Atmospheric Chemistry and Physics Discussions

Interactive Comment

Interactive comment on "Attribution of Modeled Atmospheric Sulfate and SO$_2$ in the Northern Hemisphere for June–July 1997" by C. M. Benkovitz et al.

Anonymous Referee #1

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The article by Benkovitz et al. is an interesting paper that describes the relative importance of different source regions and different sulfate production mechanisms on the sulfate burden at various locations across the northern hemisphere. The tagging method to determine source regions and production mechanisms is a powerful way of analyzing the importance of these factors. I like this article because of the in-depth analysis that includes meteorological aspects. Plus it enlightens the reader as to possible strategies to address visibility problems at specific locations.

The paper is well written and is nearly ready for publication. Some specific questions for discussion follow.

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1. Page 4032, section 4: The authors choose 3 locations, Seattle, WA, USA, Sagres, Portugal, and Barbados, to study. While these 3 locations do exhibit influence of sulfate from different source regions, I am surprised that a location in Asia exhibiting influences from both Europe and Asia was not chosen. Examination of Figure 9 suggests that European sources are 10-20% of the sulfate column burden in eastern Asia. Do the authors think there is substantial European influence in Asia?

2. Are the locations chosen for this study "extreme" examples, or are they representative for their region (i.e. is Seattle representative of the North American west coast, Sagres representative of the west coast of Europe, and Barbados representative of the Caribbean)?

3. Figure 9. It would be helpful to zoom into the region of interest (continental scale instead of global scale). The printed, ACP formatted version of this article results in fairly small panels making it difficult to see a plume reaching Seattle.

4. Page 4036, attribution to production mechanism, and Abstract, line 18. The authors conclude for regions with infrequent clouds, e.g. deserts, that gas-phase oxidation can be dominant. This seems perfectly logical and perhaps obvious. I'm not sure why it is one of the major conclusions of the paper (i.e. why it made the abstract). There are only 2 major production mechanisms for sulfate. If one pathway is suppressed, than the other pathway would dominate. What makes this conclusion interesting and unique?

5. Referee #2 is concerned that the 4 week simulation period during June-July 1997 presents limitations in generalizing the results. While s/he makes a good point, I would argue that detailed analysis of specific time periods should be carried out. It must be recognized that these authors have done similar studies for other time periods (e.g. October-November) and a collection of these detailed analyses benefits our understanding of sulfate concentrations in the atmosphere.

What's missing in this paper is 1) justification of the model configuration and integration

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method (i.e., why simulate June-July, 1997) and 2) discussion of how the current results compare to global models, especially to Rasch et al. (2000) and Barth et al. (2000) who similarly discuss the attribution of sources to sulfate.

Technical Details:

Abstract, line 17. It seems appropriate to round up to 62%.

p. 4026, line 14, state that the same, or similar, model is used in the current study.

p. 4027, line 5, lighting should be lightning

p. 4037, lines 18-29. This is a summary paragraph for section 5 presumably, but is actually summarizing sections 4 and 5. It seems to be in an awkward place.

Figure 5, the panels need to be marked a, b, c, d, e, f.

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