you will consider it for publication in BMC Cancer.

Kind regards,

On behalf of all co-authors,

Laurien Buffart.

Reviewer 1.
Overall this study is well organized, well written and consistent with previous studies. The author’s discussion and conclusions are appropriately supported by the data and reflect the overall trend while clearly outlining limitations, generalizations and the need for much more study.

First sentence abstract background "We aimed"...possibly "The review’s intention"...just sounds awkward.
We rephrased the first sentence of the background in the abstract.

Consider eliminating the first person language to more closely match standard review 3rd person? (no, “we” etc).
We changed some of the first person language to third person.

Reviewer 2.
Discretionary Revisions
1. Did you give any consideration to excluding the Cohen study (lymphoma) from analyses? I understand the intention of your undertaking was to be all-inclusive (in the context of your eligibility criteria), but given the clear dominance of breast cancer research in this field, wouldn’t restricting your analyses to those studies provide more interpretable, impactful findings?
We agree with the reviewer that in the meta-analyses it would better to focus on breast cancer only. We corrected this in the revised version of the manuscript.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
2. Results, sentence 3 – what is meant by “...because they did not describe the effects of an RCT.”?
The first inclusion criterion of studies was that the design of the study should be an randomised controlled trial (RCT). In total, 79 studies did not meet this inclusion criteria, and were therefore excluded. In the revised manuscript, we changed this sentence to make this more clearly (page 7)
3. Figures 3-11 are never mentioned in the manuscript. They should be referenced in the text if you want to include them.
We thank the reviewer for pointing this out. In the revised manuscript, we removed the figures as they have considerable overlap with Table 5 and may therefore be redundant.

4. Discussion, sentence 2 – I like the phrase “physical postures.” This is more specific than terminology you use earlier in stating that you only included studies with a “physical component”. Please use “yoga interventions with physical postures” throughout, instead of yoga program with a physical component.
We agree with the reviewer, and used the phrase physical postures throughout the revised manuscript.

5. Discussion, Clinical Implications – you state no adverse effects were reported. Did the publications explicitly state there were no adverse effects, or did they just not mention any? Did any of the studies actually systematically assess adverse effects?
We thank the reviewer for pointing this out. Five studies evaluated adverse events and provided this information in the manuscripts. Four studies did not report any adverse event, and one study (Bower et al. 2012) reported one adverse event of a participant with a history of back problems, who experienced a back spasm in yoga class. After evaluation by her physician, she was able to return to class and complete the intervention. We added a paragraph on safety in the result section of the revised manuscript (page 11-12).

6. Your reference section has redundancies, some articles are listed more than once.
Thank you for pointing this out. We corrected this.

We corrected this in the revised manuscript.

8. Results - What is your rationale for excluding outliers from your analyses? What defines an outlier? Was a cutoff point used, eye-balling, something else? How can you rationalize excluding outliers when the act of excluding them did not have consistent effects across the outcomes you evaluated? For example –excluding the outlier for Physical Function brought your I squared value from 87.5 to 0, but for Distress it brought your I squared value from 80.4 to 61.3 (which is still very high). Given the lack of rationale provided and inconsistent results in decreasing heterogeneity from this methodological approach, currently this is not a reasonable way to present your data.
In our opinion, it was most appropriate to present the results of the pooled analysis of all studies, and of the results when outliers were removed. This way all readers are informed about differences in results. Studies that had extreme values were considered outliers, and that was the case when the 95% confidence interval of the study had no overlap with the 95% confidence interval of the pooled estimate. Indeed, removing the outlier decreased the heterogeneity, but for some studies, heterogeneity remained high. This high heterogeneity may be caused by differences in findings, differences in instruments used to define the outcome, differences in patient groups (i.e. different stage of cancer, or different timing of the intervention with respect to primary cancer treatment) or differences in content of the
yoga program. Due to the small number of studies we were unable to conduct subgroup analysis further reducing heterogeneity.

In the revised manuscript, we added our definition of outlier in the method section (page 7), and added high heterogeneity as a limitation of the study (page 15).

9. Results, you state that there are 16 studies. After review I conclude that there are really only 13 studies if some of the publications are reporting on the same experimental population. If this is correct, please revise to state to the effect that there are 16 publications reporting on various outcomes in 13 individual studies. It is essential to differentiate between the number of actually trials, vs. publications on unique outcomes. The language throughout the results should be modified so that it is clear whether you are talking about “studies” or “publications” – for example in the quality assessment it is unclear if you are double counting your quality outcomes because you list all publications in the table and evaluate each of them separately.

We agree with the reviewer that there were 16 papers of 13 RCTs (studies). In the revised manuscript, we explained that more clearly (page 7). Regarding the quality assessment, we rated each paper separately, as for example, point estimates may have been given in one paper, but not in the other as outcomes differed among papers, or self-reported measures may have been used in one paper, but not in the other.

10. Discussion, Strengths and Limitations – please comment on the heterogeneity observed, and the impact that has on interpretability of effect sizes.

In the revised manuscript we commented on the high heterogeneity and the interpretability of effect sizes in the discussion section (page 15).

11. Figures 2-11 are all of poor quality, part of the images are clearly missing, and are therefore currently inevaluable.

We thank the reviewer for pointing this out. In the revised manuscript, we removed the figures as they have considerable overlap with Table 5 and may therefore be redundant.