

pain once discogenic pain has been eradicated. This will apply, however, only if Peng et al. [5] are shown to be correct [2].

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doi:10.1016/j.pain.2011.01.010

## Psychological approaches have not been demonstrated to be effective for fibromyalgia

To the Editor:

We would like to commend Glombiewski and colleagues [2] for their well-conducted systematic review and meta-analysis of psychological interventions for fibromyalgia (FM). As the authors have pointed out, previous reviews have reached different conclusions on the effectiveness of psychological interventions for FM. The authors' proposal to perform a systematic review with meta-analysis has the potential to clarify this issue.

This review is of high methodological quality, evidenced by a number of features likely to reduce the risk of bias, such as following the recommendation of the QUORUM/PRISMA guideline [3], using a scale to assess quality of the included studies, assessing clinically relevant outcomes including well-defined interventions, and including a quantitative analysis of the pooled effect sizes.

Given the rigour with which this review was conducted, we were surprised to read that the authors chose to include *within-group* changes in their analyses. Subtraction of the patient's post-intervention score from the preintervention score does not provide any useful information on the effectiveness of an intervention. Effect sizes obtained from this type of analysis contain not only the effect of the intervention but additional effects such as natural history and regression to the mean [1]. While this analysis may be appropriate for demonstrating the clinical course of the condition under the assigned treatment, interpreting these effects as evidence of the effectiveness of the intervention will lead to a biased estimate of the effect.

In a systematic review of an intervention, presenting the findings from the within-group changes is likely to mislead the reader. Indeed, in the same issue, the accompanying commentary by Nicassio [4] wrongly interpreted the review findings as providing conclusive evidence of the efficacy of psychological interventions. This is simply inaccurate, and we worry that others may have reached this conclusion too.

A more accurate estimate of the effect of psychological interventions for FM is obtained when using *between-group* comparisons, and we were pleased to note that the authors have included a between-group comparison in their review, although they have given rather less emphasis to this. The results of this analysis found that for the outcomes of pain intensity and depression, psychological interventions were minimally to moderately more effective than control conditions. However, as only a small number of studies were included in this analysis, the authors appropriately commented that these findings should be considered preliminary. It is this conclusion that should be taken from the paper, one that we feel may be overshadowed by the paper's apparent methodological rigour and the author's emphasis on the easily misinterpreted within-group changes.

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doi:10.1016/j.pain.2011.01.021

## Response to the Letter to the Editor by J.H. McAuley et al.

To the Editor:

Dr. McAuley and colleagues are certainly correct in their assessment of the difference between within-group (uncontrolled) and between-group (controlled) effect sizes. This is a well-known fact. However, the authors' conclusion that the methods used in our meta-analysis [1] do "not provide any useful information on the effectiveness of an intervention" is incorrect for a number of reasons.

First, limiting the meta-analysis to only between-group effect sizes would have excluded the majority of studies, resulting in biased and unrepresentative estimates of the current state of evidence.