Reviewer's report

Title: Effects of Exercise on Depressive Symptoms in Adults with Arthritis and Other Rheumatic Disease: A Systematic Review of Meta-analyses

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Reviewer: Matthew P Herring

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General Comments:
This article detailed the results of a systematic review of previous meta-analyses of exercise effects on depressive symptoms among individuals with arthritis and/or other rheumatic disease(s). The authors described their evaluation and summarization of three previous meta-analyses of exercise effects on depressive symptoms among adults with fibromyalgia, concluding that exercise improves depressive symptoms in adults with fibromyalgia and that there is a continued need to meta-analytic work in this area. The purpose of this research is well-defined, the methods are well-articulated and appropriate, and the research is of interest to the field. However, there are limitations regarding design and rationale that should be addressed prior to publication. Specific comments are provided below.

Major Compulsory Revisions:
1. There are potential design and rationale issues that need clarification and revision, particularly justification regarding the included meta-analyses. The individual reviews appear to inherently differ with regard to inclusion criteria for trials included in the individual reviews. It appears that the Hauser et al., 2010 review did not entirely focus on randomized controlled trials, detailing on page 6 of the published manuscript that control groups in more than 10 studies received another active therapy. Clarification is needed regarding the selection and inclusion of these data.

More importantly, the inclusion of Herring et al., 2012 in the current review seems inappropriate and potentially misleading to the readership. The Herring et al., 2012 systematic review was not focused on exercise effects on depressive symptoms among only adults with fibromyalgia. Although the authors of the current review provide a brief statement to this point in the first paragraph of page 11 (i.e., “A third meta-analysis included exercise training (aerobic, strength or both) in adults with a variety of chronic illnesses but of which fibromyalgia results were reported separately [41].”) and a footnote of Table 2, the inclusion of and focus on an aggregated mean effect (#) and 95% confidence interval for effects (k=32) derived from exercise studies of patients with fibromyalgia taken from a large systematic review that included many disease categories, not simply
fibromyalgia, seems inappropriate, inherently different from the other two full meta-analyses included, and could mislead readers to believe that the meta-analysis conducted by Herring and colleagues was a small analysis of effects among fibromyalgia patients that was of questionable study quality.

For example, the authors make numerous criticisms of the included meta-analyses which would not apply to the full review by Herring and colleagues. In the first paragraph of page 13, the authors state that “insufficient data were reported or available to calculate small-study effects for the Herring et al. [41] study”, but the overall review by Herring and colleagues did include measures of publication bias (i.e., fail-safe number of effects, funnel plot). In the first paragraph of page 18 (7th implication for research), the authors state that none of the meta-analyses reported NNT with respect to depressive symptoms, though a NNT of 6 was reported for the full sample of chronically-ill patients examined in the full Herring et al. review. In the second paragraph of page 18 (8th implication for research) the authors also state that future meta-analyses should try to identify sources of heterogeneity, but a meta-regression analysis was conducted to examine moderators of exercise effects on depressive symptoms among patients in the full meta-analysis conducted by Herring and colleagues. Additional rationale regarding the inclusion of the Herring et al. review and more explicit information regarding the nature of the included effects being but one disease category in a larger review need to be provided.

In addition, in the first paragraph of the Discussion on page 14 and again in the third paragraph on page 17 (6th implication for research), the authors highlight that no meta-analysis on exercise and depressive symptoms in adults with osteoarthritis, rheumatoid arthritis, or systemic lupus erythematosus had been conducted. However, 8 additional studies included in the pain conditions category of the review by Herring and colleagues focused on patients with arthritis or other rheumatic diseases. Is there a strong rationale for the inclusion of effects from the Herring et al. review that were derived from studies of fibromyalgia patients but not from patients with other rheumatic diseases?

2. Though well-written, the manuscript can be shortened and streamlined. The methods, particularly the Data Synthesis section, seem potentially laborious to the reader. There is a need to describe the methodology employed, but the detail, especially regarding theoretical underpinnings of certain methodology, seems unnecessary.

3. The third criterion for study eligibility (page 4) states that exercise interventions lasting an average of at least 4 weeks were required for inclusion. As a rationale for this, the authors state in the first paragraph of page 5 that “while somewhat arbitrary, 4 weeks was chosen as the minimum length of exercise since one should expect some type of change in depressive symptoms during this period of time if the intervention truly has an effect.” Although the effects of pharmacotherapy are most often realized between 4-12 weeks, the rationale for the minimum exercise duration does not seem sufficient. Was a minimum of 4 weeks chosen a priori? Were any interventions excluded because of exercise interventions less than 4 weeks? Given the growing body of evidence regarding
the importance of exercise dose and volume, it seems particularly arbitrary to select on length in weeks alone without additional clarification. For example, is there a strong rationale for including a 4-week exercise intervention in which participants exercise three times per week for 30 minutes per session at 65% HRR but excluding a 3-week exercise intervention in which participants exercise three times per week for 40 minutes per session at 65% HRR?

Minor Essential Revisions:

1. Using total citations and average number of citations per year seems arbitrary and biased toward more recent meta-analyses, particularly since the Busch et al. review was the first Cochrane collaboration review in the area of exercise and fibromyalgia.

2. If the citation indices are retained, the total number of citations for each meta-analysis should be updated in the first paragraph on page 12.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

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