Atmos. Chem. Phys. Discuss., 11, C6447–C6452, 2011
www.atmos-chem-phys-discuss.net/11/C6447/2011/

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.

L. Pozzoli et al.

pozzoli.ist@gmail.com

Received and published: 15 July 2011

We thank Reviewer #2 for his/her extensive and instructive remarks, which we tried to address below and in the revised manuscript.

Anonymous Referee #2 Received and published: 3 June 2011 This is a very extensive analysis of hindcast simulations with emphasis on sulphate and ozone. I have a certain number of minor questions and comments that would need to be addressed by the authors.

-Page 10196, line 11: remove extraneous text starting with "Luca"

Corrected.

-Page 10196, line 21: does the use of separate analysis lead to any discontinuities?

As we answered to Referee #1, we have no evidence of discontinuities in our analysis; we have explicitly highlighted this point to warn the readers in Section 2 when describing the model simulations.

-Page 10197, line 1: other GHGs in this case does not include CH4

CH4 concentrations are prescribed for the calculation of the radiative budget. It is included in the Nakicenovic et al. (2000) dataset.

-Page 10198, line 15: it would be good to either define the region boundaries or refer to the Figures.

We added a reference to Figure 1 to make clearer the definition of the regions.

-Page 10199, line 9: what is the soil NOx contribution?

In our study NOx emissions from soils are included in the RETRO inventory and they are 9.3 Tg(N)/year. Inter-annual variability is not included in the soil emission of RETRO inventory. Other estimates of the soil NO sources range between 9.7 TgN yr-1 Potter et al. [1996] and 21 TgN yr-1 Davidson and Kingerlee [1997] whereas estimates, that include the role of canopy deposition, are about 50% smaller [Yienger and Levy, 1995, Ganzeveld et al., 2002].

-Page 10204, line 26: add "modeled" before "North Atlantic"

Corrected.

-Page 10205, line 22: this is an important point that could be highlighted more. It shows that getting the mean is no indication for being able to get the tendencies.

We propose to add a point in Section 7 'Summary and conclusions' or Section 8 'Out-

look' to highlight the difficulty in determining a trend and, as said by the reviewer, how getting good correlations between observed and modeled quantities is not a sufficient indication of being able to reproduce an observed trend. On the other hand, in some cases the observed trends can be reproduced fairly well also in presence of high biases in the model simulation, looking at the anomalies.

-Page 10206, lines 26-28: this statement should be substantiated or at least somewhat documented. As is, it is not very useful.

We decided to remove this sentence.

-Page 10210, section 4.3.2: this section is far from being satisfying. It would definitely strengthen the paper to include more discussion. In addition, J(O1D) is probably more important for OH.

The section 4.3.2 has been removed and included into the section 4.3.3 "Variability, re-analysis, and nudging methods". In section4.4 we discuss the variability of OH. As said also in the answers to Referee #1, the main processes that contribute to the OH variability are both meteorological and chemical. Dentener et al. (2003) found that OH variability for the period 1979-1993 was mainly driven by meteorological processes, i.e. humidity/temperature and wet removal/precipitations. They found only a small total contribution from the changes in chemical species like CH4, O3, and emissions of NOx, VOCs, and CO. Differently from Dentener et al. (2003), in our study the effect of meteorological processes (SFIX) includes also changes in emissions of CO, NOx, and VOCs from biomass burning and lightning emissions, and it results in a negative trend of OH tropospheric concentrations. We found a good correlation between the OH decreasing trend and the decreasing trend in our NOx emissions from lightning (R=0.78), while lower correlations are found with water vapor (R=-0.48) and photolysis rates at surface (e.g. JNO2, R=-0.65; JO1D, R=-0.56). In our study the variability from stratospheric O3 is not included as well as the effect of Pinatubo eruption.

-Page 10210, lines 19-22: if this mild nudging is indeed responsible for only a partial

inclusion of processes responsible for interannual variability, then this could easily be tested by varying the strength of the nudging. This type of simulation can be simply done with tracers.

This sentence was modified also according to Referee #1 suggestions and now reads: 'This likely indicates that using different nudging methods (only prescribing through monthly averaged sea-surface temperatures or using different re-analysis datasets, such as ECMWF and NCEP) can give significant different answers on the processes that govern inter-annual variations in the chemical composition of the troposphere.' We further remark that in ECHAM5-HAMMOZ we have not applied this technique.

-Page 10211, section 4.4: it would be nice to include a methane lifetime estimate

CH4 lifetime is now included. "The mean value of the yearly means of CH$_4$ lifetime over the period 1980-2005 is 10.4 ± 0.14yr in the SREF simulation. This value is just above the 1-σ interval of CH4 lifetimes reported by the all model average in Stevenson et al. (2006)."

-Page 10220, last paragraph: you have actually not shown it that "nudging methods was also shown...", just indicated in the paper it was an hypothesis.

We were referring to the study of Hess and Mahowald and our study. We rephrased the sentence. It now reads: 'Comparing similar re-analysis studies, such as our study and Hess (2009), it is shown that the choice of re-analysis product and nudging method have strong impacts on variability of O3 and other components. For instance the comparison of our study with an alternative data assimilation technique presented by Hess (2009), i.e.\ prescribing sea-surface-temperatures (often used in climate modeling time slice experiments) resulted in substantially less agreement.'

-Figure 2 would probably be more useful with actual emission amounts instead of relative differences.

We prefer to keep the version with relative differences because is possible to see better

all the species with the same scale. This is more difficult with absolute values because the species have quite different ranges. We also added to Figure 2 a comparison to relative emission changes given in Lamarque (2010). Moreover the absolute emissions are mentioned in the legend of the figure and in Table 1.

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

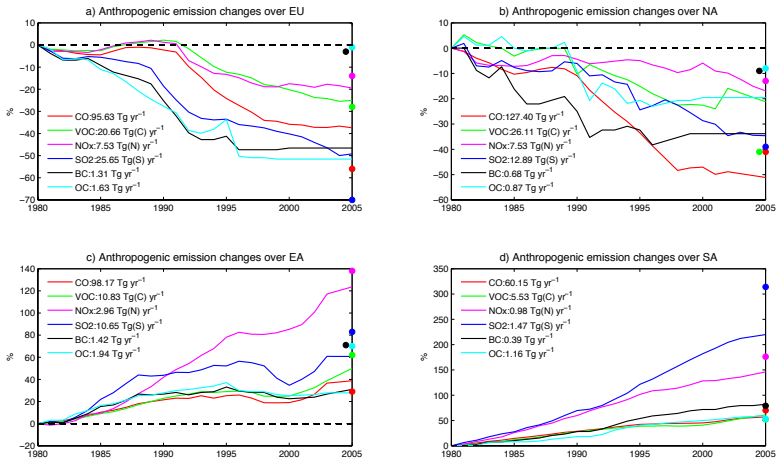

Fig. 1.