Author’s response to reviews

Title: The impact of food reformulation on nutrient intakes and health. A systematic review of modelling studies

Authors:

Carlo Federici (carlo.federici@unibocconi.it)

Patrick Detzel (Patrick.Detzel@rdls.nestle.com)

Francesco Petracca (Francesco.petracca@unibocconi.it)

Livia Dainelli (Livia.Dainelli@rdls.nestle.com)

Giovanni Fattore (giovanni.fattore@unibocconi.it)

Version: 1 Date: 14 Oct 2018

Author’s response to reviews:

Reviewer reports:

Gabriel Masset (Reviewer 1): Dear BMC Nutrition editors,

I thanks you for the opportunity of reviewing the review article entitled "The impact of food reformulation on nutrient intakes and health. A systematic review of modelling studies". I believe the manuscript is clear and well written, the review methodology is well described, and the results are correctly summarized. Although the topic of food reformulation has gained interest in the recent years, evidence regarding its potential impact on public health still appears quite limited. The submitted manuscripts thereby gives a good overview of current studies and highlights adequately their limitations. Yet, I feel the discussion could include more recommendations from the authors: they list limitations and should give more emphasis on possible solutions or further research needs. This would allow to better use the manuscript as a guide for future research. Also, the tables need some editing to make easier for an external eye. I would recommend that this paper is being accepted for publication, following such improvements. A few minor comments are listed below.

We thank the reviewer for the relevant comments. We broadly revised the discussion section and incorporated a number of recommendations and future research needs. In the next lines we reply punctually to your specific comments.

p. 3, ll. 17-9. The definition of reformulation is a bit unclear, and the reference is quite odd. I thought reformulation meant changing recipes of food and beverages product. If you apply a public health version of the terminology, it could be stated more clearly.
We agree with the reviewer that the definition of food reformulation must be clearer. The following definition was used instead: “Reformulation is defined as the process of altering a food or beverage product's recipe or composition to improve the product's health profile”

p. 4, ll.4-26. I would recommend that you merge all these sentences in one paragraph. Without returns at the end of l. 9 and l.15.

The text was changed accordingly

p. 6, l.13: why () around "weighted"?

The parentheses were taken out in the text

p.7 ll. 15-19: add references everywhere - or just make a link to the tables.

References were added to the other groups of studies as well

p.7. l. 49: It isn't really clear what is meant by 'the strength of the reformulation'.

“the strength of the reformulation” was substituted with “the amounts of nutrient reformulated” both here (p.7, line 49) and later in the results (p.8 l. 25)

p.8. l 17. It's unclear what is meant by '% salt reformulated in the models'.

“% salt reformulated in the models” was substituted with “percentages of salt reductions in the models”

p. 8. L.44: The MI abbreviation isn't explained and I'm not sure all readers would understand

The full-text definition was added to the text that now reads: “Myocardial infarction (MI)”

p.9. ll. 19-23: this comparison is maybe rather for the discussion section.

We agree with the reviewer’s comment. However, the main objective of the review is to provide a picture of the results of model-based studies on reformulation and to assess their quality, based on a specific quality assessment tool. Comparison of specific models’ results with other studies is considered out of scope and would not fit in the discussion. Therefore, we simply eliminated this comparison from the text.

p. 9. L.44: is the lowering for all types of fats?

No, just trans-fat. This was specified in the text that now reads:

“Temme et al., estimated that a broad intervention cutting TFA on a set of products would lower trans-fat consumption by 0.4 g/day (21.1%, UI=0.35, 0.45)”
P.10. ll.1-2: these absolute death reduction estimates do not mean much without context - what does it mean in terms of death rate reduction? Maybe the original study does not provide such estimates, which would a limitation to point out on these modelling works. Absolute estimates are good, but they should also be compared to some reference scenario.

Overall percentage reduction in deaths is reported in the original study (3%) and in this manuscript (P.10. ll.1-2 of the original submission). We modified the sentence to make this clearer:

“One single study estimated the effect of fat reformulation on mortality, showing that if all producers in the food industry complied with the International Choices Programme [55], SFA consumption would reduce by 15%, which in turn would reduce deaths by 3% (1,339 deaths from cardiovascular diseases and 558 deaths from cancer)”

p. 10. L.12: 7% for transparency and provision of technical documentation… I guess that's a typo?!

