

Interactive comment on “Development of a secondary organic aerosol formation mechanism: comparison with smog chamber experiments and atmospheric measurements” by L. E. Olcese et al.

Anonymous Referee #3

Received and published: 13 July 2007

This paper is dealing with the modeling of secondary organic aerosol formation in the troposphere that is a scientific topic that attracts a lot of attention in recent years. Therefore it is relevant to ACP and would deserve publication only after addressing a number of points that will clarify the applied procedure, support and justify the results. In addition to the detailed comments of the other referee, I would like to strengthen the need for the following clarifications/justifications

1. Why do the authors adopt the non evaporative approach? The fact that it fits better the results is not a justification. Actual knowledge shows that oligomerisation and heterogeneous reactions can push deviating from the gas to particle equilibrium of the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

pure SOA component by lowering the vapour pressures of the aerosol phase SOA components. Adopting a non evaporative approach does not account for molecular mass changes that might be related to those processes and should be only viewed as a sensitivity test and not as a base case simulation.

2. As a consequence, the authors have to also shown the results with the equilibrium approach and plot them in all figures of the paper together with the non-evaporative approach results. At the end it might turn out that the model is not performing as well as mentioned in the paper if a more rigorous comparison procedure is followed.

3. In addition, the authors claim (page 8373, line 3) that their results show reasonable agreement with observations without any adjustment to the partitioning coefficients derived from theory. If a non evaporative approach is used - this is a major adjustment!

4. Another area of large uncertainties in the calculations is the removal rate of the aerosols (page 8374, line 5). The authors provide an aerosol removal time constant of 7 days and a boundary layer height of 5000 m. How is the estimate of 7 days derived? For a global mean removal rate that in the case of aerosols is mainly due to wet removal processes this number is reasonable but what about the specific experiment? Can the authors provide information on the specific conditions that govern the area in March 1st? In addition the boundary layer looks huge, might be a typo? Mixing in such a high continental boundary layer (constant day and night?) could minimize the effect of dry deposition. More details and thorough discussion are needed at this point.

5. page 8374, line13: The difference of 10% between the continuous equilibrium assumption and the non evaporative approach looks too small. Again reporting both simulations in the figures would provide a better view to the reader of the importance of this assumption.

6. page 8374, line 23: Here the authors state that biogenics account for about 90% of the total SOA mass whereas in the conclusion this number is approximately 60% (page 8377, line 22).

7. page 8378, line 6 to 15: this is a rather general statement and definitely not a conclusion, should be earlier in the introduction or the main core of the paper.
8. page 8378, lines 16-21: What about a remote marine site? What about Mexico city? A few more case studies could support the validity of the modeling approach.
9. page 8378, line 22-25: How do the authors extrapolate for remote site cases? This part is not rigorous, concrete procedure and numbers have to be provided or the sentence has to be removed as very speculative.
10. page 8362, line 9: what is here stated as an 'average error' is mainly the deviation of model results from the observations.
11. page 8378, line 27: Tsigaridis et al (Atmos. Chem. Phys., 6, 5143-5162, 2006) considering that all oxidants can contribute to SOA formation with the ozonolysis reported yields have estimated only a 16% increase in the global annual biogenic SOA leading to a 29% increase to the corresponding burden. On the other hand, Hoyle et al (open discussion paper, Atmos. Chem. Phys. Discuss., 7, 9053-9092, 2007) estimate that NO₃ reactions account for 50%-60% of the total amount of SOA near the surface in polluted regions and less than 25% in more remote areas. In both studies the global estimates are far below the 235 Tg/y given as upper limit in the present study.
12. Table 1, title of the 2nd column: specify what 'significant' means, provide numbers.
13. Table 2, provide ozone levels also.
14. page 8363, line 15: and different SOA precursor molecules speciation.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 8361, 2007.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)