

Interactive comment on “Attribution of Modeled Atmospheric Sulfate and SO₂ in the Northern Hemisphere for June–July 1997” by C. M. Benkovitz et al.

Anonymous Referee #2

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As noted by the authors, this paper is a contribution to the growing body of knowledge indicative of the long range transport of submicrometer aerosols. Although this is a worthy goal, I think the authors need to strengthen the paper in a number of respects before this contribution should be published.

My main reservations concern the very limited time period for which this study is conducted - with just 4 weeks of results. As noted by the authors, other papers have dealt with annual or multi-annual time-scales. Indeed, many of the authors were involved in the three-year simulation of Rasch et al. (2000). Six years later, I did not see convincing arguments that the current study of a four week period extends upon the results of

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annual or multi-annual simulations.

The authors may have good reason to suppose that this 4-week period is representative in some way, in this case they need to make this argument.

Further, if focussing on such a limited period, I think the links between meteorology and concentration/burden need to be strengthened, perhaps with the use of trajectory analysis.

1. p. 4026, line 28. Explain why the Graf study found similar fractional contributions of volcanic and anthropogenic emissions.
2. A lot of the material here is taken directly (or almost) from Benkowitz et al. (2004) (B04). Figures 1 is identical, to B04's Fig.1, and Figure 2 almost identical to B04's Fig.4. Why give an Appendix with model description here when the model is described in B04? Table A1 here is modified from Table 1 of B04, but I actually found the B04 version clearer. The nomenclature used here with NA, Eu, etc., is explained in the text anyway. I would simply remove this material, unless it really is adding something new?
3. It would help very much if the symbols in Table A1 were replaced by proper numbers - giving the various budget terms. Although colour maps are illustrative, it also helps the reader to have Tables with numbers to refer to.
4. p. 4028, lines 5-6. Indicate the length of the time-period over which the measurement comparison was done.
5. p. 4028, lines 10-11. The text here states that a substantial fraction of model observation differences was due to subgrid and/or measurement error. Was there

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- no sign of substantial model error? No indication of problems with the model?
6. The model seems to be using 'labelled' S atoms to keep track of different source regions. Is this right? - I didn't find it explicitly stated.
 7. The 2-letter abbreviation for Europe (Eu) will be easily mistaken for the European Union (EU). I would suggest Eur instead. And why not Asia instead of As? Why save 2-letters?
 8. p. 4030. The discussion of meteorology is limited to just one week which the authors state is representative. Still, the only figure of sulfate burden is presented for 29th June, while the meteorology is presented for 23-27th.
 9. 3-D trajectories would have been more instructive to understand the transport patterns, preferably in association with some of the sulfate burden figures shown later in the manuscript.
 10. Summary and implications. This text does not mention the length of the simulation period.
 11. As well as a discussion of the limitations of a 1-month analysis, I miss here a discussion of the robustness of the results. How do these model results fit into those of other global S models, for example of the eleven models of the COSAM exercise (Roelofs et al., Tellus, 53B, 673-694, 2001)? This paper showed that current global models differ strongly in a number of features. What implications do such studies have for the conclusions presented in this paper?

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 4023, 2006.