Author's response to reviews

Title: Are two better than one? A systematic review of couple-based interventions for couples affected by cancer

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Author's response to reviews: see over
Dear Dr. Chap,

Please find attached the original manuscript entitled: “Are two better than one? A systematic review of couple-based interventions for couples affected by cancer” (Manuscript ID 5478465806092443)

We are most grateful to the editor and reviewers for their comments. We have revised our manuscript in accordance and are pleased to submit a revised version. The following outlines, point-by-point, the changes made to our manuscript to respond to reviewers’ comments.

Reviewer: Jane Ussher

Discretionary Revisions:
Comment: “The description of the results for the studies, under outcomes measures (pp 13-18) is slightly misleading, as many studies used multiple measures, and thus appear in many of the sections. The authors may want to consider citing studies, which showed positive effects, ranked by strength and effect size, and listing the outcome measures, which would be a more succinct summary. Other information would be available in the table, if the previous comments are taken on board.”
Response: The outcomes section was written to provide an overview of those outcomes measured in the studies reviewed. However, following this reviewers’ comments we have added in Table 1 the effect size and outcome measures.

Comment: There is some repetition in the discussion regarding modality of intervention (face to face or telephone).
Response: This section (bottom of page 21, top of page 22) has been rephrased and redundancy addressed.

Minor Essential Revisions:
Comment: There are two additional reviews of interventions for cancer carers, which should be cited in the introduction (Caress, Chalmers, & Luker, 2009; Ussher, Perz, Hawkins, & Brack, 2009).”
Response: Both of these reviews are now cited in the introduction.

Comment: “Why were studies excluded if they “incorporated pharmacological, exercise, or dietary components combined with psychosocial components”? I can understand why pharmacological and dietary interventions were excluded, but many cognitive-behavioural therapies do incorporate exercise.
Response: This criterion was applied to compare interventions that were relatively similar in their focus and to be able to tease out the efficacy of the psychosocial component. The criterion now reads: “To compare interventions that were relatively similar in their focus and to be able to tease out the
efficacy of the psychosocial component, interventions where pharmacological, exercise, or dietary elements were the central component of the intervention, with psychosocial elements being secondary, were excluded.”

Comment: “Why was a meta-analysis not undertaken? A systematic review may have been the most suitable methodology to adopt, but this needs to be explained or justified (see Mohr, Judd, & Terry, 1998; Newell, Sanson-Fisher, & Savolainen, 2002).”

Response: This has been addressed in-text as follows: “A systematic review was undertaken to explore the efficacy, content, and delivery of couple-based interventions. The heterogeneity of intervention content, intervention delivery, cancer type, outcome measures, intervention length, and follow-up length made the implementation of a meta-analysis unfeasible [40, 41].”

Comment: “The statement “Overall, couple-based interventions appear to be as efficacious (if not more) than patient-only or partner/caregiver-only interventions” is not justified by the data presented in the review, as no systematic comparison of one to one and couple interventions has been undertaken. If such comparisons are being made (para 1, p.19) specific references for the studies which are being compared (the one to one interventions) need to be given. The discussion of 2 specific studies on p.23 is not sufficient justification.”

Response: As this review is not in a position to directly compare individual- and couple-based interventions, this statement has been removed, and the discussion of the two studies that compared the same intervention for individuals vs. couples has been clarified. To elucidate the comparative efficacy of couple-based interventions and individual-based interventions, future studies need to include both couple and individual conditions, in comparison to no-intervention controls. This sentence has been added to the future directions section.

Comment: “On p.19 it is stated: “It could be suggested that the strengthening of relationships is key to achieving positive outcomes following psychosocial interventions”. I assume this refers to the couple relationship. Please clarify.”

Response: This section now reads “it could be suggested that the strengthening of supportive relationships (whether it be the strengthening of the therapeutic relationship or the couple relationship) is key to achieving positive outcomes following psychosocial interventions”

Comment: “On p.21 it is stated that “the vast majority of interventions were conducted with middle-aged Caucasian women in heterosexual relationships”. As the interventions were with couples, this reads oddly. I assume that the authors mean that the people with cancer were women; this needs to be clarified. At the same time, it is stated on p.24 that “available studies have primarily focused on men with prostate cancer, which invariably comes with an older sample”. This runs counter to the comment on p.21 – and to the demographics outlined in table 3 (71.86% female).”

Response: First part of comment has been edited to clarify that the females made-up the majority of patients in the interventions included in this review. Second part of comment has been edited to clarify that the available studies of where males are the patients have tended to focus on patients with prostate cancer. Issues regarding mean age of participants have also been further clarified.
Comment: “Gender differences in response to psychological interventions are noted on p.24, without supporting documentation. Gender differences in distress associated with cancer could also be referenced here (see Hagedoorn, Sanderman, Bolks, Tuinstra, & Coyne, 2008).”

Response: Having reviewed the studies cited a few sentences later (Lengua & Stormshak, 2000; Jacobs-Lawson et al., 2010), the statement regarding clear gender differences in response to psychosocial interventions was not adequately supported and therefore removed.

Comment: “The ‘suggestion’ that intervention effects may last longer if they strengthen couple relationships is supposition; if this is to be posited as the major explanation for the positive effect of couple interventions, more justification for this claim needs to be made, based on previous research on couples therapy, or on theory. Indeed, the finding that “intervention effects were found to be greater for patients with unsupportive partners [4]” and “couples in shorter relationships [23]” would seem to run counter to this claim.”

