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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global ozone and air quality: a multi-model assessment of risks to human health and crops" *by* K. Ellingsen et al.

Anonymous Referee #2

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Surface ozone simulations from a total of 18 global models are analyzed in this work. Air quality indices for the health of humans and vegetations are computed to infer the environmental impacts. There is a lot of analysis work that went into this paper and it is the first attempt of such a comprehensive study in literature. The scientific questions are clearly presented. Most problems in the analysis were acknowledged and discussed in the paper. Where the paper falls short is that no clear solutions to these problems were found. There are so many uncertainties in the models and measurements that the scientific relevance of the model results to any policy applications is questionable. It seems to me that the first question to answer in this type of analysis is either how the global model results can be applied to air quality assessments quantitatively or if it is impossible to do such assessments. I think the evidence presented in

the paper suggests that it is extremely difficult and likely impossible. If that is the case, it should be clearly stated. I will leave it to the Editor to decide if that is worth publishing in ACP.

Specific comments:

The usage of the SOMO35 index is inappropriate in air quality assessments. Since there is no threshold information on this index, it is difficult to use this index for any policy-relevant assessment. It is interesting to look at as a modeling exercise, but there is not much meaning in the results. The indices for crops and forests have similar problems because the thresholds vary drastically among the different species. The threshold value for a selected type of crop or forest also varies significantly from study to study. The one-threshold-for-all method used in this paper oversimplifies the problem to the degree that even if the model results are perfect, the impact assessment is still qualitative.

The best indices to use are obviously EU60 and USEPA80. As was noted in the paper, the model results should be weighted by population, but they are not. I wonder if there is any indication that the exceedance of USEPA80 is over such large areas and so often. It seems to me that the indices calculated from the model results are too high considering that ozone was first averaged over very large regions. A proper comparison is of these indices for the US and Europe, where there are plenty of observations. The comparison in Figure 2 is misleading for air quality applications. Figure 1 shows that highest ozone is either over Tibet or over the oceans. It is not informative.

Sections like 4.3 raise more questions than they answer. At the end of the section, I see no solution emerging from the long discussion.

The correlation between SOMO35 and EU60 is curious. I do not see why it serves any purpose. Both can be calculated easily. There is no point to calculate one and then infer the other from the correlation. EU60 has direct policy relevance, but SOMO35 does not. The R value in 2000 is almost 1, but the correlation breaks down in 2030.

ACPD

8, S1054–S1056, 2008

What does it mean?

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8, S1054–S1056, 2008

Interactive Comment