Author’s response to reviews

Title: The Effect of Culinary Interventions (Cooking Classes) on Dietary Intake and Behavioral Change: A Systematic Review and Evidence Map

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Author’s response to reviews:

Re: NUTN-D-18-00122

The Effect of Culinary Interventions (Cooking Classes) on Dietary Intake and Behavioral Change: A Systematic Review and Evidence Map

Dear Mr. Madsen,

Thank you for your letter of January 31, 2019. We have carefully considered the comments we received, revised the manuscript accordingly, and believe it is improved as a result. Point-by-point responses to each reviewer and editorial comment follow below.
Thank you for your interest in our work.

Sincerely,

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Megan McCrory (Reviewer 1):

1. Line 97. Ovid is listed twice, please remove one.

Ovid was repeated before each database because it was the interface. For clarity, we deleted the term “ovid” and left database names.

2. Lines 145-146. Need to define what the authors considered favorable and unfavorable nutrients.

In the revised manuscript, we provided a clear definition of what would be considered favorable and unfavorable. These definitions were based on the original studies. Favorable nutrients were fruits, vegetables, low-fat dairy, whole grains and fiber, lean protein, and unsaturated fats. Unfavorable nutrients were fast food, high carbohydrate foods and high sugar desserts, saturated and trans fats, and high sodium foods. Thank you for your comment.

3. Line 148 and 160. Need to define or elaborate on what the authors considered as "adequate" data and "insufficient" data.

In the revised manuscript, we clarified that adequate data needed to meta-analyze continuous outcomes are mean and dispersion measures (such as a standard deviation).

4. Line 171. Please add an explanation for what "hand searching" entails.

We clarified that this meant asking clinicians with expertise about the topic area to identify additional references.

5. Line 174. Please add and explanation for what is meant by a "pre-post study".
Pre-post studies, or before-after studies, were defined in the study selection paragraph of the methods (line 113-114 (in the new version of the manuscript)), as studies that compared outcomes after a follow-up period to baseline. This type of studies lacks a control group.

6. Lines 191-192. By definition, RCTs are randomized, yet line 191 says one of the problems was lack of randomization. Please clarify.

In the revised manuscript, we changed “lack of randomization” to “inadequate randomization procedures”. Thank you.

7. Lines 195-196. "Have" should be "had" (use past tense).

Done

8. Lines 206-207. Revise last sentence: The overall mean difference was -0.07 etc.

Done

9. Line 210. Use "was" instead of "is".

Done

10. Lines 208-215. Although these changes were not statistically significant, the changes are impressive for just a cooking class. Perhaps this deserves a comment in the discussion. Clinically important? Also, were these changes independent of changes in body weight? Please clarify.

This is a good point. In the revised manuscript, we added a sentence in the section of “practical implications” to highlight that these changes can be meaningful to patients and clinically important, even if they were not statistically significant. These changes in LDL, SBP and DBP were not independent of changes in body weight. Two studies that reported change in LDL, SBP and DBP, also reported change in BMI and we do know from RCTs and systematic reviews that change in weight is associated with change in these parameters.

11. Lines 218-291. Change end of sentence to "...which also did not change significantly (p=0.58)."

Done

12. Lines 233 and 237, and Table 1. The title of the table has "effect modifiers" not covariates. Please review and make consistent with explanation on line 233.

Done. We deleted “confounders” from line 233.

13. Line 251. Explain what the authors define as "better" energy density. Lower?
According to Curtis, et al, dietary energy density was calculated and expressed as kJ/g of food. This definition has been added between brackets instead of the previous comment about this outcome.

14. Line 255. Should say, "…significantly greater declines…"

Done

15. Lines 270-271 and abstract. Would it not be more correct to say that there was no moderating effect of characteristic of the interventions such as its delivery or intensity (Table 1)?

Cooking classes’ outcomes may be modified by these characteristics and that may be proven if there were larger trials with longer follow-up and higher quality. In other words, absence of evidence does not mean evidence of absence. Therefore, we think it may be better to leave our comment the way it is.

16. Lines 284-316. This section consists mostly of several 1-2 sentence paragraphs rather than well-developed, fewer paragraphs. Please revise.

This section was rewritten to be less choppy and now reads better. We also added 2 subheadings for better clarity. We thank you for pointing this out.

17. Tables and figures/supplementary materials. Recommend moving Tables 1-2 to the Supplementary Material, and moving Supplemental Figures 1-4 to the main paper as Figure 3A-D (i.e all on one page).

We agree that tables 1 and 2 are better suited for the appendix and we have moved them.

Reviewer 2 (Reviewer 2): PEER REVIEWER ASSESSMENTS:

PEER REVIEWER COMMENTS:

GENERAL COMMENTS: The intervention of interested is relevant, therefore, there are methodological issues that are unclear if they were addressed. If not, it can impair best practice. The authors performed a broad search for studies (multiple databases), they assessed the risk of bias of included studies and performed adequate statistical analyses.

We thank the reviewer and we have addressed their subsequent comments.

