PEER REVIEW HISTORY

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This paper was submitted to the BMJ but declined for publication following peer review. The authors addressed the reviewers' comments and submitted the revised paper to BMJ Open. The paper was subsequently accepted for publication at BMJ Open.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Health effects of adopting low greenhouse gas emission diets in the UK
AUTHORS	Milner, James; Green, Rosemary; Dangour, Alan; Haines, Andrew; Chalabi, Zaid; Spadaro, Joseph; Markandya, Anil; Wilkinson, Paul

VERSION 1 - REVIEW

REVIEWER	Scarborough, Peter University of Oxford, Department of Public Health
REVIEW RETURNED	20-May-2014

GENERAL COMMENTS	This is an excellent paper that I really enjoyed reading. The methods that the authors employed were novel and produce some fascinating scenario results. In particular, the use of time lag curves in the life table modelling and the use of quadratic (or non-linear) programming to identify optimised diets under various constraints are valuable contributions to the scientific field. This is the most robust modelled paper that I have yet come across that has addressed the question of whether healthy diets are sustainable and vice versa. I have a few comments that the authors may wish to consider. 1. On a number of occasions the authors describe their method of deriving scenario diets as a strength as it incorporates palatability. This is an advantage over standard linear programming. However, this does not necessarily ensure that the diet scenarios are acceptable for consumption (indeed, it is a common limitation of optimisation modelling that the resultant diet scenarios may not be acceptable for consumption in the public). For example, the diet scenario with no GHG constraints reduces egg consumption by about 75% and cheese consumption by about 85%. It would be good to include as a limitation that the diets used in this process are not actually observed in any population. 2. The web appendix mentions that separate life tables were created for each outcome. Does this mean that the results are not adjusted for competing risks by using a multiple decrement life table approach? 3. The Dauchet meta-analysis for the effect of fruit and vegetables on CHD states the following in the abstract: "The association between vegetable intake and CHD risk was heterogeneous (P =

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	0.0043), more marked for cardiovascular mortality [0.74 (0.75-0.84), P < 0.0001] than for fatal and nonfatal myocardial infarction [0.95 (0.92-0.99), P = 0.0058]. Visual inspection of the funnel plot suggested a publication bias, although not statistically significant. Therefore, the reported RRs are probably overestimated. " Given the concerns over the RR used for the link between vegetable consumption and CHD, did the authors consider doing a further sensitivity analysis where the individual RRs for fruit and vegetables were replaced with the single RR for fruit and vegetables combined (which is more conservative than the two individual estimates)? 4. The second sensitivity analysis allows for an estimate of the range of plausible results, but is an overestimate of uncertainty as it is unrealistic that all RRs would be at either the bottom or top of their reported CIs at the same time. Why did the authors not conduct a Monte Carlo-style uncertainty analysis, allowing the RRs to vary according to their reported distribution? 5. Page 17: "We also did not take into account relationships between saturated and unsaturated fat consumption and coronary heart disease outcomes because of the need to assess GHG emissions on food groups." I don't understand this argument. 6. Page 17: "Ultimately our estimates are likely to be conservative" I don't agree that these estimates are likely to be conservative, mainly because the scenarios do not take account of currently reducing rates of CVD. I do not think that affects the quality of the research (ultimately these are scenario analyses rather than forecasts), but I would delete this line.

REVIEWER	Simon Capewell University of Liverpool
REVIEW RETURNED	22-May-2014

GENERAL COMMENTS	Thank you for the opportunity to review this interesting manuscript "Health effects of adopting low greenhouse gas emission diets in the UK: modelling study" by Milner and colleagues. I have no major concerns. The manuscript might benefit from a bit of polishing. Specifically ABSTRACT. The Methods (DESIGN, INTERVENTION) need a bit more detail WHAT THIS STUDY ADDS A teeny bit more info on what how big a "relatively modest change" might be. INTRODUCTION Basically OK. Need to use the recent Lancet GBD paper to make the point that poor diet generates more NCD disability and death than tobacco plus alcohol plus inactivity. and then pick this up in the Discussion: they may have focussed on quantifying reductions in CHD, but much wider benefits might be