Apologizes for this typo. The correct percentage is 79%

p. 10 ll.21-23: Maybe you should re-explain what do you mean by relevance here. It's not really clear how you assessed this criterion, and most readers will not check the supplementary materials. It could also help making the link with the discussion section.

We agree with the reviewers’ comments about the confusion that may originate from this criterion. Here we mainly intended what in usually defined as the “face validity” of the model, i.e. whether models have been designed to appropriately consider all aspects of the decision problem, so that results are credible and realistic.

The sentence now reads as follows: “Lastly, only 45% of the studies scored high in face validity that is their results were considered credible and realistic (Figure 4). Particularly, studies were considered to have high face validity if they duly and credibly took into account all aspects of the decision problem, including the technical feasibility of the reformulation scenarios, and all the causal steps linking the intervention to the outcomes of interests (e.g. the reactions of the consumers and manufacturers to the intervention).

This aspect is reflected also in the revised paragraphs of the discussion section

p. 10. L 55. "might" affect policy results is maybe a better wording here?

The sentence now reads, “only two of the included studies explicitly modelled how consumer reactions might affect the policy effects”.

p. 11. Ll. 25-34. This paragraph could be linked to the 'liking' section. General limitations of the reviewed studies on parameters that are not taken into account.
The section on the technical feasibility of reformulation strategies was moved before the “liking section”. In addition the following introductory sentence was added: “The effectiveness of reformulation strategies on public health is the result of a complex causal chain that include technical/industrial aspects, marketplace dynamics and consumer reactions. However, a number of these aspects were not always addressed by the reviewed studies, hampering the reliability and credibility of the results.”

p.11. ll. 36-49. What would be your recommendation to ensure a better comparability between studies in the future, and more refined assessments of long-term impacts

An improved discussion section is proposed with more recommendations on the aspects outlined in the comments. With particular regard for these aspects, the following were added:

With regard to model comparability: “Methods for the evaluation of healthcare interventions have existed for several years [64, 68–72], but these have mainly been applied to more narrowly-defined ‘clinical’ interventions, such as drugs, devices and medical procedures [65]. In contrast, approaches in the field of nutrition and public health are not framed by common objectives, shared methods and/or a strong regulatory environment. Therefore, the establishment of an agreed framework specifying best modelling practices is needed to improve the methodological and reporting quality, as well as the comparability of studies evaluating public health interventions in general and nutrition interventions in particular.”

With regard to the assessment of long term impacts: “In addition, whenever possible, the simulated model outcomes should be confronted with real-world event data. External validation involves that the entire model or any of its components are verified by confronting the forecasted estimates with actual event data. For example epidemiological studies or trial data could be used to verify the correctness of the simulated incidence of non-communicable diseases in the absence of reformulation. In addition, the model should prove to be able to correctly predict future outcomes for the specific setting, population and intervention of interest. However, assessing the predictive ability of a model for medium and long term effects such as health and quality of life is challenging, as it would require longitudinal data with a rigorous counterfactual scenario [cit]. While this type of evidence is often missing, partial validation could be achieved by assessing the goodness of models in predicting intermediary effects such as intakes or surrogate health outcomes (e.g. hypertension)

p.12. ll. 1-2: This sentence comes a bit out of the blues; and it took me a while to understand what you meant. There is the need for a deeper discussion on the assessment of public health nutrition strategies, of which reformulation is one aspect.

In the modified discussion section, this sentence was moved after the first paragraph of the discussion and now reads: “In addition, more evidence is required on the relative effectiveness and cost-effectiveness of reformulation compared to alternative public health nutrition interventions (e.g. food taxes, public media campaigns or food labelling).”

p.12 ll. 15-28: Again, what would be your recommendations, especially considering the wider context mentioned just above?
Please refer to our previous comment and revised part in the discussion on “the need for an agreed framework specifying best modelling practices” to improve model comparability and transparency.

Tables 1-3: Some editing is clearly needed

- An extra row of table headings would help the reader understand what are inputs (methods) and outputs (results) of the reviewed studies.

We agree with the reviewer. We added an extra row dividing between “study characteristics” and “study outcomes”

- There is a need to add abbreviations in footnotes of each table. E.g. QOL, AMI, etc. In general, there may be the need to better explain some columns/results in the footnotes.

