Atmos. Chem. Phys. Discuss., 8, S4972–S4974, 2008
www.atmos-chem-phys-discuss.net/8/S4972/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on “Sensitivity of aerosol concentrations and cloud properties to nucleation and secondary organic distribution in ECHAM5-HAM global circulation model” *by* R. Makkonen et al.

Anonymous Referee #3

Received and published: 17 July 2008

This paper provides some interesting sensitivity analyses concerning the importance of various parameters and algorithms for new nucleation and SOA distributions. Although the paper is fairly well written, and open about some the simplifications used, I have a number of comments which should be addressed before this paper should be accepted for publication:

In general, I miss a discussion of the physical/chemical interactions of organics and inorganic aerosols - this has been widely discussed in the literature but isn't mentioned here. The treatment of BSOA in this paper is very simplified (more below), but the

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implications of potential interactions and complexities should not be forgotten.

Also, I think the authors should add one extra run with no nucleation or BSOA formation, to see how the emissions alone affect aerosol number. When looking at Table 2 I see a fairly good agreement of particle numbers with observations, but wonder how much this is affected by the assumptions concerning emissions.

p10959 - section 2.1.

- The assumptions in going from mass-based emissions to number emissions are important - how is this done?

- Condensation is mentioned here, and elsewhere in the text, but there is hardly any mention of coagulation. This is also an important process affecting number density at small size-ranges - why is it ignored?

p10961 - section 2.3.

- Give units for J, d, etc.

- Line 11, say "average" molecular speed for sulphuric acid p10962 - section 2.3.

- explain why you chose the 800 hPa criteria in method (2)?

p10963 - section 2.4.

- The term "dynamic" BSOA module is very misleading, since this is not at all dynamic if I have understood correctly. The production of SOA is instantaneous, and the apportionment to different size-classes set by conditions in the lowest model layer. There seems to be no dynamics after this initial allocation?

- The discussion here of the formation of BSOA is also very superficial. Terpene or isoprene oxidation products may well take many hours or days to be converted into the compounds responsible for SOA. In addition, a host of processes may result in SOA formation on long time scales (particle-phase reactions of initially volatile species,

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oligomer formation, aqueous production pathways). Although I can accept the argument that this is all so uncertain that a fixed-yield may be sufficient for a sensitivity analysis, the omission of these longer time-scale processes will bias the model's vertical distribution of OC. The discussion here needs strengthening.

- How do the modelled OA concentrations compare with measurements? Although instantaneous yields are said here to be a reasonable assumption, I would like to see how well this assumption matches observations. (The Stier et al. paper showed rather poor agreement for PCM mass - has this improved?)

- Mention that the yields are given in Table 1.

p10967 - section 3.2.

- Wrong equation numbering - there is no eqn. 4.

p10968 - section 3.3.

- The authors should explain why BSOA favours the soluble mode - which characteristic drives this preference?

p10975 and Fig. 11 - include the Bennartz results in this paper also. This makes it much easier for the reader- section 3.3.

p10992, Fig. 5 - Why is this flux and not mass?

Some references are missing, at least Stier et al (2005), Guenther et al (2005)

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 10955, 2008.