

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 #1

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The paper uses the output of a large number of numerical simulations of surface ozone obtained by global numerical simulations (performed as part of the network of excellence ACCENT) and attempts to make predictions of the surface ozone (in the form of violations of several threshold values) for the future as function of changes in ozone precursor emissions. The paper discusses and reviews in great detail the air quality indicators (almost a review on its own) but the justification of this exercise is mainly based on the reasonable comparison of the ensemble simulations with ground based ozone monitoring data for some parts of the world. Despite the fact, that the paper indeed describes the most extensive evaluation of surface ozone calculated by (see first sentence of Summary and Conclusions) I object the publication in ACP, at least in its present form.

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The Abstract (which is the most important part of the paper) does not reflect in any way the limitation of the method used. The (fair) agreement of present day ozone measurements in some areas of the world does not really justify the use of this large data exercise to (firmly) conclude something for the future, unless the limitation of the method (which is discussed in some parts of the manuscript, see below) is reflected in the Abstract (and the Conclusions) in an appropriate way. Another problem is the readability of the Figures, when the paper is plotted: The Figures are much too small and the reader needs to guess. I also recommend determining the different air quality indices directly from the ozone measurements in order to compare these results with those obtained in the model world. Individual points: Abstract: 1. Abstract: Needs to clearly declare the limitations of the method used (see comments 9, 11). What means (line 7), are characterized well; by global models? One also should mention that the some models have a maximal horizontal resolution of 10 over Europe, some are based on 40x50 horizontal resolution (see first paragraph of the experimental setup). Introduction: 2. Line 18, p. 2167: You might add: Ordóñez et al., 2005: C. Ordóñez, H. Mathis, M. Furger, S. Henne, C. Hüglin, J. Staehelin, A. S.H. Prévôt: Changes of daily surface ozone maxima in Switzerland in all seasons from 1992 to 2002 and discussion of summer 2003. Atmos. Chem. and Phys., 5, 1187-1203 (2005). This paper (as well as the paper of Jonson et al., 2006, I think) also addresses the fact, that despite the large decrease in ozone precursor emissions in Europe during the 1990s the decrease in surface ozone values over Europe was (too) small compared with expectation (numerical simulations), which might be important for the discussion of the limitation of the method. 3. Line 18, p. 2167: Does Simmonds et al. 2004 relate the increase in surface ozone at Mace Head to increases in global ozone precursor emissions as suggested in the following sentence or who attributed which trends to which emissions, please specify more clearly. 4. You also might mention the paper of C. Ordóñez, D. Brunner, J. Staehelin, P. Hadjinicolaou, J.A. Pyle, M. Jonas, H. Wernli, and A. S.H. Prévôt: Strong influence of lowermost stratospheric ozone on lower free tropospheric ozone changes over Eu-

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rope, Geophys. Res. Lett. , 34, L07805, doi:10.1029/2006GL029113(2007), showing rather similar trends for ozone at Jungfraujoch (Switzerland) and those reported by Simmonds et al. For Mace Head. 5. Line 20-line 22, p. 2167: Future surface ozone levels will be mainly determined by emission control of ozone precursors. I am not convinced, whether this sentence is valid in this absolute sense, since many factors connected with other changes, such as climate change (e.g. if the heat wave of 2003 over Europe occurs often in future, this might have a large effect on Central European surface ozone) and possibly effects of other changes, such as connected with changes in Brewer Dobson circulation and increase in stratospheric ozone might be (very) important for PBL ozone in future as well, though they are difficult to predict and to quantify. I think that you rather should clarify that you (mainly) will describe effects of changes in ozone precursor emissions on future surface ozone in this paper. 6. Line 16, p 2168: You mention, that Stevenson et al. 2006 showed, that the ensemble runs were in reasonable agreement with the measurements of the ozone sonde network, but I think, you should not suppress, that this paper also contains a further analysis, showing an interesting relationship: the analysis of the output of the individual model result indicate, that the larger the burden the shorter the ozone live time: Did you try to test, whether this relationship has any effect on surface ozone prediction of the individual models ? Experimental set-up: 7. Line 27, 2169 - line 3, p. 2170: This has been already mentioned in the introduction and comment 5 needs be taken into account here as well. 8. Line 26-28, p. 2171: Organic species: I recommend to separately include the number of organic gaseous species in Table 1, since the treatment of organics is particularly important for ozone production in the PBL. 9. Line 10-line 16, p. 2172: Here the first time appears, that the global models cannot resolve small scale phenomena ! I think, this fundamental aspect should be mentioned in the Abstract ! 10. Line 18, p. 2172: Does the reference Cubasch et al., 2001 refer to chemical numerical simulations ? Ozone air quality indices: 11. Line 7-10, p. 2176: I think, this statement should be reflected in the Abstract (see comment 1) Modeled ozone and comparison with observations 12. Line 4-line 6, p. 2177:

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What's the reason for the statement, that the interaction of increasing concentration in CH₄ and worldwide emissions in NO_x are causing the increase in modeled the tropical and mid-latitude Northern hemisphere; - please explain and give a reference 13. To Section 4.2: When comparing model output and measurements you also should address the problem of model topography vs. altitude of the measurements: The model topography in global is usually much smoother than in reality and this needs to be considered when compared to measurements: I think that the surface ozone measurements from China of Akimoto and Pochanart (reported as Unpublished data;) are made at elevated sites which might not be representative for the polluted planetary boundary layer. 14. Last paragraph, p. 2177: Fig. 1f: In addition to the points mentioned in the text large changes such as a considerable increase in occurrence of heat waves as observed in summer 2003 in Europe and substantial changes in ozone in the lower most stratosphere combined with changes in Brewer Dobson circulation are difficult to predict and therefore the prediction of climate change; should be regarded with extra caution (comp. comment 4) ! 15. Line 12-15, 2179: What do you mean by very few coordinated networks monitoring ozone outside Europe and North America; ? I think, ozone values are published from many sites in Mexico and ozone is monitored in countries in South America. Ozone is monitored at many sites in Republic China but the results are not available for the community. 16. To 4.3: I think, one should add that all the global models need parametrizations because of the many complex interactions which cannot be resolved by the simulations. However, these parametrizations need justification be comparison with measurements; maybe the models work better for some parts of the world where ozone measurements were available to improve the parametrizations. 17. Last paragraph, p. 2182: What did you learn from the comparison with the measurements from Australia ? Are no ozone measurements performed in Australia ? Calculated ozone air quality indices: 18. Did you calculate the violations of the various thresholds directly from the available measurements ? I think, it would be valuable to show these results and to compare them with the results

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derived from the output of the models 19. How looks plot of Fig 5 when using available PBL ozone measurements ? (I think, "present day ozone" means the output of the present day numerical simulations or is this wrong ?) Line 7-10, p. 2176: Global application : I think, you could strongly increase the reliability of the study when calculating first the indices from the available PBL ozone measurements (at least for the regions, where reliable ozone measurements are available) before do make the calculations using data obtained from numerical simulations. Outlook: 20. Line 21, p. 2188-line 4, p. 2194: I don't see any justification for this paragraph: The paper is already very long and the reader does not really needs to learn about the "Task Force on Hemispheric Transport";: Indeed the systematic analysis of the model uncertainties is important to "hopefully" lead to further model improvements: I think, the authors should report on their improvement when they have done their job (when this is no longer a hope) but I don't believe that this hope is worth to be communicated in a separate paragraph.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 2163, 2008.

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