Abbreviations were added at the bottom of each table

- Why are some cells blank, and some cells "not reported"?

The cells with no value are now all blank

- Either use use symbol or text to express reductions or increases, but not a mix of the two

Whenever possible Symbols have been used consistently across the tables

- Could you add the information on the time horizon? As it is a quite important topic of your analysis (I would try to capture the same elements in the tables as in fig 2)

The information on time horizon and model were added to the tables in order to be consistent with info in fig 2

Table 3: Some missing headers.

Headers are correct now

Figure legends:

- Legend of Figure 2 is missing - numbers are mixed up. To revise

We thanks the reviewer for outlining this oversight. Legends are now correct

Prisma checklist: why is the risk of bias not applicable to your review? Some of these studies were funded by the food industry - they would have an interest in showing the beneficial effects
of product reformulation. (And you have already listed the information in the supplementary file 4).

In completing the checklist, we used as guidance the “PRISMA Statement for Reporting Systematic Reviews and Meta-Analyses of Studies That Evaluate Health Care Interventions: Explanation and Elaboration”.

We did assess the risk of bias within individual studies (items 12 and 19 in the checklist). According to the PRISMA guidance these items refer mainly to the risk that certain methodological characteristics may be associated with effect sizes (e.g. for example, trials without reported adequate allocation concealment). For modelling studies we assumed that the risk of bias coincides with the different methodological aspects that were evaluated during the quality assessment of each study.

Therefore, rather than directly reporting the source of funding as a potential risk factor for biased results we preferred to assess the transparency and methodological reliability of each study. This is in line with PRISMA guidelines where funding sources are never mentioned as a direct element against which to assess the risk of bias.

As to assessing the risk of bias across studies (items 15 and 22 in the checklist) we do not feel that the problem with selective reporting of study outcomes can be applied to modelling studies (compared to clinical studies where the difference between planned and reported outcomes may conceal unwarranted effects of the clinical treatment). Nor we feel that publication bias is an issue here, being the assessments of model transparency and face validity of much greater concern to guarantee plausible (“unbiased”) results.

Quality assessment tool:

- For point 5 "outcomes", I would maybe rephrase to health outcomes or something similar. As according to your assessment a study may receive a bad score even though it describes very clearly the 'outcomes' it considered.

We changed “outcomes” with “relevant outcomes” since this criterion assesses whether the outcomes reported in the studies are relevant to the decision problem to adopt/not adopt a reformulation intervention

- Point 9. Relevance. I still do not understand what is measured here - check my comment earlier.

Please see our previous comment. Relevance is now termed “face validity” and has been moved closer to the internal/external validation criterion. More detailed example and supporting questions have been added in the quality assessment tool and in the main manuscript as suggested in your previous comment.
Reviewer 2 (Reviewer 2): PEER REVIEWER COMMENTS: To view the full report from the academic peer reviewer, please see the attached file.

REVIEWER COMMENTS FROM REPORT: GENERAL COMMENTS

* What is your overall impression of the study?

This is a fairly well-written systematic review of mainly modelling studies on the impact of food reformulation on nutrient intakes and health. This study utilised a systematic analysis approach to investigate the effect of food reformulation on nutrient intakes and health and adds to the literature in this less-studied area of research.

* What the authors' have done well?

The Introduction and Methods Sections of the paper are well-written, succinct and coherent. The authors reviewed the literature well, provided adequate justification for the study and had a clear study objective. The study design and data analysis is clear and detailed. The reporting of their study findings is relatively good particularly as the authors' attempts to reflect the study objectives. The Tables and Figures are informative and clear. The Conclusion was concise and coherent and the authors' clearly showed the implications of their study as well as identified future research needs.

* In what ways does it not meet best practice?

I have some concerns with the Results and Discussion Sections of the manuscript. In the Discussion Section- many statements were made requiring citation. There is also a need for the authors to discuss the implications of their studies for the control of Non-communicable diseases and for different regions of the world where there is clear variation in the consumption of these nutrients.

I have identified some revisions which will help the authors improve their manuscript.

We thanks the reviewer for the thoughtful and relevant outcomes. We feel that answering to the issues raised has improved the quality of the study considerably. Particularly, the discussion section has been revised broadly. Please see below for our replies to your specific comments.

