Reviewer's report

Title: Intervention components associated with increased effectiveness in dietary and physical activity interventions

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Reviewer: Stefanie Ashford

Reviewer's report:

This study reports on a systematic review of existing systematic reviews of diet and physical activity interventions aimed at individuals with risk factors for type 2 diabetes, and therefore brings together a large amount of diet and physical activity intervention literature. The authors have picked a coherent search strategy for identifying relevant reviews and have used a standardised grading system for analysing the quality of the evidence presented in each review. The authors have clearly completed a very thorough quality assessment, using a standardised questionnaire, and as such have only included high quality reviews based on this assessment.

Whilst this review could be useful for those aiming to develop diet and physical activity interventions, there are a substantial number of issues that require attention.

Major Compulsory Revisions

Key points:

• The authors need to make it absolutely clear in the abstract and title that this is a review of reviews, at present it reads as though the authors have conducted 129 analyses relating intervention components to effectiveness. Please make it clear that this is purely a summary of the analyses of the included reviews, so as not to mislead potential readers.

• Overall the authors should be more tentative in their interpretation of their findings and the subsequent recommendations offered. The review is extremely comprehensive, covering a number of different types of systematic reviews covering different populations, behaviours and analysing a variety of outcomes. As such there is a large amount of heterogeneity between the studies included, thus recommendations given on the basis of comparing and summarising these should be tentative. Particularly in the case of where the argument is backed up solely by the results of one systematic review.

• The primary and secondary outcomes of this review are not clearly defined, I would suggest these are made much clearer to aid comprehension. Specifically, the authors state that the secondary outcomes are for both types of intervention; dietary and physical activity. However, self-reported change in dietary behaviour surely should be a secondary outcome of dietary interventions only, and likewise
cardio-respiratory fitness should be an outcome for PA interventions only?

- The authors do not differentiate between evidence of absence and absence of evidence. Specifically it is unclear if there is evidence which suggests there is no association between a particular characteristic i.e. theoretical basis and outcome, or if there is presently no evidence assessing whether such an association exists. This differentiation should be made clear throughout the manuscript.

Introduction

1) Please describe in what way current interventions are too intensive, and where is the evidence? Further, what diabetes prevention programmes are out there, are they currently ineffective or just too intensive?

2) I think it could be clearer here about what is referred to by intervention components and characteristics, does this mean intervention techniques e.g. goal setting, or other intervention characteristics such as setting, delivery mode etc. Or is it both?

3) Is there any reason why the review is focusing on individual level interventions only? Is there evidence that these are more effective/cost-effective?

Methods

1) Can the authors explain why the search was only for the years 1998-2008, were there no reviews in this area pre-1998 or was this for pragmatic reasons?

2) The PICO Criteria needs to be made substantially clearer to make the inclusion and exclusion criteria more explicit. Further, the review specifically mentions cardiovascular disease twice in the inclusion criteria but there is no mention of this disease in the introduction, what relevance does this have to type 2 diabetes interventions? The link between type 2 diabetes and CVD needs to be explicit for readers who are not experts in this field.

3) A little more detail on the use of the SIGN grading system would be appropriate here, although I am aware there is much more detail included in the supplementary material Table S5. Many readers may be unfamiliar with this grading system, and given the weight it has on the final study findings, it is essential this is carefully articulated in the methods section. Specifically the sentence starting ‘quality of the evidence for each analysis is described as……’ should be reworked. Please state what the ++, + and – refer to and why, and which types of review fall under which category (causal/associative).

4) I am unsure whether the title of Evidence Grade 1 as ‘causal evidence’ is correct. According to the authors’ description, reviews would be categorised as this if they included RCTs and which statistically compared two randomised groups either descriptively or by meta-analysis. You cannot assume that the studies within these meta-analyses conducted mediation analyses for the particular characteristic under consideration, which therefore makes it difficult to infer a causal link between characteristics and effectiveness as a consequence
of a review only. Consider changing this to reflect what actually differs between the two gradings (1 & 2), which from my understanding refers to the type of review included.

Results:
Overall the results section could be substantially reworked to aid the readers’ comprehension, in particular:

1) The authors may want to think about removing the low quality evidence from this section as I am unsure this really adds much weight to their argument. Particularly as on a number of occasions it is stated that the review is focusing on higher quality evidence.

