Reviewer’s report

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

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Reviewer: Jane Ussher

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This paper examines an important issue – whether psycho-social couple interventions for people with cancer and their partners are effective. There is evidence that one to one interventions for both people with cancer, and for partners/carers, can reduce distress. However, to date, there is no published review of couple interventions, despite the fact that relationships are recognised to be central to coping with a cancer diagnosis. This review is therefore timely, and has the potential to make an important contribution to the research literature.

In summary: The question is well defined, and the methods described appropriately. The paper is well written, and the limitations of the work are clearly stated. The title and abstract accurately convey what has been found.

Discretionary Revisions

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.

There is some repetition in the discussion regarding modality of intervention (face to face or telephone).

Minor Essential Revisions

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).

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.

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).
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.

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.

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).

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).

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.

Major Compulsory Revisions

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).
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.

References


Mohr, D., Judd, A., & Terry, P. K. (1998). Guides for reading and interpreting systematic reviews III. How did the authors synthesise the data and make their conclusions? Archive of Pediatric Adolescent Medicine, 152, 915-920.


Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a
statistician.

Declaration of competing interests:

I declare I have no competing interests