Reviewer's report

Title: Integrating the promotion of physical activity within a smoking cessation programme: Findings from collaborative action research in UK stop smoking services.

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Reviewer: James Byron-Daniel

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Review

Manuscript: Integrating the promotion of physical activity within a smoking cessation programme

Taylor, Everson-Hock and Ussher

Overall this is an interesting paper using an underused methodological technique to design and modify a potential intervention. I think the question posed by the authors is sound and the action research methods are well described. The paper in general well-written and is concise, it is clear from the paper what was done and why it was done in that way.

I do have a number of reservations about the work. I am particularly concerned with the use of quantitative statistics to evaluate the ‘effectiveness’ of the self-help book in increasing advisers promotion of physical activity in stop smoking clinics and increasing self-efficacy in relation to PA and quitting in the smokers themselves. With only 7 advisers the analysis on page 10 seems fairly arbitrary and at worse potentially misleading. The authors have qualitative comments from the advisers regarding PA use and its promotion in stop smoking services. It seems to me it would be much more appropriate to analyze these comments rather than performing t-tests on 7 people on self-report measures. I can understand why the authors have done this, however this is a piece of action research and I am not convinced that the reporting of quantitative statistics (in this paper) sits that well with the theory behind it.

The statistics performed on data from the smokers themselves is better however the only significant findings are related to willingness to use PA as a cessation aid, and self-efficacy for smoking cessation (I am unsure how this is related to PA?). I assume these are based on Transtheoretical model constructs. I point out to the authors there is a plethora of research that seriously questions the value of ‘stages’ of change and how little intention is related to actual behavioural change.

In relation to the above the authors make too much of relatively weak findings (in terms of whether there intervention increased PA etc) and mention little about the limitations of the study (or the analysis). This needs to be much more explicit. As it stands this is misleading.
Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

I think this is a good piece of research, however I recommend the write up be changed, putting the emphasis onto the qualitative elements of the study, i.e. developing PA promotion tools for smoking cessation advisers and also qualitative analysis of the 7 stop smoking advisers comments. The quantitative analysis of the adviser’s data should be removed and the authors need to be clear that the analysis of the smokers themselves is severely limited i.e. power, no control group, theoretical issues with measuring intention to change and limitations of this.

Add on page 3 at least a few sentences on the work (lab based etc) that has found significant effects of Physical Activity (PA) on smoking withdrawal. The points made about a lack of interventions are valid, however I feel the authors need to make the case for looking at PA at all in this group, as it reads now this case is not made sufficiently to merit researching PA in smoking cessation contexts.

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