

Interactive comment on “The AeroCom evaluation and intercomparison of organic aerosol in global models” by K. Tsigaridis et al.

Anonymous Referee #1

Received and published: 18 May 2014

This manuscript describes an AeroCom intercomparison of 31 global model organic aerosol simulations. The study documents the state of modeling in this area and highlights the diversity of simulation approaches and global budgets. I have two major comments, followed by more minor comments or suggestions.

Given that few of these particular models include the marine and biological sources of OA, their inclusion in the general comparisons seems misleading and obscures our ability to compare the model treatments of OA. Presuming that the models which include these sources can separate these contributions, I would suggest that the authors present these as their own separate OA type, not part of the standard POA or OA budget. The discussion of these sources could also be substantially reduced throughout (or moved to the Supplement).

C2485

I would strongly suggest that the authors considerably trim the text and move extraneous results to the Supplement. As is, the manuscript is quite lengthy and challenging to read. I highlight below some specific suggestions to reduce the length of the manuscript.

1. There are a number of sections which are unnecessarily detailed and do not provide substantial new scientific insights.

a. I suggest that the authors merge and trim 4.1.4 and 4.1.5 on deposition/lifetime which are somewhat repetitive.

b. Section 4.1.7 (comparison with AeroCom phase I) does not provide any substantial new insights. I suggest trimming this down to one paragraph and including it in the introduction to set up the study.

c. Section 4.3.3: The detailed composition from 5 models is not particularly informative and is a selective analysis of 31 models which are the main focus. I suggest that this discussion be moved to the Supplement.

d. Section 5 repeats many of the result discussed in the main text, I suggest that a briefer summary would be more appropriate for a conclusions section.

2. There are far too many figures in the main text. Here are my suggestions to eliminate/merge figures:

a. Figure 1: difficult to see, move to supplement

b. All Figures 2-12: the bottom two panels are repetitive/unnecessary. I would suggest removing all of these and then merging figures as follows: Figure 2+3, Figure 4+5+6, Figure 7+8, Figure 9+10+11

c. Figure 17-21: show a lot of (redundant) information not discussed in the text. I suggest showing simply the MNB panels and moving the others to the Supplement. All the MNB plots could be merged into one overall figure.

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d. Figures 25-37: move to Supplement

MINOR

1. Page 6031, lines 13-14: how can a “modeled vertical distribution” show “diversity of over an order of magnitude”? Do the authors mean the concentrations vary by over an order of magnitude? Or somehow that the shape (i.e. the vertical distribution) varies between models?
2. Page 6031, line 27: typo “ratio is calculated.”
3. Page 6032, line 16: spatial or temporal correlations?
4. Page 6032, line 20-21: what do the authors mean by “processes”? examples or more precise language would help clarify.
5. Page 6033, line 10 vs line 12-13: Please be consistent in reporting either the means or the range.
6. Section 1.1: It would be helpful to fold in the definitions of Section 1.6 and 2.3 into this section such that one section provides a coherent overall discussion of terminology. For example, Table 4 precedes the descriptions of Section 1.6 and is therefore confusing.
7. Page 6034, line 26: what is an OA “component”? Do the authors mean a specific composition type? Or OA from different sources? Or...?
8. Page 6036, line 5: “improve” suggests that the model descriptions of these properties are currently in error. Is there specific evidence of that? Perhaps “investigate” would be a better word.
9. Page 6036, line 9: “mostly neglected” oversteps. Many models do include anthropogenic SOA
10. Page 6038, line 7: typo “oxidation to the”

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11. Page 6039, line 9: Volkamer et al. is a not “a review”, and they show that the underestimate of SOA by models increases as a function of photochemical age, not “long-range transport”
12. Table 1, 2, and 3: add the emissions totals for each model
13. Table 5: It would be helpful to indicate which models include IVOC (such as GISS-CMU-VBS as described on page 6048) in this table.
14. Section 2: please also include the enthalpy of vaporization used for the SOA models in a table.
15. Section 2.2: In each section it would be useful if the author provided ranges of emissions.
16. Page 6048, line 8: replace “alpha” with symbol
17. Page 6048: the description of ntrSOA would generally benefit from some numbers so that the reader gets a sense of the importance of these sources.
18. Section 3: include years for the observations listed here
19. Page 6053, line 24: typo “of AMS OA measurements has”
20. Page 6054, line 22: why is the cutoff diameter of aerosols not expected to be a significant issue? Particularly given that the AMS OA size cut-off is often considerably less than 1 μm , it seems that different measurements could certainly represent varying degrees of fine particle mass closure.
21. Page 6056, line 18-19: why talk about CAM5-MAM3 here when it’s listed as a model with SV SOA in the tables?
22. Page 6056, line 18: why are these two models listed as outliers? They do not appear to be the most extreme models from the figures?
23. Page 6057, lines 9-10: reference for this statement?

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24. Page 6059, lines 15-21: why is this not seen in CCSM4 which is also driven by GEOS-5 meteorology?

25. Page 6060, line 14: isoprene SOA is also typically a new source not included in AeroCom phase I

26. Figure 15: Here a single figure seems inadequate. The regional details described in the text are not apparent from the global means. I suggest that the authors show mean vertical profiles for the tropics, mid latitudes and biomass burning regions discussed in the text.

27. Page 6071, line 5 and page 6082, line 6 and page 6088, line 28: While it's interesting to hear that the authors plan to follow up with future studies, text to this effect should be removed from the manuscript unless follow-on manuscripts have already been prepared for submission.

28. Page 6072, line 20: I suggest that "less bias" would be clearer than "increased MNB"

29. Section 4.3.1: These comparisons raise the question of why models differ significantly in skill when simulating OA vs. OC (presumably the answer is that the OA:OC applied to the model simulations is not very well constrained). The discussion of this in section 4.3.3. should be integrated with the discussion here.

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