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Interactive comment on “Using proxies to explore ensemble uncertainty in climate impact studies: the example of air pollution” by V. E. P. Lemaire et al.

Anonymous Referee #2

Received and published: 17 November 2015

In this work we are introduced to a methodology, which is based on the use of a simple statistical model, which aims to confirm the impact of climate change impact on air quality and provide a range of uncertainty. The study is focusing on PM_{2.5} and ozone. While I understand the motivation for the development of such a handy tool, which will allow a fast and low-cost assessment of the impact of climate change on air quality, I am very sceptical on the methodology followed.

My major concern is the use of a very simplistic statistical model which tries to relate linearly PM_{2.5} with selected meteorological parameters. Considering that PM_{2.5} consist of a number of chemical species, which have various dependences on differ-

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ent meteorological variables (depending on their physical and chemical characteristics e.g. hygroscopicity, optical properties etc), I think it is not very responsible to assume a linear link between meteorology and PM2.5. My suggestion is that authors focus on ozone data only, and omit completely the analysis of PM2.5. Therefore, hereafter my comments will focus only on findings related to O3.

Throughout the manuscript, authors claim that with the use of their simple statistical model they can “conclude on the robustness of the climate impact on air quality”. I think this statement exaggerates on what we can expect from a simple statistical model, the use of which, in my opinion, is to provide quick-looks or quick estimates, in a fast and low-cost manner. I would rather tend to consider as “conclusive”, a result from a sophisticated numerical model, or even better, as authors also mention, results of several ensemble members. Therefore, I would suggest the rephrasing of relevant sentences in the revised manuscript.

I would strongly suggest the use of a more sophisticated statistical model, instead of the proposed linear model, to assess the impact of climate on O3. Especially if results prove to be different than those presented in the current analysis (i.e. if current findings are not reproducible), we can be sure that the current methodology suffers from several caveats –which we already known and authors already addressed - which cannot be ignored.

Another major concern comes from the resolution of the regional models used. A 50-km resolution is not recommended for impact studies. One cannot really expect much from the impact-assessment point-of-view in such a coarse resolution. Especially when higher resolution simulations are available in the community, what justifies the selection of 50 Km resolution model results? I would strongly suggest the use of 12 Km resolution simulations. However, if this is not possible, authors should definitely go into a very detailed description of evaluation and uncertainty issues of the climate data they used. Kotlarski et al 2014, Katragkou et al 2015 in Geosc Mod Dev and Garcia Diez et al, 2015 in Clim Dyn, provide details on the uncertainty issues and

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biases that stem from different EURO-CORDEX ensemble members. Authors could inform the readers in a concise way about the uncertainty (spread) associated with each meteorological variable used in the statistical model (temperature, precipitation, radiation) and how this may impact the results presented.

The manuscript needs language editing (grammar, syntax, expression, punctuation).

The title could be changed to become more informative, including more specific key-words such as “statistical model” instead of “proxies”, “ozone” instead of “air pollution”

Technical comments:

Better to avoid using 3 dots for not complete lists, better use etc (e.g. page 28364, line 22)

Abstract. Line 15. Replace “resp.” with respectively

Introduction. Page 28363, line 8/9. “Hence the need to characterize..” the sentence is not complete. Page 28363, line 27. “The lack of multi-model approach in air quality and climate projections..” I think it should be in “air quality” or “climate/chemistry” projections and not “air quality and climate” projections, since there is no lack in climate projection ensembles. Page 28364, line 19. “have” replace with “has” Page 28364, line 1-4. Please rephrase.

Methodology. Page 28365, line 15. What do you mean by “phenomenological” ? Page 28367, lines 21-23: “By using... over Europe”. Please rephrase. Page 28368, lines 24-26: This is a multi-model ensemble consisting of 12 members, 7 out of which are based on the same regional climate model. This implies that climatic information will be too much on the side of the RCA4 climate patterns. Authors could shortly discuss. Page 28369, lines 5-7. Could you please be more explicit? Page 28370, line 16 (and Figure 1 caption). Authors should add in the text and in the figure caption the model suite out of which results are taken. Page 28370, line 13. You can skip “The” in “The Figure 1e corresponds to ...”. Page 28371, line 24. You don’t define SIA and SOA in

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text. Page 28372, lines 8- ..., please explain how you calculate R square/NRMSE for each subregion. Page 28373, line 1-2. "This is because... long range transport of air pollution". Can you provide a reference for that? Why couldn't that be an impact of boundaries? Page 28373, line 4-6: Where do we see the lower variability of temperature and incoming SW radiation? Do you suggest that Temperature and SW radiation are not relevant meteorological variables to explain O3 variability in south Europe? This does not make sense to me.

Section 4. This section could be more carefully written, with better structure and proper reference of uncertainty issues available in published literature (see general comments above).

Page 28374, line 17. How do you explain the fact that surface temperature increases significantly (did you check for significance? Better to avoid using if not properly tested), and PBL does not change notably? Any references that you could use?

Page 28377, line 5. I don't think you should use the word "slightly".

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 28361, 2015.

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