

Interactive comment on “The influence of semi-volatile and reactive primary emissions on the abundance and properties of global organic aerosol” by S. H. Jathar et al.

Anonymous Referee #2

Received and published: 4 May 2011

This paper describes the implementation of the volatility basis set framework, as well as the intermediate volatility organic compounds, in the GISS II' model. The authors make a lot of sensitivity studies in order to demonstrate and quantify the uncertainties related to the new methodology and compare their results both with their previous model version that treats primary organic emissions as non-volatile, and the single global model study present in the literature. The manuscript is very well written and analyzes the large uncertainties and degrees of freedom related to the new parameterization in great detail. I suggest publication to ACP, after addressing my minor comments below.

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General comments

- 1) Page 5498, line 2: The explanation of SOA aging that appears in page 5504, lines 20-21 should also appear here.
- 2) Section 2.1.5: The authors made no sensitivity runs for the Henry law selected. This is a major uncertainty, given the rather high value selected (which is common among model studies). Why not selecting a varying value based on volatility? Volatility bins can also describe the degree of oxidation, which should be related to the solubility. This also applies to the choice of 80% scavenging efficiency for all organic particles, regardless of volatility.
- 3) Page 5506, lines 19-20: Why not changing the dH values for SOA formation as well, for consistency with the HVAP scenario?
- 4) Page 5508, line 25: The difference of 0.89 Tg is lower than the one between BASE and HVAP. This means that the assumption of the volatility distribution and aging of IVOC is at least equally important compared to their sources. Nevertheless, HVAP increased burden reflects the POC aging and volatility distribution as well. Can you quantify the contributions of each? Please comment.
- 5) Last paragraph of section 4.1.2: SOA yields come from chamber experiments that do not necessarily capture the multi-day aging that might undergo in the atmosphere. Acknowledging the uncertainties related to introducing such an aging, is there a reason that you expect that SOA will not age like the other organic aerosols in the model? Why not introducing SOA in the volatility bins and allow them to evolve?
- 6) Page 5511, lines 14-20: Maybe too efficient removal of gases can explain this as well? See also comment #2.
- 7) Page 5512, line 6: If you believe this set of heat of vaporization values is more realistic, why not using it in BASE?
- 8) Section 4.2.3: Please mention and comment in the discussion how much traditional

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SOA is present in these locations, since it does not age, which affects the volatility distribution. If there is plenty of traditional SOA, it might change the discussion considerably.

9) Same section: The use of thermograms is an extremely useful technique to validate the OA volatility in global models and it should be highlighted more in the final manuscript.

10) Page 5515, lines 19-21: How does BC burden compares with that of OC in this fraction?

11) End of page 5515: The assumption that traditional SOA do not age affects these results. If it was allowed to age, I would expect that the model would calculate more contemporary OA. Please comment.

Technical corrections

1) Linking in the text is broken. There are places that mention e.g. Fig. refvbssch

2) Page 5513, line 9: "predicts the correct fraction" should be something like "reproduces more accurately the fraction" or something similar. The word "correct" is too strong, given the uncertainties of the modeling approach.

3) Epstein et al reference is from 2010, not 2009.

4) Huffman et al reference is missing the initial H.

5) Table 4 should include Kanakidou et al. 2005, if not some models from that paper, same with the discussion in page 5509, lines 2-3. Chung et al is Chung and Seinfeld.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 5493, 2011.