REQUESTED REVISIONS:

SPECIFIC COMMENTS

INTRODUCTION

"the majority of published studies use mathematical models to predict the effects of reformulation on intakes and clinical outcomes."
Rather than directly citing model-based studies on reformulation (the identification of which is one of the outcomes of the present review), a previous review from WHO has been cited. This previous work reports a brief overview of reformulation studies and includes among its key message that “Much of the evidence on the long-term costs and benefits of [public health] interventions has been estimated using simulation modelling approaches synthesizing data on effectiveness, epidemiology and costs”.


METHODS

"Science Direct"

Comment: Please, this should read: "ScienceDirect"

Thank you for outlining this mistake. ScindeDirect was used rather than Science Direct

"Therefore, a self-developed evaluation tool was used, mainly drawing from relevant criteria recommended by the ISPOR Good Practice Task Force”

Comment: Please, define ISPOR

The full length name for ISPOR has been added at first occurrence

RESULTS

"Tables 1-3 and Figure 2 summarize the main findings and characteristics of the included studies. Overall, studies were heterogeneous in the way interventions and outcomes were modelled and reported, limiting between-study comparisons. Other study characteristics are provided in the Additional file 4."

Comment: There is a need to better describe the studies included in the systematic review.

In total how many studies were included?

The number of included studies is provided in the first paragraph of the result section and in the flow diagram reported in Figure 1 together with the reasons of inclusion/exclusion.

What types of studies were they?
The main features of the studies, including the type and characteristics of the models (epidemiological models, mathematical statistical models or markov models, time horizon used, sensitivity analyses etc.), and the types of interventions (targeted nutrient, amounts reformulated, voluntary/mandatory nature etc.), are reported in the rest of the results section. We have slightly changed the structure of the first paragraph to make this clearer.

What regions of the world did these studies cover or model? I believe this will better strengthen the first paragraph of the Results Section.

Geographical information is provided in the additional file 4. Since geographical coverage is not within the main objectives of the present review, we would rather leave the information there without commenting it in the main text, to avoid excess of information and improve readability and comprehension.

In the discussion section, we added as limitation that: “Particularly, in the present work, we did not assess existing sources of variations in the effectiveness of reformulation and other nutrition interventions between and within countries, including biological, cultural, socio-economic and institutional factors. Future work may explore the contribution of these aspects on each link of the causal chain from intervention to public health outcomes. Explaining cross-country variations may help to understand which factors are favouring or hindering the effectiveness of nutrition policies at the global level, and their role in reducing the burden of non-communicable diseases. Explaining within-country variations may contribute to incorporate equity considerations in public decision making about nutrition policies at the national level.”

"... in a reduction between 6.35 and 1,452 DALYs [41, 42, 51], which, again, was proportional to the estimated levels of sodium reductions in each study."

Comment: Please, how did the authors assess how the QALY gained or DALY averted was proportional to the estimated levels of sodium reductions in each study? Please, clarify.

Similarly to the association between amounts reformulated and intake, here we are reporting again a certain degree of consistency across studies between expected sodium reductions and QALYs gained. The sentence was modified as follows:

“Although based on less studies, results seem to be consistent across models, showing a positive association between sodium reductions and QALYs (DALYs) gained (lost)”."

"Reformulation to reduce sugar intake targeted SSBs alone"

Comment: Please, define abbreviations when first use. What are SSBs, SFA or TFA?

SSBs stands for Sugar Sweetened Beverages. The acronym was added to the list of abbreviations and reported full-text at first occurrence in the main text. SFA and TFA are saturated and trans-fat lipids. Their full-text name is already reported at first occurrence in the text (Methods section, selection of studies) and in the list of abbreviations.
"Reformulation can alter the sensory attributes of food products and influence consumer liking. This in turn may trigger unattended behaviors including the consumption of more public health sensitive nutrients, or simply more calories."

Comment': Please can you provide a reference/s for these statements?

The following references were added:


"Almost 40% of the studies did not explicitly model the effects of the interventions over time and focused on simulating how cross-sectional nutrient intake data would change if foods were reformulated. Although consistent with their declared research questions, these studies are poorly informative about how the modelled interventions will dynamically affect intake and ultimately health outcomes. In the remaining studies, time horizons were highly heterogeneous, varying from 5 years to a life-time perspective. This again limits study comparability, with studies with shorter time horizons potentially underestimating the policies effect on health outcomes"

Comment': Please, provide a reference/s for these statements?

