Re: Systematic Review of Psychological Therapies for Cancer Patients: Overview and Recommendations for Future Research

In a systematic review of randomized controlled trials (RCTs) that tested psychological therapies in cancer patients, Newell and co-workers (1) concluded that “the results of this review [led them] to be considerably less enthusiastic about the likely benefits of psychological therapies for cancer patients than [did] the results of other recent reviews.”

We are concerned about the potential impact of this negative message on hospital managers and doctors. They may well conclude that psychological therapies are worthless and thus decide to restrict the limited resources available for patients’ psychological needs, even though it is well known that substantial proportions of cancer patients suffer from psychological distress or from psychiatric disorders.

We would like to reconsider the conclusions of Newell and co-workers from the perspective of our clinical practice and research in psycho-oncology. First, Newell and co-workers could not perform a meta-analysis of effectiveness trials because they combined various psychological therapies for cancer patients. For example, they gave equal weight to traditional formalized psychotherapeutic interventions that were provided by a trained professional and to unconventional therapies that were not provided by a therapist (e.g., self-practice). However, in the past decades, other meta-analyses of RCTs have demonstrated the benefits of psychotherapy in mental health (2) as well as in oncology (3,4). Newell and co-workers ignore these positive results.
Second, testing the effectiveness of psychological therapies by performing RCTs is only one of many ways to improve the psychological care of patients. For instance, considerable efforts are still needed to identify valid and sensitive measures of the effects of psychological therapies. Also unclear are the best ways to implement psychological therapies in cancer patients. RCTs can only confirm or refute the efficacy of specific interventions. It is unreasonable to expect that RCTs alone will advance our knowledge of the effects and mechanisms of psychological therapies. Sound clinical research on psychological interventions (including RCTs) should be viewed as part of an iterative process involving different methodologic approaches. The development and testing of hypotheses of treatment mechanisms may be better achieved within the framework of qualitative research methods (5).

Third, Newell and co-workers used indicators recommended by the Cochrane Collaboration to judge the methodologic rigor of psychological intervention trials. However, the authors did not acknowledge the inappropriate or impracticability of these criteria in specific settings. For example, the criterion “patients blinded to treatment group” is not allowed by many Ethical Committees. The criterion “care-providers blinded to treatment group” is usually not applicable if the treatment group is to be compared with the group that received no treatment. The criterion “outcome blinded” is also difficult to implement when the goal of a psychological intervention is patients’ well-being. Consequently, in these contexts, methodologic rigor must be ensured through alternative design strategies.

Fourth, Newell and co-workers claim that it should not be too costly to improve the methodologic rigor of RCTs that evaluate psychological interventions. In this era of industrial drug development, methodologic requirements for the design and conduct of RCTs that test new drugs often require considerable financial investments. As with RCTs that test new drugs, RCTs that test psychological therapies also involve substantial costs. However RCTs for psychological therapies benefit from far less financial support than those for drugs. Given this disparity we hope that the conclusions reached by Newell et al. will not reduce the already-limited resources that are available to study how to improve the psychological care that is provided to cancer patients. The efforts actually provided by the psycho-oncology community to assess its practice and to introduce a rigorous methodology into the psychosocial field must be highlighted and should be encouraged.

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RESPONSE

Brédart et al. raise numerous concerns about the conduct and conclusions of our review (1). First, they are concerned that clinicians and administrators will conclude that psychological therapies are worthless and thus reduce their attempts to address patients’ psychological needs. We acknowledge that cancer patients have high levels of psychological needs and share the disappointment of Brédart and her colleagues that we could not produce stronger recommendations (2). However, to draw such a conclusion would be a gross misinterpretation of our findings—we did make tentative recommendations about the potential benefits of various psychological therapies but could not currently endorse their widespread implementation.

Second, they suggested that we could not perform meta-analyses because we combined groups of therapies that were too diverse. The opposite is actually true—we felt it inappropriate to combine such varied interventions (and outcomes), and therefore we reviewed results separately for 18 psychological therapies in relation to 19 outcomes.

Third, they suggested that we had ignored three previous meta-analyses that reported positive results (3–5). The first of these was published after our literature search ended and excluded many relevant trials because of missing data—one of the reasons we decided against using meta-analyses (3). It also explored only two outcomes and concluded that psychological therapies may help one but not the other (3). The second meta-analysis (4), which was not specifically concerned with cancer patients, concluded that “...limitations in outcome studies and meta-analytic reviews currently prevent us from drawing strong generalized inferences...” and noted that “There is mounting evidence suggesting that biases associated with individual studies do not cancel each other out when studies are combined meta-analytically, leading to inflated mean effect estimates for some interventions... and deflated estimates for others...” We discussed the third meta-analysis (5), which, although well conducted, used more diverse therapy and outcome combinations and included less than half as many trials as did our review. When the therapies were subdivided, less than half of the effect sizes reached statistical significance and half of those only just reached it (5). Therefore, we do not believe that the results of these meta-analyses contradict the conclusions of our review.

Fourth, Brédart et al. (2) were concerned about our focus on randomized controlled trials (RCTs) and suggested that qualitative methods may be a more appropriate way of testing therapies. Although we acknowledge that qualitative research may contribute much to developing hypotheses of therapies that may assist cancer patients, we believe there is widespread acceptance in the scientific community that well-conducted RCTs are, wherever feasible, the preferred
methodology for testing the efficacy of such therapies.

Fifth, they felt that our methodologic quality criteria were inappropriate for trials that evaluated psychological therapies. Our review dedicated considerable space to justifying the criteria used and concluding that only the care provider blinding criteria would be impossible, and then only for therapist-delivered interventions. However, we also discussed how this potential bias could be minimized by ensuring equivalency of other treatments, monitoring care providers’ adherence to protocols, and avoiding care provider-rated outcomes. Because most evaluations of psychological therapies employ patient-rated outcomes, we have had minimal difficulties convincing ethics committees of the frequent need to temporarily conceal the true nature of such trials from participants until after final evaluations. Without such blinding, social desirability bias would make any statistically significant results very difficult to accept with confidence.

Sixth, they disagree that the methodologic rigor of RCTs of psychological therapies can be improved without substantially increasing costs. As discussed in our review, we disagree, but even if it were true, we remain unconvinced that it would justify conducting poor-quality trials.

Finally, Brédart et al. (2) say that we should acknowledge, highlight, and encourage the efforts made by the psycho-oncology community to assess its practice and to introduce rigorous methods. The early part of our review does acknowledge efforts toward greater use of RCTs but found their methodologic quality had improved little over time—hence the section trying to encourage such improvements.

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