Author’s response to reviews

Title: Association of environmental and sociodemographic factors with life satisfaction in 27 European countries

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Author’s response to reviews:

Editor Comments:

1. Thank you for stating that Filippos Filippidis is an Associate Editor for BMC Public Health in your ‘Competing interests’ section. Could you please add whether any other of the co-authors have any competing interests to declare? If not, please state: "The rest of the authors declare that they have no competing interests".

Response: This has been amended.

2. Please state whether Image in Figure 1 is your own or taken from another source.

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http://commons.wikimedia.org/wiki/Main_Page"

Response: Figure 1 was created by us using Stata. We have now stated this explicitly in the figure legend.
3. Please consider the list of authors as it currently stands with reference to our guidelines regarding qualification for authorship (http://www.biomedcentral.com/submissions/editorial-policies#authorship).

Currently, the contributions of authors NR and VS do not automatically qualify them for authorship. Please provide clarification on their contributions, or remove their names from the list of authors and place them in the “Acknowledgements” section instead. Please note that any changes in the list of authors requires the completion of the “Change in authorship” form.

An 'author' is generally considered to be someone who has made substantive intellectual contributions to a published study. According to the ICMJE guidelines, to qualify as an author one should have:

- made substantial contributions to conception and design, or acquisition of data, or analysis and interpretation of data;

AND

- been involved in drafting the manuscript or revising it critically for important intellectual content;

AND

- given final approval of the version to be published. Each author should have participated sufficiently in the work to take public responsibility for appropriate portions of the content;

AND

- agreed to be accountable for all aspects of the work in ensuring that questions related to the accuracy or integrity of any part of the work are appropriately investigated and resolved.

Response: We have edited the authors’ contribution section to reflect VS and NR’s contributions, which would qualify them as co-authors.

Reviewer reports:

Angela Chow (Reviewer 1): This study examined the effects of sociodemographic, macroeconomic, and environmental factors on life satisfaction. Following are my comments. I hope my comments can help to further improve the manuscript.

Response: We thank the reviewer for the helpful comments which we indeed believe improved the manuscript.
P.3. Line 17. The texts about the reciprocal association of life satisfaction and mental health should be put right after "Several studies have demonstrated that low life satisfaction is a predictor of depression".

Response: We have made this change.

P.3 Line 19. Life satisfaction is not commonly operationalized with single-item measures. There have been some discussion whether or not single-item measures could be function as reliable and valid tool for capturing life-satisfaction. Nevertheless, there are some initial findings showing single- and multiple-item scales work similarly.

Response: Thank you for pointing this out. We have now edited the text into: “Life satisfaction has been operationalized with single-and multiple-item measures, with single-item measures which ask individuals to evaluate their life satisfaction as a whole using a numerical scale performing similarly to multiple-item scales”.

Measures of Life Satisfaction and analysis. This is my biggest concern. Why the researchers had to combine "very satisfied" and "fairly satisfied" into one category and "not very satisfied" and "not at all satisfied" into another category? The mult-level regression could be run by using one response (e.g., "very satisfied" or "not at all satisfied") as the reference, without any manipulation (combination) of the original data. Although additional analysis using an alternative grouping of responses were reported, this approach is not preferable especially no valid reasons for the combination of responses were given. Please consider to redo the analysis using the original data.

Response: We thank the reviewer for this important comment which was echoed by Reviewer 2. As a response, we have re-analysed our data and adopted a multi-level ordered regression model, which allows us to use all original data without dichotomising the original responses. Ordered regression takes into account the ordered nature of the outcome variable (levels of life satisfaction) and hence it is more appropriate for our analysis. The coefficients are interpreted as the odds ratio of reporting a higher level of life satisfaction for a one-unit change in the independent variable (or compared to a reference category in categorical independent variables). We have edited the Methods and Results sections to describe the new method and present the updated results. We have kept one of the logistic regression models (very satisfied vs all other responses) as a sensitivity analysis, but the ordered regression is presented as our main analysis.

P.6. "Sociodemographic factors" section. Is it possible to add educational level into the model? Education has impact on life satisfaction above and beyond financial difficulties (or income).

Response: An education variable is available in Eurobarometer datasets (“age when respondents stopped full time education”) and it was included in the variables that we explored while building our model. However, adding education to the model did not improve the model fit. We mention in the Statistical analysis section that final specification of the model was decided
following considerations of collinearity and comparing alternative models using the Bayesian Information Criterion and Akaike Information Criterion.

P.6. "Environmental factors". How about natural crisis/disasters such as storm, snow storm, and earthquake?

Response: The reviewer raises a valid point. Unfortunately, we had no data on such events available. We have now discussed this as a limitation in the appropriate section of the Discussion.

P.7. Please compare individuals with complete and missing data (i.e., check attrition bias).

