Author's response to reviews

Title: A systematic review of the effectiveness of individual, community and societal level interventions at reducing socioeconomic inequalities in obesity-related outcomes amongst children

Authors:

Frances C Hillier-Brown (frances.hillier@durham.ac.uk)
Clare L Bambra (clare.bambra@durham.ac.uk)
Joanne-Marie Cairns-Nagi (j.m.cairns@durham.ac.uk)
Adetayo Kasim (a.s.kasim@durham.ac.uk)
Helen J Moore (helen.moore@durham.ac.uk)
Carolyn D Summerbell (carolyn.summerbell@durham.ac.uk)

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Author's response to reviews: see over
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Dear Sir/Madam,

We would like to thank the reviewers and the editor for their very helpful and constructive comments. In this document we describe how we have addressed these comments—we have numbered these comments. We have made are highlighted in yellow in the manuscript.

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Editor’s comments

E1. page 6: first sentence needs some further clarification and consideration. We have amended this sentence, which we now hope is clearer.

E2. page 7, lines 170-172: the criteria used to determine the methodological quality of the study could be described briefly and referenced. We have added a brief of the tool used to assess methodological quality of studies, and a reference.
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Reviewer's report – Reviewer 1
Is the question posed by the authors well defined?
R1.1 The aim of the review is well defined. However, the author could have referred more to international literature to explain and describe the problem and known or hypothesised related causal determinants. For example, what is known about inequalities and effective approaches and interventions to tackle them in other public health problems? What are the most important factors and working mechanisms of successful examples in other fields (e.g. smoking behaviour)? (Major Compulsory Revision).
We have included three additional paragraphs in the introduction to our paper which refer to this important literature.

Are the methods appropriate and well described?
R1.2 The methods are well described. The author has chosen to distinguish studies by defining 4 levels of interventions and 3 approaches to health inequalities. This has lead to a disappointing amount of studies included in this review. Would another framework probably have been more useful? In other words: is the “system” used the most appropriate to examine the effectiveness of intervention and approaches? For example: it might be interesting to know whether there is a difference in effectiveness between interventions which are targeted to disadvantaged groups and those that make use of tailored strategies, are developed in cooperation with the target group themselves or make use of participatory action research in the developmental phase.

Or whether primary prevention programmes are more/less/even effective in decreasing or halting inequalities compared to selective prevention programmes.

Another possible perspective to take into account could be the difference in use of theoretical background of interventions. Which are more likely to decrease inequalities: those who are based on Social Cognitive Theory, theory of Planned Behaviour or designs based on a more "holistic" framework, including theories of Environment–Behaviour Relationships and factors such as automatic behaviour (habit strength) and cultural factors. (Discretionary Revisions)

We spent some considerable time and thought in choosing a framework for how we might best categorise the studies to be included in this review and, as the referee points out, could have used a number of different methods, all of which would have been interesting. However, we chose to use this framework in our published protocol (Bambra, C., Hillier, F., Moore, H., and Summerbell, C. (2012) Study Protocol: Tackling inequalities in obesity: How effective are
public health interventions at reducing socio-economic inequalities in obesity amongst children? BMC Systematic Reviews.1:16,) and do not feel it is appropriate to now change our approach. We have included an additional paragraph in the section under ‘How interventions can impact on inequalities in childhood obesity’. Please also see changes in response to 1.6

R1.3 Further I would suggest that process evaluation studies could have been studied as well for a better understanding of the (lack of) effect in decreasing or haltering inequalities. This might provide information for intervention developers and policy makers to increase the quality of programme design and implementation (Discretionary Revisions).

We agree that a ‘sister’ review of process evaluation studies would have provided a better understanding of the effect of interventions. Referee No 2 raised a similar point – please see response to R2.12

Are the data sound?
R1.4 Yes, the tables are clear as well. However, it is unclear how the author estimates positive outcomes of interventions which are directed at those who are disadvantaged. It is unknown whether the intervention would have had any impact on advantaged groups if the interventions would have been aimed at the entire social gradient. (Major Compulsory Revision)

Thank you for raising this important point. Referee No 2 raised the same point – please see our response to R2.13

R1.5 There is one remark to make regarding the presented results of the Jump-in intervention. The author states that the effect-evaluation of the Jump-in intervention did not found intervention effects on BMI or waist circumference. That is correct, however, significant beneficial intervention effect was found on the main outcome: organized sports participation. Moreover, effects were stronger for girls and for Moroccan and Turkish children who are less likely to participate in regular sports offers compared to the general population of school children. (Major Compulsory Revision)