Response: This suggestion is based on the finding from individual-based interventions that the length of time spent with the therapist (i.e. time to build rapport, build skills, practice skills, discuss and reflect on what has been learnt) moderated the improvements made by participants in these interventions. That is, the more time they spent with the therapist, the greater the improvements. A couple-based intervention can engage both partners in this process with each other, such that they are able to build skills, practice skills, discuss and reflect together, away from their therapist, and (in the context of an experimental intervention for patients with cancer and their partners) after the intervention has finished. Some credence is then given to this suggestion by the fact that two of the studies reported greater maintenance of intervention effects for a couple-condition compared to a patient-only condition.

When looking at the sub-group analyses, the statement that “intervention effects were greater for and for couples in shorter relationships” (Kayser et al. 2010) is logical given the suggestion that strengthening relationships is key to improvements following a couple-based intervention. When looking at long-term relationships vs. short-term relationships at baseline, significant differences on QoL were found. At the subsequent follow-up points, these differences disappeared. It could be argued that at baseline, shorter relationships did not have the same tools and experience to manage stressors as longer relationships – thus the intervention effect was greater for shorter rather than longer-term relationships.

- Note the Manne et al. 2005 study has since been removed from the review as it did not fit the inclusion criteria.

Major Revisions
Comment: “The method of ranking studies as ‘strong’ or ‘moderate’ is very global – and details of these rankings are not provided in the tables associated with the paper. There are a number of recognised frameworks for making evidence based treatment recommendations, which propose a hierarchical ranking of evidence, from level I-IV, which I suggest the authors consider (National Health and Medical Research Council (NHMRC), 1999; Ropka & Spencer-Cisek, 2001). The authors may also want to consider evaluating the methodological quality of each of the randomized controlled trials using the guidelines recommended by the Cochrane Collaboration (Mulrow & Oxman, 1997), and providing the rating for each study reviewed in one of the tables (see Newell, et al., 2002; Ussher, et al., 2009).”
**Response:** The method of ranking studies was guided by a reliable and well validated tool developed by the Effective Public Health Practice Project (EPHPP; Thomas et al., 2004). This tool has been used widely to assess the methodological quality of RCTs across various fields. Moreover, BMC has recently published three reviews that have used this tool (Bowler et al., 2010; Brohan et al., 2012; Short et al., 2011). Given the demonstrated validity of the tool, and how it has been applied in this review (i.e. used independently by three of the authors), we are confident that the methodological qualities of the studies have been accurately captured.

*Comment:* “There are too many tables detailing the individual studies; table 1, 2, 4 and 5 could be collapsed and incorporated into one table (which can be presented landscape). The details of the intervention (table one) could be summarised more succinctly, making this possible. This will improve the readability of the paper significantly. The number of participants in each study, the rating of the study, and the outcome (ie was there a positive effect of the intervention) needs to be included in the table. The latter can be indicated very simply (* < .05; ** < .01). It is also important to know the effect size (d score) for each study.”

*Response:* The number of tables has been greatly reduced, and the content has been streamlined to improve readability. Study N’s, ratings, significant outcomes, and effect sizes have also been included.

**Reviewer: Lea Baider**

*Comment:* the 5 tables are excessively long; and the readers will get lost in the myriad details, which do not add any benefit to understanding the content and outcomes.

*Response:* The tables have been significantly collapsed and streamlined to improve readability.

*Comment:* Page 6, line 8 from top - the authors should expand their definitive approach, adding not only the components but the population variables such as age, socio-economic status, religiosity, culture, perception of disease, gender, specifics of identifying patient-caregiver, etc.

*Response:* Consideration was given to the inclusion of some of these variables in the first instance, however given the large variability in what was reported in the studies it was decided this would not be feasible.

*Comment:* Page 9, Inclusion criteria - "...any type and any stage..." I strongly suggest that any meta-analysis should be more restricted to illness trajectory (e.g., time of diagnosis, rehabilitation, terminal illness - similar to diagnoses classifications) that relates to gender and age, such as prostate vs. breast cancer in young women (the authors mentioned it only briefly in the study limitations).

*Response:* Given the small number of interventions, and the large variability in the samples (including time of diagnosis), it was felt that restricting to diagnosis was not feasible.

**Reviewer: JB Hopkinson**

*Comment:* There have been three recent publications reporting reviews of similar scope and a fourth focusing specifically on couple interventions for people approaching the end of life. These reviews should be discussed in the manuscript. Their findings are consistent with each other, but at odds with some of the claims made in the manuscript.

*Response:* The three reviews mentioned have now been included in the introduction. While the Baik and Adams, and Hopkinson et al. reviews provide an overview of the impact on
psychosocial outcomes, we believe the current review provides much greater detail with regard to the efficacy of the intervention for patients and partners in comparison to control participants.

Inclusion of some, but not other studies:
- Some papers mentioned in these reviews (e.g. Kalaitizi et al., 2007; Giesler et al., 2005) did not include partner outcomes. Although partners were still involved in the intervention thus could be considered a ‘couple-based’ intervention, it seems inherent that a couple-based intervention measure the impact for the couple. Thus it studies that don’t measure partner outcomes were not included (*on this point, I've since noticed that the Manne et al. 2005 paper in my review does not include separate partner outcomes, so it has been removed)

- They focused on issues regarding sexuality. This has been covered in recent reviews (Scott & Kayser, 2009), and while related to psychological distress, it was deemed to be a separate consideration worthy of more specific attention. This was not made clear in the inclusion/exclusion criteria, and has since been addressed.

References