2) Where possible please include effect sizes and/or odds ratios, particularly of significant findings in the results section. Readers may not have time to look through all the tables for this information.

3) I am unclear on how the authors defined intervention techniques across reviews, and how they sought to evaluate the effectiveness of the specific techniques. Further, it is confusing that the authors summarise evidence of the use of ‘well defined techniques’ based on reviews included in this study and state they also used the Abraham & Michie taxonomy, why use both? There is surely some overlap in the techniques; self-monitoring, relapse prevention, barrier identification identified in your the reviews are certainly also included in the taxonomy.

4) I am unsure why the Michie et al (2009) meta-regression of effective techniques in healthy eating and physical activity interventions (ref. 31) has not been included in the section on behaviour change techniques. I am unsure why this has not been included here as this paper provides evidence of specific behaviour change techniques associated with effectiveness. I would suggest this is included in these analyses.

5) On page 9 evidence from the Ogilvie (2007) meta-analysis is referred to as medium quality, yet on page 8 evidence from the same meta-analysis is described as low quality. Can the authors explain this?

6) Although the authors state that interventions that included certain techniques were effective/ineffective at bringing about the desired change, they are unable to say that it was the particular technique itself that was effective/ineffective unless moderator analyses were conducted in the reviews. Other intervention characteristics might have moderated the impact of the behaviour change technique itself. Please make sure this is clear both in the results and the limitations section of the discussion.

7) In the “behaviour change technique” section of the results the authors state that change in diet and/or physical activity was greater in interventions which targeted physical activity and diet. This does not make sense.
8) I am unsure why the authors have described the difference in effectiveness of group versus individual interventions. It is quite clearly stated earlier in the manuscript, and in paragraph 1 of the discussion, that the review is summarising individual-level interventions.

Discussion

1. Para 2, line 2. Please include references to the statements about effectiveness, so it is clear which study/studies are being referred to.

2. No explanation is offered for the findings, specifically in relation to previous diabetes prevention programmes mentioned in your introduction. What has been discovered that is different to what has already been found? Given that the review aimed to enhance these, how and why would the recommendations do this?

3. A number of current diabetes prevention programmes are referenced, why are these not referenced in the introduction as this seems to be a focus of the review?

4. Whilst I do acknowledge that increasing physical activity, reducing weight and improving diet can prevent type 2 diabetes, and can therefore see the logic behind the inclusion criteria; would it not be appropriate to conduct a systematic review of these interventions with moderator analyses to evaluate the effectiveness of specific intervention components and characteristics? Instead of attempting to summarise a large heterogeneous sample of reviews of physical activity and diet interventions which are not specifically aimed at preventing diabetes.

5. The limitations section could be further expanded, it does not currently acknowledge the limitations of the present review, instead focusing on the limitations of the included studies.

Discretionary Revisions

1) Abstract: Line 2. It is critical that the intervention........are retained. Does this mean retained from current diabetes prevention programmes?

2) Introduction: It would be helpful to make the aims clearer. Specifically, is the aim to a) summarise the evidence of existing type 2 diabetes prevention programmes, to extract the most important intervention components from these to improve upon their current effectiveness? Or b) to summarise the evidence of physical activity and dietary interventions, not necessarily specific diabetes prevention programmes, with the expectation that effective components could be incorporated into future diabetes prevention programmes?

3) Methods: It is worth stating who completed the searches of the databases, as has been done for the other aspects of the methods section.

4) Methods: In the inclusion criteria the authors state that this review includes
interventions for individuals at risk of type 2 diabetes. However in table S1 they do not detail what these risk factors are in a number of studies, indeed from the table it seems one review includes RCTs with ‘health community dwelling individuals’. How would this fit in with the inclusion criteria?

5) Results: The authors could include a little more detail regarding the characteristics of included reviews i.e. population and delivery settings, and describe the number of descriptive, meta-analytic and meta-regression analyses.

6) Results: The authors may want to use subheadings to aid the readers’ comprehension, this is particularly in the case of “characteristics of the target population” in which gender, age and ethnicity and risk are all attended to.

7). Discussion: The authors state that the interventions produced clinically meaningful changes, please define “clinically meaningful”.

Minor issues not for publication

• Asterisks of included studies are missing in the reference list.

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 declare that I have no competing interests