With regard to the first part of this point, concerning the main outcome of the Jump-In intervention, we apologise for this error. We have clarified the ‘types on interventions’ included in our review in response to this point, which is similar to one of the points raised by Referee No 2 – please see our response to R2.3. In addition, we have added summary results for the main outcome into the summary table for the Jump-In and other interventions where the primary outcome was not a measure of obesity, but instead was related to diet or physical activity behaviours (Simon 2008; de Meij 2011; Herrick 2012; Burgi 2012).

We did not use ethnicity as a proxy for SES for this review (which was part of a larger review project) (see http://www.phr.nihr.ac.uk/funded_projects/obesity.asp). Therefore, although
we understand that the effect sizes observed for the different ethnic groups in the Jump-In intervention are very important, we do not feel able to comment on such findings as they are beyond the scope of this review. Including differential effects by ethnicity (or faith or culture) would have led to many more studies being included in the review. Therefore, commenting on these effects for just the studies we have included in this review would only tell part of the story. We have included an additional note about this point in the text under ‘Types of interventions’.

Are the discussion and conclusions well balanced and adequately supported by the data?

R1.6 I would like to refer to my remarks at 2. The author could elaborate more on this in the discussion. (Major Compulsory Revision)
Please also see response to 1.2 and 2.12. We have added comments about alternative frameworks that could have been used on p21 under limitations “Other ways of categorising studies (such as by whether primary prevention programmes are more/less/even effective in decreasing or halting inequalities compared to selective prevention programmes), or by examining the theoretical underpinning of interventions (such as those that are based on Social Cognitive Theory, Planned Behaviour or theories of Environment–Behaviour Relationships) could also have been used”.

R1.7 Another important question is whether different cultural backgrounds between studied low SES groups should be taken into account in the explanation and comparison of intervention outcomes, since study populations were from Asia, European and American countries. We agree that a review of ethnic/faith/cultural inequalities in obesity would be most interesting. Please see our response to R1.5. We feel that a comparison of results using the same intervention in different ethnic/faith/cultural groups would be particularly interesting. However, comparing results from different interventions in different countries would be more difficult to interpret with confidence. We have included reference to an ongoing evaluation which aims to provide useful information about the impact of the EPODE intervention on socioeconomic inequalities across Europe (Mantzki 2014).

Are limitations of the work clearly stated?

R1.8 The author should be more critical regarding the framework used for the systematic examination of intervention studies. (Major Compulsory Revision)
Please see our response to R1.2 and R1.4
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Reviewer's report – Reviewer 2

Minor essential revisions are:

R2.1 I have some concerns about the way in which some interventions have been classified as individual or community. For example, in Margaret Whitehead’s paper (the basis for the classification), life skill groups are classified as individual-level interventions, with the focus on education of individuals. The four group-based educational interventions described in lines 329-337 and lines 354-368 would appear to fall into this group. According to Whitehead, there are very few true community-level interventions, and the rationale for why the authors have classified interventions as they have needs to be more explicit. To inform this, it may be worthwhile considering the intent of the intervention – is it to strengthen individuals (increase ‘agency’) by targeting behaviour change, or is it more structural (targeting conditions in which the behaviour occurs)?

We have included an additional paragraph about why we have classified these group-based interventions as community, rather than individual, interventions, in the section ‘Types of Intervention’. We appreciate the referee’s insightful suggestion to consider the intent of the intervention as an aid to classification.

R2.2. The aim of the review (lines 103-107) should state that interventions can be prevention or treatment-based, and the word ‘obesity’ should be changed to ‘obesity-related outcomes’ rather than just ‘obesity’, as not all interventions target obese children only.

We have amended the aim. We have also amended the title of the paper and other text in the paper where relevant, for consistency.

R2.3. In types of intervention, please also add that interventions can be prevention or treatment-based.

We have included an additional sentence in this section, which provides more clarity on the types of interventions we included. We have also added this sentence to the abstract, under methods.

R2.4. It would be very helpful to report the broad search terms in the body of the text.

