Author’s response to reviews

Title: Evidence on the magnitude of the economic, health and population effects of palm cooking oil consumption: An integrated modelling approach with Thailand as a case study

Authors:

Marcus Keogh-Brown (marcus.keogh-brown@lshtm.ac.uk)
Henning Jensen (Henning.Tarp-Jensen@lshtm.ac.uk)
Sanjay Basu (basus@stanford.edu)
Wichai Aekplakorn (wichai.aek@mahidol.ac.th)
Soledad Cuevas (Soledad.Cuevas@lshtm.ac.uk)
Alan Dangour (alan.dangour@lshtm.ac.uk)
Shabbir Gheewala (shabbir_g@jgsee.kmutt.ac.th)
Rosemary Green (Rosemary.Green@LSHTM.ac.uk)
Edward Joy (Edward.Joy@lshtm.ac.uk)
Nipa Rojroongwasinkul (nipa.roj@mahidol.ac.th)
Nalitra Thaiprasert (nalitra@gmail.com)
Bhavani Shankar (b.shankar@soas.ac.uk)
Richard Smith (richard.smith@lshtm.ac.uk)

Version: 2 Date: 24 May 2019

Author’s response to reviews:

[Original Reviewer/Editor comments are in square brackets]

***Responses start and finish with double asterisk ***

[1. In response to reviewer’s query who they based their effects on a non-significant finding of changes in total C/HDL ratio after replacing palmitic acid, the rational given is that ‘it is more
conservative and robust’ as experimental as opposed to observational evidence (I looked up the ratio used from Mentink 2003 (table 2): it reports a mean change in total to HDL: ratio of 0.005 (-0.008 to 0.019))

**We thank the editor for these comments. As stated in our previous response, the total/HDL cholesterol evidence is, on balance, the most robust evidence available but we also acknowledge that the confidence interval in the Mensink paper includes zero. In light of these factors, the request of the reviewer to include sensitivity analyses on the Mensink indicators was very welcome and is, indeed, the best way, to utilise this robust evidence whilst accounting for the uncertainty in the point estimates of the parameters.

However, in view of the question 2 (below), we wonder if our previous response may have been misunderstood and therefore we hope that the following clarification is helpful in showing that we have addressed this issue in the way suggested by the reviewer.

We agree that it would not be sufficient to quote a separate paper that describes a Monte Carlo simulation of our model, and this is not what we have done. Instead, a previously published method has been employed to produce a Monte Carlo simulation which is specific to this paper and specific to this model. The Monte Carlo simulation uses independent draws of SFA, MUFA and PUFA parameters from normal distributions with prior means and standard deviations derived from Mensink et al. to produce 95% confidence intervals for our macroeconomic and Mensink model-based health outcomes. In doing so, we have sought to address the concerns raised by the reviewer in the way proposed by the reviewer. In order to clarify this further in the paper, we have adjusted the text from lines 308-15 to read

“Finally, we explore the impact of parameter uncertainty, related to the key serum cholesterol biomarker health pathway parameters from Mensink et. al. [4], by utilising a previously published Monte Carlo simulation methodology [65, 66] and adapting it for use in our Thailand CGE model in order to produce sensitivity analyses of our results. The procedure was based on 250 sets of independent draws of SFA, MUFA and PUFA parameters from normal distributions, with prior means and standard deviations derived from Mensink et al. The number of independent draws ensures that average point estimates, for all outcome measures, have a precision of <3.5 percentage points at the 95% confidence level.”**

[2. Compounding my first worry is the absence of uncertainty intervals. In response to this reviewer query, the authors state that their model has >50k equations and variables (another major red flag for any model....) and quote a separate paper that describes a Monte Carlo simulation of their model]

**With regard to the first comment concerning absence of uncertainty, as mentioned above, we believe this is a misunderstanding and we have, therefore, further clarified the text of the paper to
say that we are relying on a previously published methodology and that we are adapting it for our current purposes. Specifically, we undertake a sensitivity analysis on the key parameters of interest for our modelling scenarios. Other CGE studies have, for example, included additional sensitivity analyses on elasticities. However, since our nutrient-matching scenario does not make use of a policy instrument and does not induce substitution, variation of elasticities would not change our results.

Our model has a large number of parameters, but such is the case for any CGE model since the model is calibrated based on a social accounting matrix which includes values to represent all economic flows within an economy. The model is calibrated with real data and one of the key successes of our project was the obtaining of the very best quality national datasets available for all elements of our project. To explain:

- The data underlying this social accounting matrix are the official national accounts of Thailand and the Social Accounting Matrix was provided by the government’s statisticians

- Population values were provided by the National Statistical Organisation of Thailand and the Thailand Office of the National Economic and Social Development Board

- The Thai Health data was taken from the Thailand National Health Examination survey

- Dietary and nutrition data was taken from the Thai National Food Consumption Survey

- The Thailand Household Expenditure survey was also especially appropriate for our purposes since it was taken from the year that the FAO had provided additional support to enable more detail on food consumption to be collected than in any previous Thailand Household Expenditure survey

Therefore, whilst our model is based on a large amount of data, that data is of the highest possible quality for a CGE model applied in this context and, although CGE is not a stochastic methodology, we have introduced a statistical method for sensitivity analysis which is beyond that usually employed in models of this kind. Since the number of equations relates to a reviewer response, rather than the text of the paper, we have not addressed the issue of number of equations/variables in the text, but we would be happy to do so if this would be of help.

[3. The methods of modelling the health effects on MI and stroke are absent in main paper. In supplementary material there is a short write-up that largely quotes published studies]

**We thank the Editor for their suggestion. We have provided more information on the modelling of MI and stroke in the main paper from line 252 which now reads:
“The changes in the ratios of fatty acids consumed are translated to changes in total-to-HDL cholesterol ratios using parameters from the literature [4], and the changes in cholesterol biomarker values are, subsequently, used to compute changes in clinical health based on age- and gender-specific polynomials linking cholesterol biomarker build-up to MI- and stroke-related incidence and excess mortality rates. The health model, adapted for this purpose, has been previously published [6,48]. Furthermore, the clinical health lookup tables, underlying the age- and gender-specific clinical health polynomials, were derived from a simulated cohort of 10,000 individuals for whom the mean and standard deviation of total/HDL cholesterol ratio were taken from the Thai National Health Examination Survey.”

We have also clarified in lines 248-49 that we employ “…polynomial approximations to age- and gender-specific lookup tables derived from a micro-simulation health model to translate changes in nutritional exposure to clinical health outcomes.”**