Dear Sir,

I commend Burton et al. [1] for publishing their review in the journal. Important reviews should be subject to the widest readership and most rigorous critique if they are to properly influence future practice. Reviews that are solely conducted by small groups in academia can miss out on the wider stakeholder engagement journals’ offer that encourages debate and fosters learning.

A feature of the method used was to review other reviews without recourse to the original research. There is a danger that this perpetuates an original conclusion that may itself have been flawed or gives greater weight to a conclusion than the source data warranted.

For example, in relation to cognitive–behavioural approaches, the authors cite three reports (their Box 4). One of these is a review prepared by a team including two of the current authors [2]. That review relied on yet another review [3]. Schonstein et al. wrote: ‘All the trials with positive results were of work-related interventions. This might suggest that interventions that include a cognitive-behavioral approach and are work related are most effective. However, one must be cautious when interpreting this post hoc analysis as it is based on comparisons between trials.’ As the source data have passed between reviews, these words of caution have been lost.

In one of the other cognitive–behavioural approach references, Marhold et al. [4] reported that their programme was not effective ‘for patients on long-term sick leave’.

The evidence for cognitive–behavioural approaches may be considered compelling in some selected populations but the cautious and limited conclusions of previous reports do not provide, as the authors suggest, ‘strong’ evidence for ‘MSDs in general’.

Finally, many of the studies on interventions for musculoskeletal illness are of patients with back and neck pain. Where is the evidence that back pain interventions can be generalized to upper limb disorders?

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References