We have included all of the search strategies in additional file 1. We do not feel that adding broad search terms is particularly useful; however, if you would like us to include these in the body of the text we suggest the following text: “We used search terms related to obesity, for example ‘body mass...”
index’, ‘weight gain’ and ‘body fat’, intervention type, for example ‘health promotion’, ‘policy’ and ‘community’, and study design, for example ‘evaluation’, ‘intervention’ and ‘RCT’.

R2. 5. Line 154 – please add in a brief justification of why only studies with a duration of 12 weeks were included (and is there a reference to support this?)
We have included a brief justification and reference in the section ‘Types of studies’.

R2. 6. Line 187 – indentified should read identified
Thank you – this has now been corrected

R2.7. Line 188 – retrived should read retrieved
Thank you – this has now been corrected

R2.8. Line 232 – should read ‘intervention group (IG) compared with control group (CG)
Thank you – this has now been corrected

R2.9. Line 264 – trail should read trial
Thank you – this has now been corrected

R2.10. Line 363- leading should read lending
Thank you – this has now been corrected

R2.11. The sub-headings within the results section don’t seem to have a consistent format which makes it difficult to follow. Overall, I think this section would benefit from more ‘sign-posting’ sentences to provide direction for the reader. It would also be very helpful to briefly describe the aims of each study, but if this not possible because of the word count, could the authors mention for all studies whether interventions were preventive or treatment-based.
We have revised the sub-headings in the results section to improve the format. In Table 1, we have now included information on the aim of each study.
R2.12. The discussion would benefit from some analysis of the potential mechanisms that underpin why some interventions were successful and others were not: 1) for where interventions appear to be similar in their aims/content but have different effects; and 2) for the different levels of interventions (e.g. potential reasons why school-based nutrition and PA education and exercise sessions or family-based education weight loss programmes were successful).

An additional paragraph has been included in the discussion section, where we explain that we have tried to unpick why some interventions had a positive impact on inequalities in obesity, and others did not. Following your comments, we went back to the original study papers but were unable to find any consistent programme components that shone through, beyond those themes which we had already mentioned in our original submission. We have added an additional paragraph to the end of the section on ‘What works in reducing inequalities in obesity-related outcome?’ in the Discussion. In this paragraph we mention the importance of process evaluations, and that having information from these would have helped us to better identify themes, which would then allow us to postulate potential mechanisms.

R2.13. Lines 437, 475 and 503 refer to ‘interventions reducing inequalities’ – I’m not sure this is actually the case (assuming this statement is referring to targeted interventions conducted in low SES children only). If these interventions had been conducted across the entire socioeconomic spectrum, they may have been more effective in higher SES than in lower SES children. Thank you for raising this important point. We have revised the sentences you have highlighted, and provided clarification. We agree that if these interventions had been delivered universally, they may have been more effective in higher SES children. In the Introduction section, we have added a new paragraph on interventions targeted at low SES geographical areas, and their ability to reduce inequalities.

R2.14. Table 2: It’s not clear how the studies are organised in this table. Are they ordered by effect on inequalities, then gradient vs. targeted? If so, this should be explicitly stated. It would in fact be better if they were ordered according to how they are presented within the text. If there is room, it would be very helpful to include an (abbreviated) aim for each study. Studies have been re-ordered in the table to how they are presented in the text as suggested. Study aims have also been added to the tables.

R2.15. Studies 20, 34 and 36 need more information about the setting.

More information has been added for study 20. Unfortunately the setting details are not well reported for studies 34 and 36 and therefore unclear. This is now stated in the tables.
Discretionary revisions

R2.16. Tables 5-8 seem almost unnecessary. Could this information be added to Tables 1-3?

We agree that these tables may be of limited interest to many most readers, and have removed these from the main article and added them as an additional file. These tables were requested for our main report (available at http://www.phr.nihr.ac.uk/funded_projects/obesity.asp) by one of the reviewers. These data are not available for all of the studies included in our review and we believe that including this information in the main summary tables may be confusing. However, we do want these data to be accessible for those who are interested and suggest that the additional file provides an acceptable solution.

Once again, we thank you for your useful and insightful comments and hope that you find our revisions satisfactory. I look forward to hearing from you.

Yours faithfully,

Frances Hillier-Brown

Research Associate
Wolfson Research Institute
Durham University, Queen’s Campus
University Boulevard
Stockton-on-Tees
TS17 6BH
Tel: +44 (0)191 3340456
Email: frances.hillier@durham.ac.uk