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Atmospheric
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Discussions

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

Anonymous Referee #2

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This manuscript describes the inclusion of a SOA module in the Oslo CTM2 chemistry transport model, and the results calculated are compared with both total bulk organic aerosol measurements and previous modeling studies. The topic of research is very interesting, the improvement of the Oslo CTM2 is substantial now that SOA are included, but the paper lacks originality, since almost all results have been reproduced before. The approach used is a carbon-copy of that of Chung and Seinfeld (2002), and although this is not blameworthy, the authors should had gone a step further, rather than staying to the same (but very interesting) global chemical production, global burden and lifetime. For example, they should try to search deeper why their model calculates so much SOA compared to Chung and Seinfeld (2002). Theys could also try to compare their model with high altitude organic aerosol measurements, to see if they perform better in simulating total OA, since their chemical production and burden are at the high end of the today's proposed range. Further, they could try to compare their SOA results with SOA (and not bulk OA) measurements that had become available in some measuring stations throughout the world.

Having said that, I find that the paper is interesting and the results presented are valid, but due to lack of originality I find the manuscript non-publishable before a major revision. I also present below some points that should be taken into account prior publication.

1) page 9056, line 6: reference should be replaced by: Tsigaridis and Kanakidou (2003); Lack et al., 2004

2) page 9056, lines 13-18: The fact that NO3 oxidise the VOC does not necessarily mean that semi-volatile compounds are being produced.

3) page 9062, lines 20-21: Depositing insoluble aerosols with the same rate on continents and oceans is a major approximation that should be improved. How do you treat deposition of gas-phase species?

4) page 9063, line 26: inclusion of isoprene _and aromatics_ as a SOA precursor.

5) page 9066, lines 21-22: This is not a new finding, since it was mentioned also in Tsigaridis and Kanakidou (2003).

6) page 9066, line 27: Since the authors appear to have a finer temporal resolution in the data, why averaging over the whole year and lose important and potentially interesting information?

7) page 9067, lines 26-29: These sites should be identified and possibly be excluded from the analysis, since they can be misleading.

8) page 9068, lines 5-7: There might also be unknown SOA precursors.

9) page 9068, lines 8-11: The authors should include a reference for that comparison,

or present it in the manuscript, since they identify wet deposition as a major source of disagreement between models.

10) page 9068, lines 13-14: This is because of the very high yield. I do not believe that there is evidence to lump everything on b-pinene yield, even if Chung and Seinfeld (2002) did that. There is more work on SOA yields from the NO3 reaction, for example Spittler et al. (2006) in atmospheric environment page S116, that find lower yields with comparable partitioning coefficients.

11) page 9069, lines 20-28: Mentioning that you do not model very well NO3 but NO2 is good is an evidence against the model's gas-phase chemistry. One should pay extra attention in comparing the NO3 radical point measurements with the average model results in a large model grid.

12) page 9070, section 4.2: The fraction looks too high. You have no terpene emissions during the night, when there is the maximum of NO3 radical concentrations, and still 60% of SOA is produced by the VOC+NO3 reaction? My feeling is that the NO3 yield used is way too high.

13) page 9070, line 26: either POA or precursors of SOA _or both_.

14) All tables: Footnotes should be placed under the tables and not to the table description, to be easier to find.

15) Table 2: POA emissions are once in TgC and once in Tg, what OM/OC ratio is used?

16) Table 3: The use of the same E/R value for all classes can introduce errors, since based on Martinez et al. (1998, Environ. Sci. Tech., 3730) the E/R for the reaction APIN+NO3 is 841 and for BPIN+NO3 is -1248. This means that within the same hydrocarbon class (class I) that both a-pinene and b-pinene exist, the reaction rate due to temperature changes should be different.

17) Table 4: If the authors have ignored the isoprene+O3 reaction is clearly wrong,

since based on calculations of Tsigaridis and Kanakidou (2007, Atmos. Environ., 4682) O3 consumes 16% of total isoprene, globally and annually averaged. Maybe the authors mean that they neglect the production of SOA from this reaction?

18) Table 6: What does the footnote a mean?

19) Table 9, Lahore: This is way too high for a representative regional station, maybe it is an urban station that should be excluded?

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 9053, 2007.