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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* “Reanalysis of tropospheric sulphate aerosol and ozone for the period 1980–2005 using the aerosol-chemistry-climate model ECHAM5-HAMMOZ” *by* L. Pozzoli et al.

**Anonymous Referee #1**

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Review of “Reanalysis of tropospheric sulphate aerosol and ozone for the period 1980-2005 using the aerosol-chemistry-climate model ECHAM5-HAMMOZ” by Pozzoli et al. In this paper the authors presented results from two hindcast simulations: one with varying anthropogenic emissions and one with constant emissions. They compared simulated results to measurements, in particular surface measurements over North America and Europe.

I found this to be a very nice paper. Multiyear analysis of chemical hindcast simulations is a difficult undertaking. It involves sifting through an enormous amount of data, syn-



thesizing the data, and presenting the results in a clear and cohesive manner. I think the authors succeeded extremely well in this. I found the analysis and presentation of these simulations to be clear and insightful. The figures and tables included in the paper were well chosen. They are easy to read, and yet present a great deal of useful information. The text was well written.

I have a few comments and suggestions below, but would recommend this paper for publication in ACP with only minor changes.

1. I think the paper could have tied its results in better with some of the ozone trend work already published. While the authors addressed simulated and measured ozone trends globally, I feel they have not been very diligent in referencing some of the previous work on these trends. Some of the papers that come to mind is the work of Dan Jaffe and Dave Parrish in the Western U.S.; Jonson and others in Europe; Lamarque et al. in their recent ACC-mip simulations.

2. The meteorology was apparently changed from reanalysis to operational analysis in 2000. It would be surprising indeed if this has no impact on the meteorological variables. While I understand the authors had little choice, I think there is some cause for concern here. While the authors comment on it briefly later in the paper, I think the possible ramifications should be clearly addressed near the beginning of 10196 where the authors first discuss the model simulations.

3. The simulated emissions were apparently kept constant after 2000, or were based on trends. In some locales there is now enough information to assess how good these projections are based on more recent inventories. I think this would be valuable. To what extent are the simulated emissions after 2000 consistent with more recent measurements? I don’t think an extensive evaluation is needed here, but it would be helpful in interpreting the results if the authors could consider their emissions in the context of more recent results.

4. Forcing a general circulation model (CAM) with interannual SST variations (10210:

Line 20) as in Hess and Mahowald should still incorporate significant meteorological variability. Thus, I think it is rather speculative to assume that the rather low variability found in the Hess and Mahowald runs is due to the SST forcing. I think the conclusions (page 10220) raising questions about the applicability of this technique to future climate experiments are too strong. It will be interesting to see if this is a general result when more models use this type of simulation for chemical analysis.

5. I would have liked to see a little more discussion in the conclusions as to the general importance of meteorological variability on chemical constituents. The conclusions mention global meteorological variability, but not the importance of the meteorological variability on the chemical record. I think this was an important and interesting component of your study.

6. I think it would be helpful if you could summarize some of the aspects of interannual variability the model really does not capture well. While I think you addressed this somewhat, it would be helpful to have a better understanding of the unsolved problems that remain: what aspects of the observations do we really not understand?

Minor Comments:

-An additional very careful reading of the paper would be helpful. There are a number of places where words were missing or there is a grammatical mistake. Some of these are listed below along with other minor comments.

Page 10193 Abstract, line 15: I found this sentence confusing. Are the numbers in parenthesis standard deviations? At this point it is not clear what you mean by natural and total variability. A little bit of explanation here would be helpful.

Abstract, line 20: "Ozone increases" should probably read "Simulated ozone increases"

Page 10194 Line 23-24: "To our knowledge . . ." I would leave this sentence out or make it clearer. What do you mean by consistent?

Page 10195 Line 7: I think "antagonistic" is the wrong word here.



Page 10196 Line 11: "Luca Pozzoli 27 March 2011 10:39 a.m" Line 26-27: "The relaxation technique . . . to retain . . . cloud and aerosol ". I'm not sure what you mean here.

10198: Line 15: "derived emission trends". Please give where these emission trends are derived from. Also, I assume from figure 2, that the derived emission trends change on an annual basis. Please clarify in the text.

10201 Line 6. Please change "from" to "are from"

10203 Line 25. Discussion about the influence of model bias on model variability and trends: did I miss this discussion or was it omitted from the paper?

10204 Line 3. It is not clear exactly what the EU region is. I believe it may be the same as the EU HTAP region, but this should be clarified. Line 9. "Augments" by 9 ppbv. I think you mean increases by (augments by is not very good English).

10205 Line 5. 20% reduction. I presume you mean 20% reduction over Europe of all HTAP emissions. Line 6. "were resulting" Line 22. Chage "Despite" to "Despite the fact"

10207 Line 6: "125%". This estimate seems small compared with satellite measurements of increases in NO2 and estimates from other emission inventories (e.g., Klimont et al., 2009).

10208: Line 16: "We further remark. . .". This comment is rather out of place here. Instead it should be given up above where describe the model evaluation.

10209: Line 2: Is stratospheric influx determined as a residual?

10211 OH variability. Please briefly specify the model processes here that contribute to the OH variability. In particular I think it should be mentioned that interannual variability in the ozone column is included. Are the effects of Pinatubo included?

10214 Line 11: "interannual variability" – do you mean trends? Line 16: ".05-.08" I believe this should be -.05 to -.08.

10218: Line 1: "... correlation ..." It is not clear from the sentence what you are correlating with what. Line 9: the global number given in Table 5 is -1.32, much smaller than the range -26 to -48 given here. Is the global number correct?

Tables and Figures Figure 2. What is graphed here are changes in emissions, not the emission trends. Thus the title of the figures is somewhat misleading. Table 5. It is not clear which correlations are significant and which are not.

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 10191, 2011.