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	reasonably be expected. The analysis makes appropriate use of the most recent data and meta-analyses. The technical details in the Supplementary Material are sufficient for understanding and replication by someone with the technical background. There does not appear to be a substantial area that needs improvement. The observations below are relatively minor and can be clarified without additional analysis. METHODS some clarification might help: 1. Are GHG reduction percentages based only on emissions from the food cycle? Or, are they total GHG emissions? If only food cycle, it would be good to mention what percent of total GHG emissions are related to the food cycle (if possible). 2. It is not clear how palatability was assessed / defined. Is it an explicit constraint in the optimization? Or is it approximated by the elasticity in the objective function? RESULTS Figure 1: The trends for Stroke, Cancers, and Diabetes are hard to discern. It might be easier to see full details with y-axis on log scale. It might also be worth plotting an additional figure with only the cancers. Depending on the outcomes of these 2 suggestions, Figure 2 may no longer be necessary (e.g., maxima may be evident for each disease category). DISCUSSION. Signalling a greater degree of caution would be good, because this is just a modelling study. For instance: "Our model suggests that such changes to UK diets would save almost 7 million YLL over 30 years" might be better phrased as "Our model suggests that such changes to UK diets MIGHT save almost 7 million YLL over 30 years" and the same advice applies to the rest of the text, please. bottom of page 18: “estimates are likely to be conservative”. Disagree, because a constant counterfactual was used. Due to the decreasing mortality trends as mentioned in the same paragraph, in the future there will likely be diminishing returns. Absent additional analysis, it is not clear whether the results are overestimates (due to assumption of constant mortality) or underestimates (due to other benefits that were not modelled). See for example “Future declines of Coronary Heart Disease mortality in England and Wales could counter the burden of population ageing” (Guzman Castillo et al., to appear in PLOS One). LIMITATIONS. "The limitations of the study relate primarily to inadequacies of the available input data." disagree, would prefer you to say "The limitations of the study relate primarily to inadequacies of the available input data, and, to the assumptions (as with all modelling analyses)

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	Perhaps comment on the result (Tables S3 and S4) that Tea consumption would more than double. This is curious. Presumably tap water (not mineral water) would be a lower-GHG substitute for other liquids. Is this result due to data being unavailable on tap water consumption? It might be good to expand under the heading "Optimized diets" in the appendix, including any other surprising results. Very Minor points: 1. Reference 15 in the Technical Appendix is not complete. It reads only "Marmot et al. (2007)".

VERSION 1 – AUTHOR RESPONSE

Reviewer 1 comments

This is an excellent paper that I really enjoyed reading. The methods that the authors employed were novel and produce some fascinating scenario results. In particular, the use of time lag curves in the life table modelling and the use of quadratic (or non-linear) programming to identify optimised diets under various constraints are valuable contributions to the scientific field. This is the most robust modelled paper that I have yet come across that has addressed the question of whether healthy diets are sustainable and vice versa.

I have a few comments that the authors may wish to consider.

1. On a number of occasions the authors describe their method of deriving scenario diets as a strength as it incorporates palatability. This is an advantage over standard linear programming. However, this does not necessarily ensure that the diet scenarios are acceptable for consumption (indeed, it is a common limitation of optimisation modelling that the resultant diet scenarios may not be acceptable for consumption in the public). For example, the diet scenario with no GHG constraints reduces egg consumption by about 75% and cheese consumption by about 85%. It would be good to include as a limitation that the diets used in this process are not actually observed in any population.

Response:
We agree with the reviewer that the optimized diets are not genuine observed ones. This will always be a limitation of modelling but we have attempted to tackle this more robustly than in previous studies. This point has been added to the section on limitations (page 16).

2. The web appendix mentions that separate life tables were created for each outcome. Does this mean that the results are not adjusted for competing risks by using a multiple decrement life table approach?

Response:
Although a single (multiple decrement) life table could have been used to estimate the total health impact, our preference was for the different outcomes to remain separate since they have different levels of impact and patterns over time. On the other hand, a multiple decrement life table would not give changes in life years separate for <u>each</u> outcome. As such, we have made no change to the manuscript here.

3. The Dauchet meta-analysis for the effect of fruit and vegetables on CHD states the following in the abstract: "The association between vegetable intake and CHD risk was heterogeneous (P = 0.0043), more marked for cardiovascular mortality [0.74 (0.75-0.84), P < 0.0001] than for fatal and nonfatal myocardial infarction [0.95 (0.92-0.99), P = 0.0058]. Visual inspection of the funnel plot suggested a

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publication bias, although not statistically significant. Therefore, the reported RRs are probably overestimated. "Given the concerns over the RR used for the link between vegetable consumption and CHD, did the authors consider doing a further sensitivity analysis where the individual RRs for fruit and vegetables were replaced with the single RR for fruit and vegetables combined (which is more conservative than the two individual estimates)?

Response:
We do not expect this to fundamentally change the results or the main messages of the paper. As such, we have made no change to the text.

4. The second sensitivity analysis allows for an estimate of the range of plausible results, but is an overestimate of uncertainty as it is unrealistic that all RRs would be at either the bottom or top of their reported CIs at the same time. Why did the authors not conduct a Monte Carlo-style uncertainty analysis, allowing the RRs to vary according to their reported distribution?

Response:
A Monte Carlo uncertainty analysis was not feasible for this work. First, the optimizations were each run multiple (100) times to maximise the chance of finding the global optimum solution in each case. It was not possible to repeat the process for probabilistic sensitivity analysis due to the required computation time. Second, any probabilistic sensitivity analysis would need to be performed in two parts – first for the optimized diets, then for the health model (since the two components are separate). Though this may be possible, it would be extremely computationally intensive. As such, we performed a more limited analysis to estimate upper and lower bounds.

5. Page 17: "We also did not take into account relationships between saturated and unsaturated fat consumption and coronary heart disease outcomes because of the need to assess GHG emissions on food groups." I don't understand this argument.

Response:
The sentence was included to explain why we had not modelled the well established relationship saturated and unsaturated fat and coronary heart disease. We did not model this relationship because it would be highly likely to be confounded by the (modelled) relationship between meat consumption and CHD. We have rephrased the sentence to make this point more clearly (page 17).

