Reviewer's report

Title: The effectiveness of community engagement in public health interventions for disadvantaged groups: A meta-analysis

Version: 1 Date: 4 March 2014

Reviewer: Suzanne Richards

Reviewer's report:

Comments

Major compulsory revisions:

1. Although the authors have provided a PRISMA checklist, they have not adhered fully to PRISMA guidance http://www.prisma-statement.org/ for the reporting of their review. Although reference 10 provides source documentation, the omission of some methodological detail is a major limitation that needs to be addressed. I have tried to identify areas where this occurs. Greater clarity of reporting is required.

2. This paper is long and difficult to follow due to the complexity of the data presented. I did wonder whether it would be better to drop some of the data (indeed reference 10 suggests it might be available elsewhere) and provide a more focused paper referring to a smaller number of key messages, and perhaps where the data are more robust/conclusive. The ideas are important, but I did struggle to get to them.

3. Discussion. Whenever you do a meta-regression of this sort, combining data from a large number of diverse studies, I am generally uneasy at the appropriateness of combining such data statistically. That you can come out with a ‘d’ value doesn’t mean that you should, if you are essentially mixing ‘apples with pears’ i.e. ‘clinical’ heterogeneity is present. My sense when reading the descriptive data was that this may apply here. However, I acknowledge that the academic community is split over the appropriateness of such analytic approaches and there is no clear consensus. In the discussion the authors talk about the appropriateness of statistical techniques. Perhaps they could also reflect a little on the issue of clinical heterogeneity too here.

Minor essential revisions:

Comment 1: Page 4 ‘problems with the designs of the primary studies (e.g. the time to follow-up in the mortality studies etc).

Can you rephrase this sentence as it is unclear. Do you mean ‘primary prevention’ studies. The phrase ‘mortality studies’ doesn’t really mean anything – do you mean studies which included mortality rates as an outcome? Just a few more words here would help?

Comment 2: Page 5 paragraph 2. You define the term ‘community’, but you don’t add any specific references against this term. Did you follow any specific
definition or did you create your own? The same could be said for ‘engagee’, ‘participant’ and ‘deliverer’, although for these three terms I suspect you had to create your own definitions to suit your study aims.

Comment 3: Page 5 paragraph 3.

The control conditions include ‘no or minimal community engagement’. How did you define minimal engagement and operationalise it?

Secondary aims. Might be helpful to give a few more examples of the potential effect modifiers here – it is a quite a long time before you describe this in any detail later in the paper.

Comment 4: Use of acronyms (e.g. DARE, NIHR HTA, DoPHER, NHS EED, TRoPI) in the search strategy. For researchers outside the UK, many of these acronyms won’t mean a lot. You may need to spell some of the more UK-centric terms out more fully. Also – did you really not search the major electronic search databases of PubMed and EMBASE? I appreciate this is only a brief synopsis of a fuller report – but you might need to add a little more here to help this paper stand alone.

Comment 5: Page 7 paragraph 1 after bullet list. What are the policy objective areas of the Marmot Review? This is a major eligibility criteria which contributed to excluding over half of the potentially eligible studies. Please could you describe them more fully to assist international readers.

Comment 6: Page 7 – data extraction section. Please briefly describe how the process of data extraction was conducted and quality assured (e.g. were data double extracted by two independent reviewers?).

Comment 7: Page 8. What are ‘community outcomes; or ‘engagee’ outcomes? You need to define your outcomes more fully – perhaps in a web-supplement if the lists/definitions of each type of potential outcome are lengthy.

Comment 8: Page 9: What are ‘outcome evaluations’ – do you mean a systematic review assessing effectiveness (irrespective of study design)? Can you reword please?

Comment 9: Page 11. The description of ‘what are funnel plots’ could be shortened. Notwithstanding this, the authors do not state explicitly how the funnel plots were used to ascertain the presence of bias. Was it based on visual inspection of the funnel plot? Please state explicitly.

Comment 10: Page 12, paragraph 2. You refer to seven systematic reviews that failed to meet minimum methodological standards to be regarded as systematic reviews – but nowhere in your eligibility strategy do you define or cross-reference to what these are. At this point in the text, I am becoming confused as to whether your focus is on systematic reviews or trials (randomised or non-randomised). I think the problem is that the eligibility criteria and selection of studies aren’t that clear in the methods section. If this could be revised, then the results section will become clearer.
Comment 11: Results section – description of studies included in meta-analysis sub-heading (including studies, participants, interventions sub-headings). This is a vitally important section, but the descriptive data are sparse across a range of core areas. There are also no cross-references to tables which describe the components of the selected studies to allow the reader to fill in the gaps. This section is brief to the point it is impossible to get a feel for the selected studies based on the data given. For example. When describing the ‘comparators’ used in intervention studies – the authors state that the largest groups of evaluations employed usual care comparators (n=38, 29%), while 13 (10%) had a comparator that differed only from the intervention by not involving community members. However, 131 studies were selected for inclusion. What were the other comparator groups?

Comment 12: Presentation of results data. There are a substantial number of meta-analyses presented, and the results section is difficult to follow given the sheer volume and complexity of the data. From a technical perspective, I did wonder whether it was wise to prevent pooled estimates of effect (for example, page 15 – Maintenance of intervention effects on reported health behaviour follow-up outcomes d=0.09) when significant variation was observed on heterogeneity testing. This test is telling you that the pooling of data isn’t working well.

Comment 13: Discussion – practical significance sub-heading. This list is a really useful summary of the key findings. However, some of the recommendations lack comparators making it difficult to interpret some of the recommendations. For example “Interventions tended to be most effective in adult populations and less effective in general populations for health behaviour outcomes.”. What is the comparator to the ‘adult population’ of the ‘general population’. Please review this list, and ensure the comparators are clearly stated to help the reader.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

'I declare that I have no competing interests'