

## ***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 #2**

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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" Page 10196, line 21: does the use of separate analysis lead to any discontinuities? Page 10197, line 1: other GHGs in this case does not include CH<sub>4</sub> Page 10198, line 15: it would be good to either define the region boundaries or refer to the Figures. Page 10199, line 9: what is the soil NO<sub>x</sub> contribution? Page 10204, line 26: add "modeled" before "North Atlantic"

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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. Page 10206, lines 26-28: this statement should be substantiated or at least somewhat documented. As is, it is not very useful. 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(O<sub>1</sub>D) is probably more important for OH. 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. Page 10211, section 4.4: it would be nice to include a methane lifetime estimate 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.

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

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