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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.

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We thank the referee for the helpful suggestions and comments. Please see also author's comment "general comment".

1) We have rewritten section 2.4, in which our approach of modeling BSOA formation is described. In the revised text, a detailed discussion on uncertainties involved, together with relevant literary information, has been included.

2) Primary emissions play a significant role in number concentrations of certain locations in Table 2. However, experiment B already gives quite similar result to an exper-

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iment without nucleation, since binary nucleation rates are generally rather low in the boundary layer.

3) The issue of mass to number based emissions is discussed in paper on ECHAM5-HAM (Stier et al., 2005). We did not want to include details on M7 microphysics on this article; hence the reader is referred to Vignati et al. (2004), which includes a description of aerosol model dynamics. A sentence is now included to list microphysical processes of M7.

4) Missing units were included and word ”average” was added. Since boundary layer height was not a diagnostic variable in ECHAM5-HAM, we decided to use fixed 800 hPa criteria for the boundary layer limit, since this value was already used by other submodels.

5) We have renamed our approach as ”Hybrid BSOA formation scheme”. We have also noted in section 2.4 that using fully dynamic approaches is not feasible at the moment in large-scale modeling frameworks.

6) We fully agree with the referee on the critics on discussion of BSOA. In revised text (section 2.4), we have addressed the points raised by the referee.

7) The main effect of our new approach, as compared with the original ECHAM5-HAM, is to re-distribute the secondary organic aerosol more realistically over the particle size spectrum. Changes in the total amount of OA are not very sensitive to this redistribution. The ideal way to test our approach would be to compare the predicted size distributions of SOA (or OA) with observations. Unfortunately, the available observational data on OA, and especially SOA, size distributions is almost non-existent at the moment. As a result, we can only make comparisons between other quantities affected by the SOA size distribution, such as CCN done here.

8) Equation numbering was fixed.

9) In our approach, organic vapors condense preferably onto modes having the largest

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surface area (or more accurately, the largest condensation sink). The soluble modes are therefore favored because of their water uptake in the ambient atmosphere (which increases their surface area, but not the surface area of insoluble modes). We have explained this more explicitly in the revised text of section 3.3. In reality, the driving forces behind distributing SOA over the particle size spectrum are more complicated than assumed here (see discussion in the revised text of section 2.4).

10) We have now included satellite data in the paper to make comparison easier, and figure text was rewritten. Text was modified to include also discussions about differences between model and observation.

11) Point of the figure is to compare BSOA aerosol partitioning between hybrid BSOA scheme and original ECHAM5-HAM. Original ECHAM5-HAM uses 1:1 partitioning between accumulation and Aitken modes, and Fig. 5 shows how different results hybrid BSOA scheme produces on the average.

12) Reference list was revised, and missing references were added.

References:

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Vignati, E., Wilson, J., and Stier, P.: M7: An efficient size-resolved aerosol microphysics module for large-scale aerosol transport models, J. Geophys. Res., 109, D22202, doi:10.1029/2003JD004485, 2004.

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