Author's response to reviews

Title: Dietary habits in three Central and Eastern European countries: the HAPIEE study

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Author's response to reviews: see over
Re: Response to comments

Dear Editor,

Thank you for the comments on the manuscript MS: 5716663062899822 “Dietary habits in three Central and Eastern European countries: the HAPIEE study”. We were pleased by the positive and constructive comments made by both reviewers. We have revised the paper along the lines suggested by the referees (detailed response to their comments is below).

We hope that we have addressed all issues raised by the reviewers and editor, and we hope that you will find our revision acceptable.

Yours sincerely,

Sinead Boylan.

RESPONSE TO COMMENTS

In response to the comments, responses have been starred and all the alterations in the manuscript have been highlighted in red text.

Editors Comments
1. Please clarify ethical approval.

**Details have been added on ethical approval under the heading ‘Methods’ pg 6.
2. Informed consent must also be documented.

**Details on consent also added under the heading ‘Methods’ pg 6.

**Reviewers Comments

**Reviewer 1 (Vladimir Shkolnikov)

1. Although, the samples are drawn in a random manner from typical sites in each of the three countries, they are probably not nationally representative. At least, the samples do not include people living in rural areas and some of the sites (Novosibirsk) are geographically distant from places where most of the country’s population lives. It should be clearly stated whether the samples are nationally representative or not. If the latter is true, then throughout the whole paper the countries’ or peoples’ names (“Czech Republic”, “Poland”, “Russia”; “Czechs”, “Polish”, “Russians”) should not be used in a way as if one speaks about respective national populations. It would be correct to write instead: “Czech sample”, “Polish sample”, and “Russian sample”.

**We agree that we need to clarify that our samples were recruited from urban areas and we have now stated this under the Methods section (pg 6) and in the Discussion section (third paragraph, pg 15). Please note any references to whole countries’ or peoples’ names are now referred to as “subjects” or “sample”.

2. It would be good to acknowledge and briefly discuss certain socio-demographic characteristics and their potential impacts on the final results. It seems, for example that highly educated individuals are over-represented in the samples. What is the magnitude of this shift compared to the national and/or city populations and in which way this shift can influence the results. It also seems that Krakow and Novosibirsk are substantially bigger according to their population size than the Czech cities. Is that right and can this difference influence the results somehow?

** We agree with both points. First, there were, indeed, a high proportion of higher educated subjects in our Russian and Polish samples; and second, Krakow and Novosibirsk do differ in their size from the Czech towns.

With respect to higher education of these samples, we cannot claim that these samples are representative of their respective countries or, indeed, cities. Krakow and Novosibirsk are towns with large universities and research centres and the proportion of university educated people is thus higher in these towns than in the whole countries. It is also well known that response rates are usually higher in higher socioeconomic groups and in healthy persons. It is therefore likely that, assuming that healthy people and people with higher education have healthier diet, our estimates would show a more favourable picture than there is in reality. Although we did not find in our data a distinguishable gradient in
daily energy or macronutrient intake by education (not shown in the paper), we have added an additional comment to the discussion acknowledging the possible selection bias (third paragraph, pg 15).

With respect to the fact that Krakow and Novosibirsk are bigger according to their population size than the Czech cities, it is difficult to assess the biases. On one hand, the Czech sample, from six cities across the country, may be a more nationally representative sample than in the other two countries. On the other hand, diet may indeed differ depending on the size of city. While our impression (anecdotal evidence) is that these middle-sized towns would not differ dramatically from Prague (the only city comparable in size with Novosibirsk and Krakow), such a bias cannot be excluded. We discuss this possibility on pg 16 (first paragraph) of the revised manuscript.

3. In the very first paragraph on p. 5 and in the second paragraph on p. 20, elevated CVD mortality and also its increase over the 1990s are connected with diet. It seems that this point has to be made with greater caution. It is safe to say that poor diet was an important contributor to persistently high level of cardiovascular mortality in Eastern Europe in the 1970s-1980s. There are, however, diverging opinions on whether the diet change has seriously contributed to the rapid health improvements in Central Europe and to the rapid health deteriorations in the FSU over the 1990s. In particular, Rychtarikova (2004) argues that the diet and other behavioral changes in the Czech Republic in the 1990s were relatively insignificant compared to rapid and important improvements in the medical care system. Regarding Russia and the FSU, most studies focus on alcohol and/or psychosocial stress as major factors of the recent health crisis. Although RLMS monitoring registered certain diet changes over the 1990s, they seem to be not too harmful and could hardly produce a noticeable mortality increase (see studies by B.Popkin and colleagues for more detail).

**Thank you for this comment. We agree that the influence of diet on mortality and mortality trends is debatable. The statements made in paragraph one (pg 5) and third paragraph (pg 20) have been revised and are now less strong. A reference has been made to the paper by Rychtarikova paper (2004) (pg 21, third paragraph).

4. On p. 18 diet changes are mentioned. It is important to discuss existing studies devoted to the diet changes and their health consequences in Russia and other Eastern Europe. Pay attention to papers by Poledne and Skodova (2000), the ones by Rychtarikova and by Popkin and colleagues (http://www.cpc.unc.edu/projects/rlms/papers.html) and (perhaps) some other significant works.

** As in the point above, we agree that this issue has not been resolved and that more caution is necessary. We have therefore modified the text, added a paragraph (paragraph two, pg 21) discussing the studies which have monitored diets in these countries and their
health consequences. Due to the already lengthy Discussion section, we have added four of the most relevant references suggested by the reviewer.

5. Minor comments:
The manuscript is full of abbreviations. A list of abbreviation in would be helpful for a reader.
In the Abstract (p. 3, para 2) “atleast“.
There is no explanation for “SFA” and “PUFA” (used in Table 4).

**A list of abbreviations has been added to the Title page (pg 2). All other minor comments have been addressed and are highlighted in red text.

Reviewer 2:
Name: Cecile Knai

Minor essential revisions:
This was a very useful paper nevertheless it would benefit from a slightly broader explanation of the factors leading up to the current situation (health/diet-wise). Moreover it would be useful to frame this information in terms of public health issues. What is the impact of the dietary habits reported here? How do these results align themselves with other reports/data? What actions can policy makers take to respond to such results?

** We are grateful for the positive comments. We have added a paragraph (paragraph two, pg 21) which highlights changes in diet and their possible implications.