6. Page 17: "Ultimately our estimates are likely to be conservative" I don't agree that these estimates are likely to be conservative, mainly because the scenarios do not take account of currently reducing rates of CVD. I do not think that affects the quality of the research (ultimately these are scenario analyses rather than forecasts), but I would delete this line.

Response:
We have removed the reference in the sentence to "conservative" estimates (page 17) and have also referred to the fact that we did not account for potential future reductions in underlying CVD rates. (The mid to long trends in CVD are unclear as a result of increasing obesity.)

Reviewer 2 comments

Thank you for the opportunity to review this interesting manuscript "Health effects of adopting low greenhouse gas emission diets in the UK: modelling study" by Milner and colleagues.

I have no major concerns.

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The manuscript might benefit from a bit of polishing.

Specifically

ABSTRACT.

The Methods (DESIGN, INTERVENTION) need a bit more detail

Response:
The abstract was restructured for submission to Journal of Epidemiology and Community Health.

WHAT THIS STUDY ADDS

A teeny bit more info on what how big a "relatively modest change" might be.

Response:
The 'What this study adds' section was removed to meet the requirements for Journal of Epidemiology and Community Health.

INTRODUCTION

Basically OK.

Need to use the recent Lancet GBD paper to make the point that poor diet generates more NCD disability and death than tobacco plus alcohol plus inactivity.

and then pick this up in the Discussion: they may have focussed on quantifying reductions in CHD, but much wider benefits might be reasonably be expected.

The analysis makes appropriate use of the most recent data and meta-analyses. The technical details in the Supplementary Material are sufficient for understanding and replication by someone with the technical background. There does not appear to be a substantial area that needs improvement. The observations below are relatively minor and can be clarified without additional analysis.

Response:
We have added a reference to the most recent GBD results in the Introduction (page 4).

METHODS

some clarification might help:

1. Are GHG reduction percentages based only on emissions from the food cycle? Or, are they total GHG emissions? If only food cycle, it would be good to mention what percent of total GHG emissions are related to the food cycle (if possible).

Response:
The emissions reductions are only those associated with the diet, which was the focus of the research. We have made no change to the text.

2. It is not clear how palatability was assessed / defined. Is it an explicit constraint in the optimization? Or is it approximated by the elasticity in the objective function?

Response:
See above. We have replaced the term "palatability" throughout the text and made clearer that the likely acceptability of the new diets is accounted for implicitly in the method.

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RESULTS

Figure 1: The trends for Stroke, Cancers, and Diabetes are hard to discern. It might be easier to see full details with y-axis on log scale. It might also be worth plotting an additional figure with only the cancers. Depending on the outcomes of these 2 suggestions, Figure 2 may no longer be necessary (e.g., maxima may be evident for each disease category).

Response:
We tried to alter the plots to reflect the reviewer's suggestion. However, we felt the new plots were less clear and therefore made no change to the figures.

DISCUSSION.

Signalling a greater degree of caution would be good, because this is just a modelling study.

For instance:

"Our model suggests that such changes to UK diets would save almost 7 million YLL over 30 years"

might be better phrased as

"Our model suggests that such changes to UK diets MIGHT save almost 7 million YLL over 30 years"

and the same advice applies to the rest of the text, please.

Response:
We feel that the change suggested by the reviewer is overly cautious. We used the phrase "Our model suggests" to make clear that these are modelling results.

bottom of page 18: "estimates are likely to be conservative". Disagree, because a constant counterfactual was used. Due to the decreasing mortality trends as mentioned in the same paragraph, in the future there will likely be diminishing returns. Absent additional analysis, it is not clear whether the results are overestimates (due to assumption of constant mortality) or underestimates (due to other benefits that were not modelled). See for example "Future declines of Coronary Heart Disease mortality in England and Wales could counter the burden of population ageing" (Guzman Castillo et al., to appear in PLOS One).

Response:
See above. This part of the sentence has been removed (page 17).

LIMITATIONS.

"The limitations of the study relate primarily to inadequacies of the available input data."

disagree,

would prefer you to say

"The limitations of the study relate primarily to inadequacies of the available input data, and, to the assumptions (as with all modelling analyses)

Response:
We have altered the sentence to reflect the reviewer's comment (page 16).

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Perhaps comment on the result (Tables S3 and S4) that Tea consumption would more than double. This is curious. Presumably tap water (not mineral water) would be a lower-GHG substitute for other liquids. Is this result due to data being unavailable on tap water consumption? It might be good to expand under the heading “Optimized diets” in the appendix, including any other surprising results.

Response:
Tea has relatively low emissions and micronutrient levels. As such, the optimization process can potentially change the amount in the diet greatly with little penalty. The same is true for some of the other liquids. For this reason, we added a constraint preventing total liquids from more than doubling.

The reviewer is correct regarding the unavailability of detailed information on tap water consumption.

Very Minor points:

Reference 15 in the Technical Appendix is not complete. It reads only “Marmot et al. (2007)”.

Response:
The reference in the technical appendix has been updated.

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