The discussion section has been revised quite extensively based on the valuable comments of all reviewers. In the paragraphs on time horizons, the following sentence was added to provide context information on existing literature:

“The dynamic nature of public health interventions and the presence of complex, interdependent factors have been already pointed out in the literature [64–66] calling for more methodological developments such as the use of system dynamics modelling to incorporate time dependencies [67]”

In addition, the following references were added:


"Overall, there is enough evidence on the existence of different effective strategies to improve population diet quality [60]. However, to allow a full comparison among possible alternatives, more studies comparing reformulation policies to larger sets of other interventions (e.g. food taxes, or "sinking lid" to target nutrients) are needed"

Comment: As this statement was not based on any meta-analysis and the studies included are highly variable, I suggest that the first part of the sentence needs to be revised thus:

"Overall, there is moderate evidence on the existence of different effective strategies to improve population diet quality"

Based on this and other comments this sentence was moved earlier on in the discussion section and modified as follows:

“In addition, more evidence is required on the relative effectiveness and cost-effectiveness of reformulation compared to alternative public health nutrition interventions (e.g. food taxes, public media campaigns or food labelling).”

Comment: There is also a need for the authors to discuss the implications of their studies for the control of non-communicable diseases and for different regions of the world where there are clear variations in the consumption of these nutrients.

The following paragraph was added to the discussion section among the limitations of the study:

“Particularly, in this study, we did not assess existing sources of variations in the effectiveness of reformulation and other nutrition interventions between and within countries, including biological, cultural, socio-economic and institutional factors. Future work may explore the contribution of these aspects on each link of the causal chain from intervention to public health outcomes. Explaining cross-country variations may help to understand which factors are favoring or hindering the effectiveness of nutrition policies at the global level, and their role in reducing the burden of non-communicable diseases. Explaining within-country variations may contribute to incorporate equity considerations in public decision making about nutrition policies at the national level.”
"To the authors' best knowledge, this is the first review to report a detailed description of modelling studies focusing on the reformulation of food products. It is also the first study to assess the study quality based on an ad-hoc assessment tool."

Comment: Please, it is generally safer to avoid these statements as a group has performed studies covering most of the objectives and data sources used for this study. Please see:


In addition, there was no external validation of the ad-hoc assessment tool.

The statements was changes into:

“This review reported the potential effects of reformulation policies on intake and health and proposed an ad hoc tool to assess the quality of modelling studies on reformulation.”

ADDITIONAL REQUESTS/SUGGESTIONS:

As stated in the commentary above, please.

Temme Elisabeth H. M. (Reviewer 3): NUTN-D-18-00053

Review of The impact of food reformulation on nutrient intakes and health. A systematic review of modelling studies

In this paper the authors aim explore the impact of reformulation on nutrient intakes, health outcomes and quality of life; and to evaluate the quality of modelling studies on reformulation interventions. Reformulation of processed foods has the potential to improve population diet, but evidence of its impact is limited.

These sentences include the main critique I have with the study. Many modelling studies have been carried out, most of them modelling potential effects on nutrient intake and selected health outcomes. In most papers, I think, rather unrealistic scenarios are studied. The realisticness of the scenarios must be checked from the perspectives of how likely is it that foods are changed, sold and eaten in the way is assumed in the models? How far are the modelled situations form the current food consumption patterns? To what level is this taken into account? Now many studies describe potential effects, but what are the actual (to be expected) effects?

We thanks the reviewer for the useful comments. The realisticness of modelled scenarios was our main concern as well. We tried to make this clearer throughout the whole manuscript and in the supplementary materials. Precise answers to your comments are provided below.
Abstract:

Line 19: Evidence on reformulation policies was strong for sodium: what do you mean? Effective policies? Are the modelling studies looking at policies?

The sentence was changed as follows:

“Evidence on the positive effects of reformulation on consumption and health was stronger for sodium interventions, less conclusive for sugar and fats.”

Non appropriate time-horizon: It is not clear to me what this means?

Here we meant that time-horizons in the models were too short for the models to incorporate all relevant health effects of reformulation policies, as this is usually a common recommendation for decision-analytical models assessing health interventions (at least in a “clinical” setting).

The statement was reworded as follows:

“Study quality was often compromised by short time-horizons, disregard of uncertainty and time dependencies, and lack of model validation.”

