Author's response to reviews

Title: Effectiveness of interventions to promote healthy diet in primary care: systematic review and meta-analysis of randomised controlled trials

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Author's response to reviews: see over
Dear Editor

Thank you for your communication dated 28th October 2013. We are grateful to the reviewers for their consideration of the manuscript and thank you for the invitation to submit a revision. We are now enclosing a revised version of the paper that we have modified as follows:

Reviewer #1:

1. Introduction

This review evaluates the effectiveness of interventions to promote healthy diet in primary care. This is an important issue with the increase in obesity in most countries. The Introduction deals with these issues but there should be more discussion of the specific issues related to overweight and obesity and its impact on morbidity and mortality as well as the burden in primary care. Also the argument that PHC is well placed to deliver advice should be expanded on and referenced.

The following sentences have now been added to the end of first paragraph on page 3 and reads as: “Obesity imposes a significant burden of morbidity and mortality on populations. The health care costs associated with obesity are substantial and the vast majority of the costs are attributable to treating health consequences of obesity including type 2 diabetes, cancer and cardiovascular diseases”.

Also, another sentence is added to the beginning of the second paragraph of page 3 and it reads as: “There is evidence for the effectiveness of primary care-based interventions to promote physical activity [9], alcohol reduction [10] and smoking cessation [11].”

References have been added in the sentence in second paragraph in page 3. “The regularity of patient consultations in primary care[12], and the value that patients place on medical advice[13], offer opportunities for general practitioners to play important roles in promoting health and preventing disease.”
2. Methods

The methods are well described. However, there should be definitions of primary care and dietary promotion intervention. The validity of exclusions needs more explanation. Was a sample of the excluded studies checked? Was there a threshold amount of physical activity in the excluded interventions as many diet interventions contain reference to energy balance?

We have now added the following lines defining primary care and dietary promotion intervention in the context of manuscript in Page 5:

Primary care in this context refers to interventions delivered through the first point of contact in a health care system, where the service provider acts as the principal source of advice to patients, rather than through specialist referral. Dietary promotion intervention in this context means any methods which are used to promote healthy diet, including healthy eating advice and counselling, telephone calls, group lectures or use of any other dietary education materials including posters, booklets and guidelines.

We have also added the following line in Page 5:

“We excluded multifaceted interventions including those with physical activity promotion along with diet promotion and we did not set any threshold amount of physical activity for exclusion.”

Also the interventions considered are very heterogeneous. Thus there needs to be greater justification that meta-analysis of these heterogeneous studies is appropriate.

In the description of the analysis, it is stated that where the baseline data were not reported, the difference in the mean outcomes between groups at follow up was used. Surely this means that the outcomes reported are heterogeneous? Can the authors explain how this is taken into consideration in the inferences they have drawn?

The paper reports on tests for heterogeneity in the meta-analyses. We also report on the range of intervention strategies and the extent to which these draw on behavioural theory. We have now modified the Abstract so that it reads: “The design and delivery of interventions were diverse with respect to grounding in behavioural theory and intervention intensity.” (Results section) and “Presently-reported interventions to promote healthy diet for primary prevention in primary care, which illustrate a diverse range of intervention methods, may yield small beneficial changes in consumption of fruit, vegetables, fibre and fat over 12 months. The present results do not exclude the possibility that more effective intervention strategies might be developed.” (Conclusion section)

3. Findings

The findings are well presented. It is important to describe the intensity of the intervention. In addition to the number of contacts, it would be useful to have some idea of the duration of contacts. While there is reference to face to face contact, this is not quantified.

Thank you, a new column has been added to Table 2, which provides details of the intensity of the intervention.
Reviewer #2:

Major points:

1. This reviewer is surprised about the differences in the results obtained by the authors of this manuscript and those reported by Ress K et al (ref. 11) in their Cochrane systematic review. The causes of such big differences should be analyzed and discussed in the paper.

   We have now added the following lines to page 15, under the heading “Comparison with studies”.

   “The Cochrane review included studies with participants with chronic conditions, as well as participants at high risk of colorectal cancer and breast cancer. Studies were carried out in faith and work settings as well as in primary care. These differences in participant characteristics, resulting from differences in inclusion/exclusion criteria between our study and Cochrane review may explain these differences in results.”

2. It seems that the author were very restricted in the selection of the studies included in the review. Why they exclude studies on asymptomatic participants with high-risk status or even established medical conditions? If the aim of the author was to evaluate the effectiveness of intervention to promote healthy diets among participants with "healthy status", why they do not select studies on community-based participants? The rationale of their study should be explained better.

   We have now added the following lines at the end of page 5:

   “In order to focus on a population approach to primary prevention, we excluded trials which included participants who were pregnant, or with existing chronic conditions, or at high risk of diseases such as colorectal or breast cancer, or with participants who were relatives of family members with chronic health problems linked to diet. Such high risk participants or those with established chronic conditions linked with diet, may be more motivated to make dietary behaviour changes which may apparently show higher effectiveness of interventions promoting healthy diet in primary care. Also, there may be possibilities of diet restrictions which may limit the participation in diet promotion intervention and may apparently show lower effectiveness.”

   We have also modified the wording of the objectives to mention primary prevention.

3. The results of this review are too descriptive. I miss a deeper analysis of the results. For instance, comparing the mode of administration or the diet assessment tool (telephone interview, etc.) with the final results,

   Thank you. We noted (page 12): “A descriptive analysis was implemented because the small number of included studies meant that it was not possible to conduct meta-regression analyses to formally test the impact of intervention characteristics on observed effect sizes.”

   We do on to observe (page 12) that: “For fruit and vegetable intake, assessed either singly or in combination, and for serum cholesterol, effect sizes did not clearly increase with increasing participant contact. In contrast, for fibre and fat intake, the interventions with the most contacts clearly had the largest effects. None of the interventions that contributed data to pooled effect size estimates for fruit, vegetable or fibre intake were based on theory. However, for fruit and vegetable consumption considered together, interventions based on theory appeared to have larger effects than those not based on theory. In contrast, the most effective interventions for reducing fat intake were not based on theory, nor was there any clear benefit of using theory for reducing serum cholesterol. There was no clear relationship between the total number of intervention techniques used in an intervention and the effect sizes observed.”
Minor points

1. Explain better why trials comparing one type of diet promotion intervention with another only were excluded? (page 5)

Now we have added the following sentences and explained why we did not include trials comparing one type of diet promotion intervention with another, in the beginning of page 6.

“..because our aim was to estimate the effect size difference between a diet promotion intervention and the existing usual care or no intervention; we did not aim to compare any two methods of diet promotion interventions.”

We hope you agree that these changes have improved the paper. Thank you for considering this paper for possible publication in BMC Public Health.

With best wishes

Yours sincerely

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