

Interactive  
Comment

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

**Anonymous Referee #2**

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This paper presents a comparison of global organic aerosol models that participated in the AEROCOM intercomparison. Models in the study range from relatively simple with nonvolatile POA and SOA calculated based on a fixed yield to volatility basis set treatments with semivolatile POA and non-traditional SOA. Some of the main conclusions of the paper include:

1. Global organic aerosol models are growing in diversity
2. Models generally underestimate organic aerosol
3. Models are likely missing an anthropogenic OA source
4. Increasing model complexity does not necessarily lead to better performance

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5. There is a need to understand not just the sources of OA, but the processes by which sources form OA

Overall, a comparison of this type is definitely worthy of publication in ACP. Some changes can be made to improve clarity and make the messages more concise for future readers.

Major comments

Some of the aerosol classification categories (traditional POA, traditional SOA, marine POA) do not seem mutually exclusive and it is unclear where certain mass resides. The authors should focus on how their paper presents the quantities (not just how the models label them). Specifically, according to the authors some models include a non-volatile SOA source as part of traditional POA (tPOA). This occurs in models that have a direct yield of SOA from monoterpenes. Wouldn't it be better to label this direct yield distinct from POA? Even though those models form aerosol directly from VOC emissions, isn't it more correct to term that aerosol "effective SOA" than "POA?" It is a little unclear if this effective SOA is included in the POA emissions of Figure 2b or Fig 3b. Also, a figure similar to Figure 2, but including the sum of all POA and SOA (figures 2 and 3 combined) would help more evenly compare models where POA may include SOA. Furthermore, dividing modeled OA into HOA and OOA (page 6050) seems challenging if nonvolatile SOA is part of tPOA.

The classification of MSA is also a bit confusing since page 6049 indicates MSA is included in tPOA for CAM4-Oslo but exists as a separate mPOA which is a subset of tPOA in IMPACT. It seems as though this would lead to double counting in:  $OA = tPOA + mPOA + trSOA + ntrSOA + MSA$

Overall, the paper could be shortened by moving some information to supplemental information. For example, site descriptions on page 6085 could be moved.

Minor comments

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1. Would it be better to present MNB as percentages instead of fractions? That way they would have “units” of percent in the text so as not to be confused with  $\mu\text{g}/\text{m}^3$ . This is a stylistic choice so the author may leave as-is.
2. The last sentence of the abstract emphasizes the need to separate natural and anthropogenic OA to determine climate impacts. Is this driven by differences in their spatial distribution or properties of anthropogenic vs. natural OA?
3. Add one sentence to the introduction (section 1) giving a brief overview of the AE-ROCOM study design (such as year targeted, specific emissions or user choice, etc)
4. Page 6033, line 19, plus/minus needs formatting
5. Page 6034, line 1-2: Expand the definition of SOA. What about aqueous reactions? Heterogeneous reactions? There are more SOA sources than condensation of semivolatile vapors.
6. Page 6036, line 9: consider rephrasing the comment that anthropogenic VOC sources of SOA are “mostly neglected by global models” since page 6046 line 26 indicates a few models consider them.
7. Page 6038, check year in text and reference of Hodzic et al. 2014
8. Sections 1.1-1.5 are AMS and current-model centric. What about a slightly more general view highlighting some emerging issues (IEPOX uptake?) that the inter-comparison may be able to provide evidence/for against? Can provide support for/against?
9. Page 6044, line 1: reword “contribute by up to”
10. Can section 2.2 more closely parallel table 2? There are five classes in section 2.2 and 2 columns in the table.

11. Page 6052, what year of the datasets are used?
12. Page 6053, line 29: See Budisulistiorini et al. AMTD <http://www.atmos-meas-tech-discuss.net/6/11181/2013/amtd-6-11181-2013.html> for possible ACSM biases. The RIE for organics is not well characterized which could result in unrealistic OM levels (OM/OC ratios above 4).
13. Page 6055: Line 25-26, awkward sentence
14. Page 6056, line 20: why was SOA production scaled in some models? To match observations?
15. Page 6062: Line 17, does disagreement with the mean necessarily imply the dry deposition is too low or is there another reason?
16. Page 6068, line 3, remove OC from sentence since units are for OA.
17. Page 6078, line 20: Could increased SOA during the summer result from an enhanced anthropogenic source rather than biogenic SOA?
18. Page 6079, line 10-11: Can you comment on whether or not treating POA as SVOCs (thus making them temperature sensitive) could be a factor for higher winter OA levels? Figure 25 (VBS) indicates it doesn't play a role.
19. Is there a model development priority list that could be added to the conclusions? Either for processes that are significant but not widely implemented in models or that lead to large differences between models?
20. Table 1: Can you include a version number or release/freeze date for each model since it is unclear if later improvements to the models were included but not referenced

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21. Table 2, add another column for sources other than fossil fuel, biofuel, and biomass burning so that additional sources are not buried in the comments column
22. Table 4, page 6121: both the term terpenes and monoterpenes are used. If the term terpene is used only to refer to monoterpenes, use that term instead.
23. Figure 4, can you distinguish missing data from zero values? I assume all zeros in Figure 4b are missing data?
24. Figure 2b, Is the GISS-CMU-VBS POA emission rate before or after partitioning? Doesn't most of the POA evaporate?

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