More on time-horizon and time dependencies issues in the models has also been reported in the revised discussion section.

Background

Page 3: Line 49: scheduled .. and reached??

The sentence was modified as follows: “the pace of implementation, including how reduction targets are set, scheduled and reached.”

We understand and agree with the reviewer’s concerns about the realisticness of these models. This was reflected in our attempt of extracting information on how the models addressed critical issues such as realistic reformulation targets, as well as industry and consumer behaviors. Our view is that, given that it is particularly challenging to predict such behaviors before the actual policy implementation, models should first consider these parameters explicitly and secondly they should correctly incorporate the uncertainty in these parameters by doing extensive sensitivity analysis.

This was discussed more thoroughly in different paragraphs of the discussion section and incorporated in the quality assessment tool.
Page 4 and line 9: Extreme scenarios/models are used that model the potential effects of full implementation of measures… is this realistic?

Please see our reply to your previous comment.

Line 13: limiting as well realismness of outcomes for population health.

The sentence now reads: “Nonetheless, the extreme flexibility of modelling and the required assumptions needed to simplify complex nutrition interventions may introduce a considerable variability, thus limiting between-study comparability and even challenging the plausibility of models results for population health.”

Quality assessment of studies

Include criteria (and results of this assessment) on realismness of the scenarios studied and how much change is needed from the current food consumption patterns.

We rename the “relevance” criterion in the quality assessment tool that now is defined as “face validity”. According to Caro et al 2014, “Face validity of the model addresses how plausibly the model represents the diseases, settings, populations, interventions, and outcomes it is intended to analyse”. The description of the specific elements that affect face validity was expanded in the discussion section and recommendations on specific aspects for reformulation interventions were proposed.

Page 7, line 57-58. You distinguish between voluntary and mandatory, level of implementations is maybe a better word to describe what you mean.

We understand that the level of implementation is the key parameter affecting the impact of any reformulation intervention and as such it was extracted and reported in the analysis. However, we would prefer to keep the mandatory/voluntary distinction. This preference is justified by the fact that whether to adopt a stronger, regulatory (mandatory) approach rather than a more collaborative (voluntary) one is an important policy option, and therefore the estimated differences between the two scenarios may be of interest to policy makers aiming to design and implement a reformulation initiative. In addition, this terminology is used by many of the studies included in the review, including, but not limited to, Wilson et al (2016), Gillespie et al (2015), Collins et al (2014), Cobiac et al (2010) and Smith-Spangler et al (2010).

Here we assumed that the interventions where all products consumed or marketed are reformulated are comparable to mandatory interventions, as no voluntary policy would achieve this degree of pervasiveness across manufacturers. This was specified better in the methods section: “Even if not explicitly stated, interventions where all products consumed/marketed are reformulated were assumed to be likewise mandatory interventions, as it was considered that no voluntary policy would achieve such degree of pervasiveness across manufacturers.”
Page 8, line 12. Additionally including actual market shares in the models e.g. for foods with certain reformulation levels is another way of modelling more realistic situations like: Temme et al, 2010, PHN, Public Health Nutrition: 14(4), 635-644

We thank the reviewer for suggesting this relevant study. However, rather than citing the suggested paper here in the results section (where only the results of the included studies are reported), we cited it in the discussion section when addressing the challenges to anticipate consumer and manufacturer reactions to reformulation and the need for explicit, transparent and realistic assumptions on these parameters. Please see our reply to your further comment below.

Line 24, sometimes you refer to salt and sometimes to sodium, did you do re-calculations? Please use one of the two.

Yes, in the tables we re-calculated all results to sodium reductions. We added a line explaining this in the methods:

“In addition, salt amounts were converted to sodium amounts using a 1g/400mg conversion rate.”

Also, the word “sodium” was used consistently throughout the manuscript, expect when referring to the use of “discretionary salt”.

Line 28 … targeted foods and scenarios studied

The statement was modified accordingly

Page 9, line 45: insert trans

Trans was inserted. The line now reads:

Temme et al., estimated that a broad intervention cutting TFA on a set of products would lower trans-fat consumption by 0.4 g/day (21.1%, UI=0.35, 0.45) (Table 4)

Line 47: delete: more and from the same research group

The text was changed accordingly

Line 58 % of what??

