

Interactive comment on “Multi-model evaluation of short-lived pollutant distributions over East Asia during summer 2008” by B. Quennehen et al.

Anonymous Referee #1

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Multi model evaluation of short-lived pollutants over East Asia during summer 2008.

This paper reports on an exercise where six global and 1 regional model are performing the same model experiment (i.e. using the same ECLIPSE emissions, summer 2008) and compare model results with a collection of measurements. While the paper pulls together an interesting set observations, the paper is not well framed in terms of defining a key-question or hypothesis. For instance can the models be used to constrain emissions? Can the set of models be used to provide better estimates of air pollution, RF, or any other parameters, compared to earlier studies.

While the region of interest is highly relevant, it is not clear why the problem is tackled with six global models, and only 1 regional model. It is also not clear what the new

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findings are, compared to what is already known.

I could imagine that the paper would be better placed in the ACP sister journal, GMD (characterizing model performance), an option that the editor and authors should seriously consider. In summary I think the paper in its current form has to little scientific novelty to justify publication in ACP. The authors could consider however choosing a more in depth discussion of certain aspects of the evaluation. In my comments below I mention some potential deepening of the analysis. I suggest that such work doesn't necessarily have to be done with all models, but additional sensitivity studies with 1 or 2 models could make the paper more relevant and attractive.

Detailed comments:

P 11051 I 5 troposphere includes surface=>better tropospheric columns

I 6 I don't think a mismatch of NO₂ between global models and a few stations can be conclusive on emissions, given coarse resolution of the models. The regional model should be more representative- but even then it will depend much on the vicinity of local sources.

I. 9 I think to some extent this provides the argument that coarse resolution models can not be used for such detailed comparison in polluted areas. This is not new knowledge.

I. 10 What are the important conclusions? Are the global model RFs all wrong? What did we learn?

p. 11055 I. 15 Although of course attractive to use, CAREBEING was taking place in a period of lower than usual emissions, while this was not considered in the models. This is of course strange, but perhaps only minor fraction of analysis is affected? Not clear. Why not use one model with 'lower' emissions in the period and region of interest?

p. 11056- table 1: Is it correct that NorESM was not running 2008? If so, why is it then included?

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p. 11056 Can you provide some statement on the effect of not including seasonality in most emissions? (Especially since the focus on the short period). Here a sensitivity experiment would have been appropriate.

p. 11057 l. 11 There is some strange reasoning: on the one hand dust emissions in WRF-CHEM are too high, and two lines later it is argued that it doesn't matter anyway, so why leave it out?

p. 11059 Was NorESM nudged, or calculating own meteorology? If the latter, why was it included?

p. 11059 what means FNL?

p. 11061 Does it make sense to compare WRF-CHEM with the IASI ozone profiles?

p. 11061 explain the resolution of IASI and how models at different resolution were compared. One month or one year?

p. 11062 indeed global models have difficulties putting the correct location of the monsoon flux.

p. 11063 l. 20 Which time period? interesting to see that the models are having so different bias/error with regard to NO₂. However I would like to be sure that they really used the same emissions. Then some dedicated experiments could point to explain why the difference are so large (lifetime/mixing/deposition)?

p. 11064 If air quality is an issue, the authors should worry also about the vertical resolution of the first layer, the height of the emissions, the height of the measurements, and how the model was sampled.

p. 11065 Given the lifetime of CO, it would be instructive to learn what OH concentrations the models are having now, and what would be needed to reproduce the right gradients? Can the coarse resolution models be used for this?

p. 11066 l. 25 I understand that properly taking into account aerosol scattering is a big

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thing for NO₂ retrievals. Was this done at these locations. What could the error?

p. 11068 l. 25 What was assumed for size distribution for particulate emissions?

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