Response: Following this comment, we have compared those with complete and missing data with chi-square tests. We found that individuals who were single, students and aged 15-24 were more likely to have been excluded from the regression analysis. We now discuss this as a limitation of our study.

P.16. "Consistent with existing literature, our findings indicate that males, middle-aged individuals and unemployed individuals are more likely to report lower levels of satisfaction with life." Indeed, research on lifespan development has shown life satisfaction does not necessarily drop at midlife.

Response: We have edited the text to remove the mention to middle-aged individuals.

Richard E. Lucas (Reviewer 2): The question that the authors address in this paper---whether environmental factors impact well-being---is interesting and important. However, I do not think the introduction does a good job setting up the specific question that they will address. For one thing, the beginning of the paper makes it seem as though the research will focus on general characteristics, rather than environmental characteristics that affect well-being. This leads the reader astray a little bit. More importantly, the authors don't really develop a strong rationale as to why it is important to look at environmental predictors. Although I can think of some reasons why they would, it is not obvious to readers exactly why this should be the case. In addition, the literature that the authors cite include a mix of studies that focus on legitimate reasons why environment might matter (e.g., "pollution is bad," "unpleasant climates reduce well-being") and many studies that focus on weather effects as artifacts. Indeed, most of the cited studies that examine whether do not work from a theory where weather *actually* matters. Instead, the theory motivating this work is that weather affects short-term mood (which should be irrelevant for longer-term well-being) and that weather effects reflect a biasing effect of current mood on life satisfaction judgments. So overall, I think that the introduction is not as effective as it could be in setting up the reasons why this question is interesting or the motivation for looking at this question in this specific way.
Similarly, although I am biased because I believe that the weight of the evidence suggests that weather is not reliably linked with well-being outcomes (see, Lucas & Lawless, 2013), I think the authors overstate the extent to which there is now legitimate debate about this issue. For instance, as we review in our paper, most of the studies that find weather effects are either extremely small in size (many with sample sizes below 50) or appear to have extremely flexible analyses (e.g., the Connolly paper that the authors cite did not actually find an overall weather effect; it was only significant among women, which is likely a post-hoc, exploratory analysis that capitalizes on chance). This is a relatively minor issue, as the authors themselves found no effect of weather. However, I think that that is very consistent with the weight of evidence that exists. Furthermore, I think that some of the complicating factors that the authors discuss (e.g., whether warm temperatures are positive in cold climates but negative in warm) have been addressed in some of the prior work.

Response: We thank the reviewer for this thoughtful and knowledgeable commentary on our manuscript. As a response to the comments above, we have extensively revised our text (introduction and discussion).

We have edited the introduction taking into account the reviewer’s suggestion. We still discuss the variability in findings from previous research on the subject, but we highlight the findings from the bigger studies, which are consistent with the reviewer’s view. We believe that the revised introduction paints a more accurate image of the evidence in the field. Additionally, we focus more on environmental factors and discuss more explicitly the potential value of our analysis for public health, which should make the rationale for our analysis more convincing.

The authors dichotomize the life satisfaction scale, but little justification for this decision is provided. Why not use analytic methods that do not require recoding and limiting the amount of information contained in this outcome measure?

Response: Following suggestions from both reviewers, we have completely revised our analytical approach and adopted a multi-level ordered regression model. For details, please see response to reviewer 1’s comment.

Ultimately, as the authors acknowledge, the paper is somewhat limited in the number of environmental variables that they are able to examine and the specificity of the regions that can be analyzed. The authors balance this somewhat by looking at a wide variety of additional demographic factors that might predict well-being. The problem is that these effects have been widely studied (sometimes using the same data), so these analyses don’t really contribute a lot. I think the paper rests on the strength of the environmental analyses, and I have mixed feelings about the contribution of examining this limited set of variables in this way. Again, perhaps a better justification in a rewritten introduction would make the contribution clearer.

Response: We agree that the main contribution of our analysis lies in the exploration of associations between life satisfaction and environmental factors. For this reason, we have slightly
changed the title to highlight the environmental factors and edited the text extensively (particularly the introduction and the discussion) to focus more on these environmental factors. Having said this, we also feel that confirming findings from earlier studies with regard to sociodemographic factors associated with life satisfaction is a worthy contribution of our analysis, considering that we could analyse a big sample from 27 European countries, which has not been done in this population before. Thus, we have kept this element of our analysis, although we now give less weight to it.

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I always sign my reviews: Rich Lucas

I also believe that the role of the reviewer is to identify strengths and weaknesses of a paper, not to provide a recommendation about acceptance versus rejection. Because editorial management systems require a response to questions about recommendations, I always select "revise and resubmit." This selection should not be interpreted as a recommendation, but rather as "I choose not to provide a recommendation."