Reviewer’s report

Title: Applied public health research - falling through the cracks?

Version: 2 Date: 12 June 2009

Reviewer: Graham Moore

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This is an interesting article, which makes some good points. However, I feel that many of the arguments presented within it ought to be clarified and strengthened before it is considered for publication.

Major compulsory revisions

1) Page 5 – The ethics of randomisation.
This argument is underdeveloped. At present it looks like it is based on personal opinion ‘we considered it unethical’. Many would argue (and there is a substantial body of literature on both sides of this debate) that, as there is little robust evidence that exercise referrals schemes ‘work’, there is sufficient clinical equipoise to consider randomisation ethical. There is a common line of argument amongst RCT proponents that it is only unethical to deprive the control group of a service if you are confident that service is of benefit to them – a position which has yet to be reached with exercise referral, given the current evidence base. The authors need to make clear how they would counter these arguments. Is this because the service exists already? What would be the implications of dismantling the service and setting up a randomised trial?

2) Page 5- Recruitment and attrition.
I believe that most exercise referral RCTs have seen fairly equal attrition between intervention and control groups rather than large control group attrition as you anticipate here. Why do the authors anticipate that their study would be different? This statement needs further clarification and justification.

3) Page 7 – Efficacy versus effectiveness.
I would be careful about suggesting that RCTs are interested only in efficacy and not in real world effectiveness. This ignores the move towards pragmatic randomised controlled trials (such as the current evaluation of the National Exercise Referrals Scheme in Wales), which attempts to embed RCT methods into the scheme’s rollout. Here and elsewhere in the article (e.g. page 5), I feel that the authors need to put a little more thought into explicating the trade-off between internal and external validity, which appears to be one of the main cruxes of their argument. In what ways would one expect a randomised controlled trial to lack external validity by comparison to a simple before and after design? Can the authors think of any examples? There are some highly relevant recent articles, such as one by Gidlow et al. (2008) which make points similar to
those by the authors, but are not discussed.

Minor compulsory revision

4) Page 7 – Stakeholders dependence on continuation of service.
Some clarification here is needed of the authors relationship with the stakeholders and implications for the research process. Degree of independence between the study and the scheme. Likely pressures for positive results?

5) Page 7 – Dissonance between types of evaluation required and types research bodies.
This is mentioned several times throughout the manuscript. However, it could be made clearer what it is that the authors mean by this statement.

Discretionary revision

6) Page 6 – Non-randomised study (or quasi-experimental design).
Some good points here. In addition, what incentive would patients in control areas have to enter the study if they knew at baseline they had no chance of entering the service straight away or (if put in a wait list control group) after a 12 month waiting period?

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I am currently myself involved in the process evaluation of a randomised controlled trial of an exercise referrals scheme