The line was modified as follows: “found percentage reductions in SFA consumption to be in a range between 1.1% and 40%”

Page 10, line 1 reducing SF by 15% by what type of scenario?

In this study, % reductions for each food category are specifically calculated taking as benchmark the Dutch Choices Programme. This reference was added to the text: “One single
study estimated the effect of fat reformulation on mortality, showing that if all producers in the food industry complied with the International Choices Programme [54], SFA consumption would reduce by 15%, which in turn would reduce deaths by 3% (1,339 deaths from cardiovascular diseases and 558 deaths from cancer) [32].

Discussion

Include some additional discussion on the quality and realism of the scenarios assessed and lessons that can be learned for the future.

The discussion section has been widely revised to incorporate aspects on quality and plausibility of model assumptions.

See point before on voluntary and mandatory, please reword

Please consider our previous comment and the reason why we would prefer to keep the voluntary/mandatory distinction

Page 11, line 2, would be smaller… this is also confirmed by study mentioned above using current market shares.

Thanks again for suggesting this paper. This has been cited in the revised version of the discussions as a potential source of real world data when discussing challenges of modelling consumer and manufacturers reactions:

“in some cases, real world sources can be used to model more realistic scenarios. A study by Temme et al assessing the impact on intake of foods reporting a health logo estimated the expected consumption rate of healthy and unhealthy products, by looking at real market shares of products with an healthy logo over the total purchases in each food category”

Line 41, these studies are poorly informative, I do not agree, this type of studies give other valuable information on a cross-sectional level.

Line 43 dynamically.. mostly these models contain information of the food consumption surveys, that were found poorly informative in line 41.

The paragraph on time-horizon and time dependencies has been broadly revised and now reads:

“The time horizon considered in reformulation models should be long enough to account for all relevant consequences of the interventions, including the long-term health effects of improved dietary patterns. In addition, many aspects of the reformulation models can vary over time including the industry uptake of reformulated products, consumption habits and preferences, and secular epidemiological trends in non-communicable diseases. In this review, 40% of the studies did not explicitly model the effects of the interventions over time and focused on simulating how cross-sectional nutrient intake data would change if foods were reformulated. Although
consistent with their declared research questions, these studies do not estimate how the modelled interventions will dynamically affect food purchases, intake, and ultimately health outcomes. The dynamic nature of public health interventions and the presence of complex, interdependent factors have been already pointed out in the literature [64–66] calling for more methodological developments such as the use of system dynamics modelling to incorporate time dependencies [67].

Line 58: what do you mean? By external validation?

The paragraph on time-horizon and time dependencies has been broadly revised and now reads: “Besides face validity, other standard steps of validation applies to reformulation models [21]. Internal validity should be verified by demonstrating that the model behaves as intended and has been implemented correctly. In addition, whenever possible, the simulated model outcomes should be confronted with real-world event data. External validation involves that the entire model or any of its components are verified by confronting the forecasted estimates with actual event data. For example epidemiological studies or trial data could be used to verify the correctness of the simulated incidence of non-communicable diseases in the absence of reformulation. In addition, the model should prove to be able to correctly predict future outcomes for the specific setting, population and intervention of interest. However, assessing the predictive ability of a model for medium and long term effects such as health and quality of life is challenging, as it would require longitudinal data with a rigorous counterfactual scenario. While this type of evidence is often missing, partial validation could be achieved by assessing the goodness of models in predicting intermediary effects such as intakes or surrogate health outcomes (e.g. hypertension).”

Page 12, line 4 sinking lid?

The sinking lid policy was found in studies comparing alternative nutrition policies (e.g. Nghiem 2015) meaning by that a policy where “The amount of food-grade salt released onto the market is reduced annually to the point where the recommended level of sodium intake is achieved”. To avoid confusion, we modified the example list in the test to report most commonly known food policies: “(e.g. food taxes, public media campaigns or food labelling)”

Please also note that in the revised description, this paragraph has been moved earlier in the discussion.

Line 11, this study reported potential effects .of... on intake and health..

The text was modified accordingly and now reads: “This review reported the potential effects of reformulation policies on intake and health and proposed an ad hoc tool to assess the quality of modelling studies on reformulation.”

Tables can be made much clearer with respect to lay-out and content, see points raised above.

Based on your comments and the comments from other reviewers, tables were modified to improve clarity and readability.