REQUESTED REVISIONS:

ABSTRACT
The abstract is missing clarification of the study eligibility criteria, participants and definition of what is culinary intervention. It could also have a more detailed description of the study appraisal and synthesis of methods. The results described in the abstract are missing the p-values for the associations and meta-analyses. It could also describe the limitations of the study, implications of key findings, and the registration number for the protocol of the systematic review.

Thank you for your comment. We have added the systematic review objective to the abstract. We also clarified study eligibility criteria, definition of culinary intervention. We clarified study appraisal and data synthesis. We added the weighted mean difference values with their 95% confidence intervals. The protocol for this systematic review is not registered.

Introduction

- The authors should provide an explicit statement of questions being addressed in the PICOS format (participant, intervention, comparisons, outcomes and study design)

We have clarified the study objective in the introduction (and abstract). We have also explained and clarified the PICOS in the introduction. It is also explained in the methods, study selection.

Methods

- There is no indication of the existence of a review protocol, and if yes, where it can be accessed.

This systematic review follows a priori established protocol. The protocol was not registered.

- Missing specific study characteristics (for example, PICO format, length of follow-up). Unclear whether authors excluded studies not published in English language.

The PICO is explained both in the introduction and methods, study selection. The length of follow-up is not a criterion for our protocol, but we extracted follow-up duration and number of sessions and commented on it in the results, description of the intervention section.

- unclear whether there was a protocol for this systematic review available.

This systematic review follows a priori established protocol that was not registered.

- unclear criteria for inclusion of study designs

We have clarified that in the study selection section of the methods.

- unclear criteria for inclusion regarding participants of interventions
As described in the introduction and methods, we have included participants of all ages and health status.

- unclear dates of last search strategies conducted in the databases, if contact with authors was made to identify additional studies or details.

The search was done in May 2017. We contacted some authors to request some missing data.

- no access to appendix where the search strategy should be available, therefore I was unable to access the quality of the search strategy.

The search strategy is included in the appendix.

- The inclusion of multiple populations (children, healthy adults or adults with morbidities), is concerning because they are different in many aspects, therefore should not have been assessed together.

We agree with the reviewer that children data would be better represented separately. Unfortunately, there is very few studies to allow for meaningful comparison based on age. In the forest plots, we added a notation to pediatric studies for better clarity. We also draw attention to this clinical heterogeneity in the limitations section.

- Unclear which types of additional intervention components were considered for inclusion. Also, the studies with multiple components should have been assessed separately.

We did not restrict the type of additional interventions and we have them listed in table 2. We only excluded studies with more than two additional interventions.

More than two thirds of the included studies had one or two additional components. With the limited number of studies assessing one outcome, factoring that in would not allow for any analysis to be done. We believe our work provides a good summary for the literature available on cooking classes. This systematic review demonstrates the need for additional research, while providing a blueprint for higher quality designs that minimize confounders and heterogeneity as much as possible.

- unclear which are the primary and secondary outcomes of interest. Ideally, maximum of 3 primary outcomes and 7 secondary outcomes.

We categorized two types of outcomes, cardiometabolic outcomes and behavioral outcomes. The label of primary and secondary was originally designed to reflect power calculation in individual trials, thus, it is less useful to a systematic review. In the revised manuscript, methods section, we categorize the outcomes into these two broad categories. We thank the reviewer for suggesting the need to make this section more clear.

- No description of methods of additional analyses (sensitivity or subgroup analyses).
We did not perform sensitivity or subgroup analyses. There were no sufficient data to allow such analyses.

In the data extraction and risk of bias assessment, there is the description of using Newcastle-Ottawa Scale, which is used for observational data. Previously, it was mentioned inclusion of "studies with cooking classes and outcomes were compared after follow-up period to a control group ou baseline (ie, pre-post). These designs does not configure observational design. Therefore, it is unclear if observational cohort studies were included or not. If yes, they should be assessed in a different SR or analyzed as a subgroup.

This is a terminology issue. An interventional study that is not randomized can be called an observational study (here, the intervention would be the exposure in a cohort study). This follows Cochrane and GRADE writings but others may differ. In this systematic review, we clarified that we included randomized and nonrandomized studies as long as they had a comparison group, or the comparison was the baseline value of the same group.

- the authors did not state the principal summary of measures (eg, RR, the difference in means)

The principal summary of measure was the mean difference. We have clarified that in the data synthesis and analysis section of the methods.

Results:

- Risk of bias for each study is not presented.

The risk of bias of each study is presented in tables 3 and 4 of the appendix.

- studies were not described per subgroup (either of study designs or types of participants);

Type of participants and other study characteristics are available in table 1 of the appendix. Study design descriptions are listed in tables 3 and 4 which also present the risk of bias assessment.

- the meta-analysis figures are not available for the reader;

The forest plots were in the appendix but now we have moved them to the main paper.

- the results appear to be described all together, not considering the study design. Ideally, the authors should separate the results of RCTs from the results of non-RCTs.

The revised forest plots indicate randomization status. The narrative identifies RCTs when relevant. We agree with the reviewer that ideally we would have a sufficient number of studies to determine differences in effect estimates between RCTs and non RCTs.