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## ***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 #1**

Received and published: 4 March 2011

This manuscript describes efforts to include the semi-volatile nature of primary organic aerosol (and IVOCs) into a global model. The authors conducted a series of sensitivity studies and also used a more stringent set of metrics to test their model performance. I particularly appreciated the clarity of the manuscript and the balanced discussion of uncertainties. I have a few of suggestions for the paper (see below), after these are addressed, I recommend that the paper be accepted for publication in ACP.

I felt that the authors did a very nice job of testing some of the key uncertainties with their sensitivity studies. However, I was surprised that this did not include a test of the aging mechanism. They did briefly contrast in the text the approach of Pye and Seinfeld

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(2010), but it would have been informative to see a simulation with no aging included to identify the importance of this mechanism to the total mass of OA simulated. While I hate to suggest that another model simulation is necessary I urge the authors to seriously consider adding this. I think it would really complete the set of simulations performed and add to the citability of the paper as a comprehensive global sensitivity study.

**Terminology:** I have a query here about the comparison of OOA with SOA and HOA with “traditional” POA. Should the comparison not be done with  $OOA=SOA+POA_{hydrophilic}$  and  $HOA=POA_{hydrophobic}$ ? Given that the crude aging mechanism of traditional models moves “aged” POA into the hydrophilic category, it seems sensible to imagine that it is a proxy of aged-POA or would appear OOA-like to an AMS. Thus, your assumptions inaccurately disadvantage “traditional” models in the comparisons. I think this is key in Figure 8 for example. On a related point, throughout the text OOA:OA and SOA:OA are used interchangeably. For clarity I would recommend referring to the observations ONLY as OOA:OA as not all future studies will convene to your definitions.

The model evaluation of isotopic composition inherently is based on the simulation of OC & EC mass concentrations. You clearly need to add an evaluation of simulated EC concentrations to the discussion in Section 4.2.4, particularly in light of your modifications to the relative emissions of EC/OC to correct the isotopic values. A comparison at the IMPROVE sites should be added.

**Minor comments**

1. Abstract, line 21: typo “lied” should be “lie”
2. Introduction, line 8: insert “non-refractory” or remove 20-90% quantifier.
3. Introduction, line 20: Park et al., 2006 – useful to mention where/what obs were used since the conclusion is so different from other studies. Also, could you later bring

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us back to this study in contrast to your results? Why does Park et al., 2006 “traditional” simulation of IMPROVE obs look relatively unbiased?

4. Table 2: list that GFED2 is for 2005 in the caption
5. Page 5503, line 22: typo reference on Figure number
6. Section 2.1.4: Please clarify in the text that your aging scheme does not add additional mass.
7. Page 5505, line 19 & page 5511 line 23: Heald et al. 2005 is not a reference for North American emissions (it is the ACE-Asia manuscript).
8. Page 5505, line 25: Chung and Seinfeld, 2002 and Liao and Seinfeld 2005 used an enthalpy of vaporization of 42 kJ/mol not 30 kJ/mol as cited.
9. Section 4.1.1: re-order figure numbering. Figure 8 is discussed in the text following Figure 5 (before Figures 6 and 7 are introduced)
10. Section 4.2.1, Rest of the World comparisons: suggests that the model has very little skill in reproducing the variability in observed OA! A little depressing after the efforts to include all these additional volatilization/aging mechanisms. How much do you think coarse grid scale could contribute to this?
11. Section 4.2.2: A little strange to re-introduce Figure 8 here when previously discussed in Section 4.1.1. Consolidate the discussion.

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

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