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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Pre-industrial to end 21st century projections of tropospheric ozone from the Atmospheric Chemistry and Climate Model Intercomparison Project (ACCMIP)" *by* P. J. Young et al.

Anonymous Referee #1

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This paper analyses the tropospheric ozone obtained from a multimodel ensemble under the Atmospheric Chemistry and Climate Model Intercomparison Project (ACCMIP). The manuscript is well structured and well written, and all the aspects of tropospheric ozone are correctly analysed. The comparison of present day (year 2000, simulation Hist 2000) ozone mixing ratios with observations is discussed in detail, but I think that section 5 (about past and future ozone mixing ratios simulated by the models) is scientifically the most interesting part, together with section 6 where the results are discussed.

In principle I am in favour of such manuscripts and the work behind it, as an intercomparison between models is essential to understand flaws and to indicate improvements for single model. Following such considerations, I am a bit puzzled about this paper: although the ensemble mean is analysed in great detail (and also the single models, especially for past and future predictions), it is missing some clear scientific goals or outcomes, or at least these are not correctly presented in the abstract/conclusions.

It is good to see where the ensemble is performing well or not compared to the observations, and also how ozone could change in the future, but the manuscript lacks of a conclusion that I would very much appreciate: where should the model be improved? Or better: where is located the largest error (or variance, if you believe it is related) source of the models, taken singularly or as ensemble? On the same line: what are the key factors controlling ozone in future or past scenarios? From what I could infer from the abstract and the introduction this is "the last major multi-model comparison" (Page 21621, line 27) without a lot more. This problem is somehow underlined also by the authors, who claimed that "a unified approach to ozone budget specifications is recommended" (Page 21618, Line 23), using this as a sort of excuse for not giving too much quantification of where the uncertainties lay. But, is it really so bad that nothing can be said? I believe that the paper could improve a lot if few clear conclusions were drawn in the abstract/conclusion sections.

I think that almost all of the scientific results that I can see in this paper were already mentioned by Stevenson et al. (2006) more than 5 years ago. Is it not possible to go further?

Hence I would recommend the publications once this main concerns is addressed. Although I understand the hard work behind, I hope that my suggestions and my request of additional material will help the authors to improve this manuscripts.

Main concerns:

- **Scientific goals.** As mentioned before it would be good if anything is said re-

garding the possible improvements. Where are the main source of errors located? Would a more sophisticate emission dataset lead to large improvement? Although I understand that without a rigorous ozone budgeting this is not quantitatively possible, I still think that even a qualitative assessment will improve the paper. Some of what I would like to see is in some degree already mentioned in the manuscript (see Sect. 6), but it would improve the paper if these interesting interpretations were highlighted in the abstract/conclusions. For example, focusing on the past-future ozone estimations, the stratosphere-troposphere exchange of ozone is shown to be very important (see Page 21640). Is it possible to estimate somehow the relative influence with respect to the other analysed key factor (i.e. methane and NO_x)?

- **Metric.** I know that this sounds like a race between models, and it is not my intention to suggest it. However any model developer could learn a lot about its own model comparing his results with observations and other models as well (especially the ensemble one, which should give the best results). The deep analysis carried here would be very profitable for all models. Hence I suggest to add in the electronic supplement some comparison based on a selected metric and add a chapter in the text summarizing such results. This is partially present in the tables, and, in theory, this information could be infer from the figures in the supplement. However, I got lost in the hundreds of figures, and an additional summarizing plot (such as Taylor's diagram) is needed. Finally, please compare also quantitatively your results with previous results from other multimodel ensemble: is dry deposition or stratospheric-tropospheric exchange in your ensemble still comparable to what obtained by Stevenson et al. (2006)? In the manuscript I could find only one quantitative comparison to previous results (i.e. Page 21631, line 14-15), while the others were only included in the figures.

Minor concerns:

- **Page 21618, line 5**. What do you mean with "well"? Could you add a correlation value?
- **Page 21620, line 11** "ozone precursor emissions [..] were high". High with respect to what? Garnaut et al (2008) suggested that the previous emissions are underestimated for green-house gases. Could it be the same for ozone precursors as well? Could you also conciliate these different points of view?
- **Page 21623, line 8-16**. Although tedious I think it is necessary to list the meaning of the acronyms or at least a reference for each model, once mentioned for the first time.
- **Page 21626, line 4-5**. The t-Student's distribution converge to the normal distribution only for large number of degree of freedom. In your case you have limited number of models (14 or 15 models). Hence I expect that the 95% confidence level not to be given by the 1.96 times the standard deviation. Please check.
- **Page 21630, line 5**. " Figg.2d and 2g show higher ozone levels over source regions". I expect that the authors mean regions where ozone precursors are emitted. Due to page formatting is difficult to see the figures (I suggest to enlarge them in the revised version). However from Figg.2d and 2g the Mediterranean basin has higher ozone concentrations than central Europe. Is this also a sign that precursors are more emitted over the Mediterranean basin than, let's say, France or Germany? Please reformulate this sentence.
- **Page 21630, line 17-18**. What about different Planetary Boundary Layer Height (PBLH)? Could it also play a role in the variability between model at the surface? If so, how?
- **Page 21631, line 4**. I am intrigued by the large variability of the ensemble in the Arctic. Could it be related to precursor transport from Europe and its relation

with the North Atlantic Oscillation (see Christoudias et al., 2012) ? From the introduction I deduce that the model were forced with a climatology of the year 1995-2005. Were the model perhaps presenting a persistent positive NAO index, implying a persistent northward transport of pollutants from Europe?

- **Page 21631, line 21-25**. Is the VOC emissions mostly connected to Isoprene emissions (as the authors suggested in Sec.2.1)? If so, there should be larger standard deviations for future scenario mainly over tropical forest. Is that the case?
- **Page 21633, line 11**. From the figure, the ozone levels at 500/750hPa in the NH winter (January) are always overestimated (and outside the standard deviation). This seems to disagree with the text. Please clarify.
- **Page 21633, line 17**. I like this point very much. Is it possible to guess why there is such improvement?
- **Page 21637, line 5**. Please, add a number after "good". Is it $R^2 > 0.9$ or > 0.5?
- **Table 1,2**, Tab. 3 is exactly the same as Tab.1. You should move Tab.3 to the electronic supplement, as it does not give any additional information.

References

Christudias, T., Pozzer, A., and Lelieveld, J.: Influence of the North Atlantic Oscillation on air pollution transport, Atmos. Chem. Phys., 12, 869-877, doi:10.5194/acp-12-869-2012, 2012.

Garnaut, R., Howes, S., Jotzo, F., and Sheehan, P.: Emissions in the Platinum Age: the implications of rapid development for climate-change mitigation, Oxford Review of Economic Policy, 24, 377–401, 2008.

Stevenson, D., Dentener, F., Schultz, M., Ellingsen, K., Van Noije, T., Wild, O., Zeng, G., Amann, M., Atherton, C., Bell, N., et al.: Multimodel ensemble simulations of present-day and near-future tropospheric ozone, Journal of Geophysical Research-Space Physics, 111, 2006.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 21615, 2012.