Author's response to reviews

Title: Costs and effects of a 'healthy living' approach to community development in two deprived communities: findings from a mixed methods study

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Author's response to reviews: see over
Dear Ms Neilan

1866082081366577 - Costs and effects of a 'healthy living' approach to community development aimed at reducing health inequalities in two deprived communities: findings from a mixed methods study

Thank you for forwarding the reviewers’ comments to our submitted paper and inviting us to address the points made. We have used this as an opportunity to strengthen our paper.

Please find below our detailed responses to the comments made. The reviewers’ comments are marked in bold and our response appears below each. We also attach our revised paper which highlights the changes we have made to address the comments.

Reviewer 1: Sven-Olaf Isacsson

Comment 1
What can we learn from this project in other countries than UK? Most references are from UK. Action research of different models have been used for many years in many countries. When telling that grassroots approach is effective the reader is interested in evidence for this. Why did it work in some settings and not in others? Where and when? And is it true that top-down is completely ineffective?

We have included further information and international references in the background section to address this point. These are highlighted in yellow on pp4-5. Our paper does not argue that top-down approaches are ineffective, but we do make the case that it is high risk and that bottom-up approaches are increasingly being used.
Comment 2
This may lead to conclusions regarding what you missed in your own planning process. This should be included in your discussion. As it is now your paper is mainly a description of what you have done without any effort to describe why you were not so successful. What was innovative—testing new hypotheses? Could the citizen use mail etc? If you give information on this important question maybe we can learn something new. It is hard work behind your project and your methods and results are well described and acceptable.

We accept that further discussion would be beneficial about the less successful aspects of this study and have added this to the section about Study Implications in the Discussion. The innovative aspects of this study are: a) the community focus of the healthy living approach and use of existing resources rather than centring efforts on a building; and b) the health economics evaluation of our action research approach. Our amendments in response to the reviewer’s comments are highlighted in green on p23. We hope we have understood the point and answered it fully.

Thank you for recognising the hard work that all participants contributed and on which this project is based.

Comment 3
SF 36 is well known but you should inform the reader about how to interpret the score.

We agree that our SF36 results should be more clearly presented and have therefore revised Table 3. We have also amended the results section within the text. These changes are highlighted in pink on p15. Table 3 is on p36.

Comment 4
You have described why so few participated at the second interview but was the main reason lack of interest based on how you were running the project?

Low participation at second interview was due to loss of some community researchers, which was a consequence of longer timescales, community mobility and community upskilling. We have added a sentence in the study limitations section to explain this, highlighted in turquoise on p25.
Reviewer 2: Muhammad Kamrul Islam

Comment 1
The title of the paper seems not compatible with the discussion in the paper. Particularly, the issue - 'reducing health inequalities' - is not explicitly (may be it is implicit) discussed in the manuscript although state in the title. I suggest, if the authors would like to have such expression in the title, more emphasis should be given (explicitly) how authors specify 'health inequalities' per se.

We have removed the phrase ‘aimed at reducing health inequalities’ from the title. As stated in the background (p4, para 2), the funding programme for this study aimed to reduce health inequalities. However, since this is not referred to any further in the manuscript, we agree with the reviewer that the title of the paper should be amended.

Comment 2
Further amplifications are needed for justification of doing the research. In particular, to understand the story- ‘the intervention’ need to be clarified further.

We have added an introductory sentence to the section about the intervention and also a box clarifying the Health Living Approach. These additions are written in red lettering on pp5-6.

Comment 3
Clarification also be needed about the “effects of a ‘healthy living’ approach”. Particularly, on the indicators of the effects of implementation of the HLA.

We have amended the wording of the study objectives to specify the indicators of implementation which are reported in the results section. We hope this clarifies the reported effects of the Healthy Living Approach. The amendments are written in blue font on p7.

Comment 4
‘Before and after study design’ (as used in the study) may be endured with the ‘recall bias’ (a systematic bias which may occur when the way a survey respondent answers a question is affected not just by the correct answer, but also by the respondent’s memory) which would have been a significant threat to the validity of the study. To convince the readers on this concern, explanations would be warranted.

We agree that the limitations of our before and after study design should have been discussed and have included a paragraph which explains the points made by the reviewer and clarifies why we considered this design was the most appropriate in the context of this study. This addition is within the study limitations section and written in grey font on p25.
Comment 5
Small number of respondent with low response rate (year 1: 44% and in year 4 with new entry: 42%) and the high turnover of people in year 4 may cause two sorts of biases - selection bias and attrition bias. Selection bias is certainly a common and potentially serious problem, and qualitative study (similar to quantitative study) needs to recognize the consequences of selecting extreme cases of the outcome they wish to explain (see e.g. Collier & Mahoney, 1996).
Attrition of the original sample represents a potential threat of bias as well, if those who drop out of the study are systematically different from those who remain in the study. This issue may be important as attrition can negatively affect the internal validity of the study.

We accept this comment. In addition to the additional discussion included in our response to Comment 4 above, we have also acknowledged the risk of selection and attribution bias again in the Study Limitations section. This addition is highlighted in purple font, also on p25.

Comment 6
Though the authors indicate some limitations of their study, the issues particularly, selection bias and recall bias should also be discussed. Another potential source of bias in qualitative research is the interviewer bias. Readers may be sceptic about the results and conclusions if the authors would not clarified how their work has not been endured with such biases. It may be important to discuss how such biases would be at least minimal for this study.

We agree that these limitations should have been included. We have taken the opportunity to highlight the issues of selection, recall and interviewer bias and have discussed how we sought to minimise these. These points are included in the Study Limitations part of the Discussion and are highlighted in green font on pp25-26.

Comment 7
Page 10, para 1: as author noted “Volunteers (community members and others who freely gave their time) are acknowledged as a vital resource in the intervention. Their time was recorded but not valued in money terms as part of the costing exercise.”
However, it is widely accepted and recognized that any sort of time cost must be included in any costing or cost effectiveness analysis. Particularly, from the societal perspective all costs, such as the value of volunteer time should also include in any cost analysis (see e.g. Gold et al, 1996). In the 2003 Handbook on Non-Profits Institutions in the System of National Accounts the United Nations Department of Economic and Social Affairs provides an overview of several methods for valuing volunteer time.

We have included a money value for the volunteer time in the discussion, as suggested. These comments are highlighted in turquoise on p26. We have also updated the abstract to express the economic valuation as cost per community. This is also highlighted in turquoise on p3. Drummond has been used to reference the fact that there are various ways of putting a money value on volunteer time and these methods are reviewed in economic evaluation text books including Gold et al and also the UN Handbook on Non-Profits Institutions in the Systems of National Accounts.
Comment 8

Overall, I think, it is not crystal clear that the results and conclusions of the study are adequately supported by the data.

We have found the comments made by Reviewer 2 very helpful and have made a number of changes and additions to our paper in response to his observations. In addressing his comments, we hope that we have managed to more clearly and explicitly show that our results and conclusion are adequately supported by the data.

Thank you for the opportunity to respond to these comments and amend our paper. We hope we have addressed the points made and, in so doing, have been able to improve the quality and clarity of our submission.

We look forward to hearing from you.

Yours sincerely,

B A Evans