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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Secondary organic aerosol in the global aerosol – chemistry transport model Oslo CTM2" *by* C. R. Hoyle et al.

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We thank the reviewer for their suggestions which improved this manuscript. The comments are addressed below.

1. The inaccurate wording has been tidied up. Aerosols are now described as "partitioning into" rather than "condensing on".

2.We have added the following discussion to section 3: "Ammonium sulphate aerosol has been used in many smog chamber experiments as a seed aerosol Odum1997,Kroll2007,Northcross2007,Kleindienst1999,Griffin1999a. It was found by Kleindienst1999 that even with relatively high concentrations of ammonium sulphate

aerosol, the partitioning model of Odum1997 described the SOA yield satisfactorily. Therefore it is possible that the dry mass of ammonium sulphate aerosol can not simply be added to the existing organic aerosol in order to calculate the SOA partitioning. Nevertheless, doing so provides a useful sensitivity experiment. A study by Edney2000 suggests that the amount of water in the ammonium sulphate aerosol does not affect yields of SOA formed from toluene oxidation products. In this study, liquid water in ammonium sulphate aerosols has been assumed not to affect the partitioning of any semi-volatile organic species."

3. The following text has been added to section 2.4: "It should be noted that while POA is treated as non-volatile in this study, very recent research has indicated that this may not be strictly true. As emissions are diluted away from their sources, species with relatively high effective saturation concentrations may evaporate, in addition, organic species may undergo oxidation reactions leading to more volatile POA species Shrivastava2006,Donahue2006,Robinson2007".

4. We have performed a sensitivity study by running the model with an exponential hydrophobic to hydrophilic aging time of 1.15 days. This was found however, only to have a minor effect on the burden of SOA (a reduction from 0.7 Tg to 0.67 Tg). These results are now included in the manuscript.

5. Unfortunately the deposition of SOA in the model is not dependent on surface roughness, and perhaps this should be changed in the future. Model meteorology does play some role in the deposition though, as deposition occurs in the lowest model layer, and the concentration of SOA in this layer is dependent upon the mixing in and out of this level. As SOA loss is greatly dominated by wet deposition processes, we do not believe that the effects of surface type will greatly affect the results.

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6. We have changed “GISS CTM” to GISS GCM II’ .

7. The description of the various species has been improved in this table, we agree, it was rather confusing.

8. We have now included plots of measured data vs modelled data, as suggested, however we have not differentiated between SOA and OM on these plots. This aspect is well described by the annual means in the tables, we feel. These new plots help to illustrate that although the model generally underestimates the measured values, there are many days in the case of the IMPROVE data, where the model reproduces the measured values well.

9. This statement has been removed.

10. The measurements are from different regions, and as discussed in the text, the modelled values at the Crete site are likely influenced by mainland pollution. It is normal that the model will underestimate values in some regions, while overestimating in others, and this appears to indicate the absence of a systematic bias in the model. Unfortunately it is difficult to perform a through global validation of NO_3 as a function of altitude, because of the lack of suitable measurements.

11. This sentence has been altered. It now reads: “ These results suggest that there may be unaccounted for sources of POA, or SOA precursors, although a too-rapid removal of OA from the model atmosphere as well as uncertainty in the SOA yields from the oxidation of precursors may also contribute.”

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