

Interactive comment on “Influence of anthropogenic emissions and boundary conditions on multi-model simulations of major air pollutants over Europe and North America in the framework of AQMEII3” by Ulas Im et al.

Anonymous Referee #2

Received and published: 17 April 2018

Review of manuscript ACP-2017-1231 by authors Im et al.

The manuscript describes results from AQMEII3 exercises studying the impact of global and regional emission reductions using regional models fed by the global model C-IFS.

There is a lot of work behind model ensembles and inter-comparisons. The manuscript present results mainly in a clear manner, but there are sections that need to be clarified. I also see that information about the model simulations included is lacking. I have the

Printer-friendly version

Discussion paper



following main points that I think important for the authors to address before publication of the manuscript in ACP:

- Table 1. You are ordering runs according to groups, not according to models. After a more thorough look many of the groups use the same model, sometimes even on the same resolution. What is the use of an ensemble of groups running the same model? Ideally this should give exactly the same results unless someone makes an error or the model version is different.
- Linked to the previous bullet: you end the conclusions with raising the issue of the impact of different model parameterization. However, you do not include such information. You should include a description of important model facts and add a discussion on these linking them to your results.
- Based on this information, perhaps some model runs should be removed from the ensemble (too many of the same model? Too simple parameterizations for some species?).
- In the abstract you describe daily maximum 8h mean ozone. Is this what you show and evaluate in the tables and figures? Or is it monthly/annual means? You need to clarify this (in all figures/table legends as well as in the methods) or (/and) only include results in the abstract which you are actually showing as results in figures/tables.
- The RERER value analysis is interesting. It would be of great value if you describe the ozone RERER value based on monthly values (daily max 8h mean or mean), since ozone formation capacity/local contribution is seasonally dependent. Perhaps you can come up with a smart way of illustrating these rather than just adding more table values.
- I don't see the point of showing figures 11 (GLONAM-BASENAM) to 14 (EUREUR-BASEEUR). I would much rather see geographically resolved RERER values as a complement to the other figures.
- Line 221-224. The method of first taking difference then calculating mean is only valid

[Printer-friendly version](#)[Discussion paper](#)

if you are working with means. How do you treat the daily maximum 8h mean? Is the method valid for this metric (if that is what you are showing in the figures for ozone).

- Table 1. The number of simulations (scenarios) is different when comparing the table to the method text (for Europe). An x is missing in the table (grey area for north America-region).
- Table 3. You state unit: % for NMB and NMGE, but the values in the table are clearly without unit. You should not have different units for North America and Europe (for RMSE in this case).
- You have a supplement but you do not refer to it in your manuscript.
- The figure legend of S3 is incorrect.
- Section 2, first paragraph is messy and repetitive.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-1231>, 2018.

[Printer-friendly version](#)[Discussion paper](#)