Reviewer’s report

Title: Effects on pain and disability of education to facilitate knowledge about chronic pain for adults: a systematic review with meta-analysis.

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Reviewer: Adrian Traeger

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Reviewer Report

Effects on pain and disability of education to facilitate knowledge about chronic pain for adults: a systematic review with meta-analysis.

This is an interesting review on the effects of educational treatments on symptoms of pain and disability in patients with chronic low back pain and widespread pain disorders. The review is generally well conducted and is at low risk of bias according to the newly published ROBIS criteria (http://www.robis-tool.info). This study is therefore likely to add important information to the body of knowledge on education and chronic pain. Education is widely believed to be an effective stand-alone treatment for patients with chronic pain. The results of this systematic review suggest otherwise, and that any evidence for its effectiveness is preliminary at best. I would suggest the following changes to strengthen the manuscript for publication:

Major Compulsory Revisions

Introduction
1. The gap in research is not clearly defined in the introduction. In the discussion, the authors cite 7 previous reviews on the effects of education on chronic pain, 3 of which performed a meta-analysis. What is the gap in research that has been left by these previous reviews that the current review aims to address? What makes this a new question?

2. Although it was a secondary objective and tested post-hoc, some background on testing the effects of education on psychosocial outcomes is needed.

Methods
3. The search was conducted 18 months ago (pg 6, para 1). To ensure the results are up to date, the search should be run again. Evidence from any additional studies would strengthen this review and increase the interest level.

4. Papers were excluded if they assessed education in specific conditions, rather than chronic pain in general. 5 of the included studies were on chronic pain/fibromyalgia, but 4 were on chronic low back pain. If the authors consider
chronic LBP to be widespread chronic pain this is fine, but it should be specified in the exclusion/inclusion criteria (pg 5, para 2). Also, what is meant by “specific” conditions associated with chronic pain could be better defined. For example, some readers would consider IBS to be non-specific condition.

5. Some readers might consider a meta-analysis of 2-4 studies a little premature. What criteria was used to decide whether to perform a meta-analysis or not?

Results

6. The most interesting forest plots are Figures 2 and 4. The remainder could be in the supplement. Tables 4a and 4b are the most informative and should be visible in the results section.

7. Table 1 would be clearer if there were separate columns for education intervention and comparison intervention.

Discussion

8. The wording is slightly ambiguous in the first paragraph of the discussion (pg 16, line 338). PNE was only effective in the very short term, and only when compared to a different type of education. The data appear to better reflect preliminary evidence that PNE is effective immediately after treatment and only when compared to an anatomy/physical activity lesson. This should be the conclusion regarding PNE.

9. Education appears to only influence short-term outcomes, if anything. The authors do not reflect on why this might be. What is the likely impact of a small, short-term effect on disability in a chronic pain population? Catastrophising changed significantly in PNE but was recently shown to not mediate the effects of pain on disability (Lee et al Pain. 2015 Jun;156(6):988-9). Factors other than catastrophising might therefore explain the improvement in disability following PNE. Some more discussion on mechanisms behind any potential effect of increased knowledge on pain or disability is needed. For example, the authors suggest that effects of PNE are through reconceptualising hurt vs harm (pg 18 para 2). How exactly might this affect pain or disability?

10. Potential for bias in the primary studies needs to be discussed, particularly for those with positive results. Table 3 has ticks in boxes for 6 studies indicating low risk of bias for blinding participants and personnel. It is not possible to blind therapists and is questionable as to whether one can blind participants in an education trial. This warrants interpretation in light of the positive results for PNE.

Conclusion

11. For the same reasons as point 5 above, the authors should make the conclusion more specific. There is preliminary evidence that PNE has immediate effects on disability when compared with anatomical/activity management education. There is also a typo on line 435.
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.

Declaration of competing interests:
I declare that I have no competing interests