Atmos. Chem. Phys. Discuss., 5, S2570–S2573, 2005
www.atmos-chem-phys.org/acpd/5/S2570/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Transport and mixing zone of desert dust and sulphate over Tropical Africa and the Atlantic Ocean region" *by* K. V. Desboeufs and G. Cautenet

Anonymous Referee #2

Received and published: 12 September 2005

Review of "Transport and mixing zone of desert dustĚ" by K. Desboeufs and G. Cautenet

General comment: This paper addresses an important question in the study of aerosol interactions and the effects of dust aerosol: the mixing between dust and sulphate aerosol, which could affect the hygroscopicity and trace metal solubility of the dust particles. The authors use a regional model to investigate the mixing processes, focussing on Saharan dust crossing the Atlantic for a specific month.

Some major issues need to be addressed before this paper should be accepted:

1. The authors do not make it clear why they choose using a regional model rather than a global model for this study. The higher resolution of the regional model is probably of advantage compared to a global study, but this is not shown here. In contrast, with the limited area of the regional model it is not clear if all sources, from which aerosols are transported into the model region, are taken into account. This is less a problem for dust than for the sulphur species: The authors need to clarify if either sulphate fluxes from middle European and American sources across the region borders are in some way prescribed, or show that those sources do not play a role for the model region.

2. It would be useful to show get a larger context of the results: e.g. to compare the presented January results with results from a spring and a summer month, when the transport pathways may be quite different, and the dust is transported within higher atmospheric layers- the mixing with sulphates may then be quite different.

3. Even if biomass burning is a weak source of SO2 compared to global anthropogenic emissions, it may still play a role in the Sahel, as in particular in the January biomass burning is strong and even minor sulphur sources may influence dust aerosols. Also, is an oceanic DMS source contributing to north Atlantic aerosols? Even if a minor source, it may still affect the dust aerosols in this region.

4. The results of the sulphate concentrations should be compared with observations in a table as was done in Table 1 for the dust aerosol results. Please discuss the validity of the assumption of immediate conversion of SO2 to SO4 - there are quite a lot of model results from global models that could be used to qualify this assumption for the region investigated here.

5. In the discussion of the results the possible role of mixing of dust particles with organic aerosols should be mentioned, in particular as in January the dust from the Sahel and Southern Sahara would be mixed with the biomass burning smoke.

6. In the description of the modelled dust events in January 1993, the authors mention that comparison with the Meteosat IDDI was done, but do not show any results -

ACPD

5, S2570–S2573, 2005

this should be added in the paper, possibly as additional figure to show that the model successfully simulates those events when compared with observations. Also, for this month retrievals of the Absorbing Aerosol Index from the TOMS instrument are available, those should also be used to clarify if the model predicts the patterns of dust emission and transport correctly.

7. A useful aspect of this study is that the authors focus on the calcite content of the dust, the component of the dust aerosol that would be most important for reactions with sulphate aerosol. However here is also a major criticism of this work: The information on dust calcite content is taken only from the study of Claquin et al., 1999. While that study can be regarded as a starting point for this type of investigation, it is not made clear if the soil mineralogical composition would also be reflected in the composition of lofted dust aerosol particles. Also, it is known that by far the most of the Saharan dust is emitted from preferential hot spot sources like the Bodele depression, which may have a specific mineral composition that is not reflected in the Claquin study. The model results should be compared with mineralogical measurements of airborne dust particles to show if the modelled composition is at al realistic. As this is animportant factor in the results, the dust mineralogy requires considerably more attention in this paper.

8. Figure 8 is unclear. What is the definition of dust affected by sulphate? Is it dust mixed with some sulphate molecules, or does it describe dust particles totally coated by sulphate? Clarification is needed here. Also, it would be useful to show a percentage of mixed vs. clean dust, however this is defined. Similarly, it would be educational to show a map of the percentage calcite in the airborne dust, as this information from the model could then be compared directly with any results from mineralogical analysis in specific places.

9. I do not understand the claim (page 5628, lines 9-10) that the coated sulphate concentrations at Barbados are 10 times lower than Li-Jones et al 98 values, please clarify the text here. Which are the observations indicating external mixing between

ACPD

5, S2570–S2573, 2005

dust and sulphate that are mentioned later in this paragraph?

10. Minor technical comments: #

a. Page 5622, line 25: “modelled dust concentrations” should replace “modelled dust loadings”

b. Page 5629, line 9: “SO4” should replace “SO2”

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 5615, 2005.