

Interactive
Comment

Interactive comment on “A global perspective on aerosol from low-volatility organic compounds” by H. O. T. Pye and J. H. Seinfeld

Anonymous Referee #2

Received and published: 8 April 2010

The manuscript is a modeling study which goes beyond the traditional organic aerosol (OA) representation in global models, that of primary non-volatile and secondary semi-volatile OA. The authors have introduced additional OA precursors and made primary OA semi-volatile, in line with recent findings. The approach used is supported by the very few experimental data available and has been validated against the previous “traditional” OA simulation. Sensitivity studies have been performed, in an attempt to both understand and quantify the differences between the new and old model approach, and identify on which parameters the model’s results are more sensitive to.

This is a very well written paper with detailed and clear analyses and explanations of results. I suggest its publication in ACPD, after taking into account the following minor points.

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General comments

1) The authors try to explain their results based on wintertime OA calculations over the USA, claiming that the absence of biogenic SOA makes the results easier to understand. Although this is true, summertime OA are also very interesting to study. The conclusions extracted from the wintertime discussion (present in several places in the manuscript) can be used in a discussion of the summertime OA, with special attention to the biogenic SOA.

2) Most of the comparisons are being made using the traditional simulation as a basis. The reason given is that between the traditional and the revised simulations, the former was the one that compared the best with measurements (figure 7). Nevertheless, the traditional simulation is just another model result. The comparisons of the sensitivities should be made with the observations themselves, and not with the traditional simulation, which might have a better agreement with measurements, but is not the reality. In order to conclude that the 2*SVOC simulation is the best, this has to be extracted from its comparison against measurements, not against the traditional simulation.

3) In comparing simulations with measurements, some statistical analysis is necessary. The only part where model-measurements comparison was made is figure 7. In order to have comment #2 answered, a quantitative analysis is required, that is completely absent from the manuscript.

4) A brief discussion on how the results are expected to change if alkanes were used as a surrogate for IVOC instead of naphthalene would be useful, especially concerning the spatial distribution, the seasonality of emissions (if any) and the modern vs. fossil carbon discussion. My main concern is how the conclusion in page 4106, lines 24-25 would change.

Specific comments

5) Page 4091, line 11: Can you support this, as you did in the previous paragraph for

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the total emissions?

6) Page 4094, line 20: What kind of identity should be preserved, but is being lost? Which is the “artificial migration” you are referring to?

7) Page 4095, lines 1-3: More information on the lumping is needed.

8) Page 4099, line 1: Can you estimate an e-folding lifetime for the revised simulation?

9) Page 4104, middle paragraph: More numbers are needed in this discussion.

10) Page 4106, lines 27-29: Why?

11) Page 4108, lines 4-6: This applies to all semi-volatile compounds, not just IVOC.

12) Page 4108, lines 10-15: Robinson et al. (2007) is based on Donahue et al. (2006) that takes into account fragmentation, by assuming that 10% of the oxidized material from any volatility bin will produce compounds with $C^* = 1e6 \text{ ug/m}^3$ (see Donahue et al. (2006) and the supporting material, matrix A).

13) Page 4110, line 9: Studying only two years does not give concrete results for the interannual variability. Now different is 2000 from 2001? Maybe including this comparison in the Appendix will provide this information.

14) Page 4110, lines 18-20: Since the results from different years, different meteorologies and different years are very similar, how can this conclusion be extracted? Why not a certain tuning for, say, year 2000, will not work for the year 2001?

15) Figure 6, top-left panel (DJF) and figure 7 bottom panel should be the same, but the colorscale has a factor of two difference. Is this just a mistake?

Technical comments

16) The first paragraph of the abstract has too many parentheses, making it hard to follow.

17) Units throughout the manuscript should be uniform, as much as possible. Using

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both TgC and Tg is not always necessary and is often confusing.

18) Page 4084, line 18: “which in the” should be “which is the”.

19) Table 6, Kom columns: saturation concentration should be used, or discuss Kom in page 4102.

20) Figure 1 should have SI units (m⁻² instead of cm⁻²).

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 4079, 2010.

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