Reviewer's report

Title: Sensory retraining for patients with chronic low back pain: A pilot Randomised Controlled Trial.

Version: 1 Date: 14 January 2014

Reviewer: Kieran O'Sullivan

Reviewer's report:

This is a novel, timely and valuable pilot study. It adds some valuable data to what we know about novel approaches to the management of CLBP. The main limitations of the study have been well acknowledged by the authors. Therefore I believe it is worthy of publication once the authors have made some minor amendments.

Discretionary Revisions

• The use of several different terms for this type of assessment and treatment may confuse the reader. In fairness to the authors, the terms they use are used widely in the literature. However, they might look at the various terms they have used (e.g. Sensory retraining in the title, Acuity training in the running head) and choose one – or as few variations – as possible.

Minor Essential Revisions

Abstract:
• correct brackets - [RMDQ])
• table 2 – why no subject 11?

Introduction:
• “Tactile acuity training can improve pain and two-point discrimination performance in patients with CRPS [5].”…compared to tactile stimulation alone?
• “needed TO build on this work” ?
• Provide some brief reasoning (maybe at the end of the 2nd last paragraph) for the inclusion of a qualitative component, as this is first mentioned in the final paragraph.

Methods / data analysis:
• Add bracket ) after RMDQ

Results:
• Page 12, line 3 - patient’ –typo?

Discussion:
• Page 14 – first sentence of 2nd paragraph – had “a” statistically better
outcome?
• Limitations section: “generalisability” – typo?
• Limitations – no intention-to-treat analysis was performed as far as I can see, despite 9/24 participants being lost to follow-up. This should be acknowledged

Conclusion:
• “Future work, should consider the need for automated devices, to make home delivery more feasible” – remove commas?

Major Compulsory Revisions
• I feel that the abstract and conclusion should reflect more clearly that for 3 of the 4 main outcome measures (pain and disability, both in absolute terms and as % change from baseline) there was no difference between the groups. And that the one significant change between the groups was in favour of the placebo control group, suggesting the active intervention was ineffective.
• I feel the authors could emphasise the importance that novel interventions should be capable of being applied in typical settings more strongly. It would seem to me that one of the main differences between their results and that of Moseley et al (on CRPS) is the degree of interaction with a clinician, with Moseley et al having a clinical interaction every weekday. It would appear that the approach used by the authors of this study is much more representative of typical NHS practice, and yet was ineffective. This suggests that trying to integrate this type of intervention into existing practice is not warranted. With this in mind, the study by Johnson et al on graded motor imagery for CRPS treatment in the NHS seems to be a natural fit with their conclusions, and would be worth adding to their discussion.
• The conclusion does not adequately address the possibility (probability??) that in the current formats available, there is no strong evidence to suggest that tactile acuity training techniques are an effective treatment, or treatment adjunct, for CLBP

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

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