

Interactive comment on “An extended secondary organic aerosol formation model: effect of oxidation aging and implications” by F. Yu

F. Yu

yfq@asrc.cestm.albany.edu

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The author thanks Dr. Napelenok for the thoughtful and constructive comments. My point-to-point replies to the comments are given below (the original comments are copied here in *Italic*). The manuscript will be revised accordingly and submitted to ACP for final publication.

This is an interesting and well written manuscript on an enhancement to the $n \times 2p$ model for SOA formation and its implementation in the GEOS-Chem model. Some comparisons with observations are provided at a boreal site in Finland and several implications for climate forcing calculations are discussed. I have reviewed this manuscript with several colleagues at EPA and we came to a consensus at offering the following questions and recommendations centered mainly on model formulation:

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The author appreciates the constructive comments by Dr. Napelenok and his colleagues at EPA.

The majority of new SOA papers coming out currently are some kind of an extension to existing models, so it may be beneficial to come up with a title that uniquely distinguishes your work from other “extended SOA models.” In the text, perhaps you can build upon Jim Pankow’s $n \times 2p$ nomenclature (e.g., use “ $n \times 2p$ + SOG” aging instead of just the word “extended”).

Agree. Will revise the title to distinguish the work.

When the terms SV-SOG and MV-SOG are introduced in the Abstract, it would be helpful to provide the range of c^ values that these two groups span at 290K.*

Will provide C^* values as suggested.

It may be beneficial to clarify your reasoning on why particle growth should be modeled kinetically rather than thermodynamically (page 19815, line 16).

The thermodynamical equilibrium approach assumes that the secondary organics in the particle phase and gas phase are always in instantaneous equilibrium which is a good approximation for organics with relatively high saturation vapor pressure. Based on equilibrium, the gas concentration of low volatile species (such as H_2SO_4 , LV-SOG defined in the paper) will be very low. In the real atmosphere, it takes time for condensable organics produced in-situ to get into particles and thus substantial concentrations of low volatile condensable species can build up in the atmosphere. The growth of particles (especially those freshly nucleated ultrafine particles) should be modeled kinetically to properly account for the contribution of nucleated particles to CCN.

How appropriate is it to age an Odum 2-product model given that the Odum 2-product model may overpredict the amount of gas-phase SOG? For example, Cappa and Jimenez (2010, ACP) found that the amount of semivolatile gas-phase organic in equilibrium with OA could range from 20corresponding to higher (and perhaps more rea-

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sonable (Epstein 2010 EST)) enthalpies of vaporization.

The parameters used in 2-product model are based on laboratory measurements which generally last for several hours. Aging is known to occur in the atmosphere where secondary organics can float around for days. Actually, the aging reduces the amount of gas-phase SOG.

Donahue's parameterization of the enthalpy of vaporization (page 19817, line 10 – 11) was recently updated (see Epstein et al., EST 44:743-748, 2010). Perhaps a column of Hvap values can be added to Table 1.

Thanks for the tip. Will add a column of the enthalpy of vaporization in Table 1.

The standard distribution of compound vapor pressures was likely chosen as a mathematical convenience, but is there any evidence to support this assumption? From the caption of Figure 3, it seems that the standard deviation of 2 was used in the analysis. It would be helpful if this was stated in the text along with some explanation of how was this parameter was determined and under what conditions it might be different.

The vapor pressures depend on temperature and thus have significant diurnal and seasonally variations. The standard distribution is introduced so that the values of ξ can change smoothly with temperature. In the 2-product model, only one mean vapor pressure is given for each SOG group. Each SOG group may contain hundreds of different organic species with vapor pressures distributed around the mean value. Log-normal distribution is a logical one but to evaluate how good is such an approximation needs laboratory data. Yes, we assume a standard deviation of 2. These will be pointed out in the revised manuscript.

Are equations (6) – (8) solved by operator splitting? If so, in what order are the processes of partitioning, nucleation, and oxidation solved?

Yes. In the model, we first solve partitioning, then oxidation, and then condensation.

Do the new and existing aging/oxidation reactions consume radicals in the chemical
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mechanism?

No. This is expected to be relatively small compared to other loss processes of radicals.

What deposition and scavenging parameters are assumed for each SOG species? It seems that only 6 additional tracers were added to the model to accommodate the mechanism enhancements (page 19821, line 27). I understand that the different size bins were tracked as an aggregate and split when necessary based on surface area. However, it is not really clear how it was possible to track the first generation products for all the species coating the several listed surfaces (dust, poc, etc.)

Yes, only 6 additional tracers are added (LV-SOG, SP-LV, BC-LV, POC-LV, and salt-LV). We assume that LV-SOG has similar deposition and scavenging parameters as H₂SO₄. The deposition and scavenging parameters for SP-LV, BC-LV, POC-LV, and salt-LV are following those of sulfate on these different types of particles.

In the manuscript you frequently use the term "vapor pressure." You probably really mean "saturation vapor pressure." If that is the case, it should be changed for clarity.

Yes, will modify as suggested.

In equation (8), why is only condensation but not partitioning included for a loss pathway of LV-SOG (i.e. why is there no Lpar term)?

LV-SOG vapor pressure is low enough for explicit condensation (i.e., solving the equation of molecules diffusing to the particle surface), similar to H₂SO₄ gas. We can't calculate both kinetic condensation and equilibrium partitioning at the same time as it will lead to double count. In addition, equilibrium partitioning will not allow the buildup or accumulation of LV-SOG concentration in the atmosphere as their saturation vapor concentration is very low (0.001 ug/m³).

Page 19818, lines 14-18: The authors use a new term HV-SOG without defining it. Based on their C values (10 ppb – 10 ppm), does HV-SOG include VOC1-6? Or are the HV-SOG analogous to Robinson's IVOC? It might be useful to make this connec-*

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tion.

I apologize for forgetting to define clearly the HV-SOG in the manuscript. As detailed in the text, SV-SOG and MV-SOG correspond to the “two products” in the widely used 2p SOA model. HV-SOG refers to those high volatile oxidation products of VOCs (i.e., products other than those SOGs indicated in Equations 3 and 4 which experimental yielding information are available). Many HV-SOGs may be further oxidized to CO₂ (or other volatile carbon species) but some unknown fractions may be end up as SOA. As I understand, the widely used 2-product model (at least the one implemented in GEOS-Chem) does not track HV-SOG. To avoid confusion, I will consider deleting the discussion of HV-SOG in the revised manuscript.

*Page 19820: What is the C^*_{upp} for LV-SOG based on?*

Based on the observed contribution of low volatile organics to growth rates of nucleated particles (and hence the typical concentration of LV-SOG) in the atmosphere and the Kelvin effect.

Page 19821: It would be helpful to give a few details about the PM sectional size distribution. How many bins are used?

The details have been reported in Yu and Luo (ACP, 2009). We use 40 bins for secondary particles with variable bin resolution. In the revised manuscript, I will provide a brief summary of bin structures.

Figure 5: The text states several times that this figure “clearly” shows improvement from the no oxidation case to the oxidative aging case when comparinnig to observations. To me, this figure is not very clear at all. Is there a better way to present this? Perhaps show “difference” plots between the two cases and observations?

Good idea. Will show “difference” plots between the two cases and observations.

Figure 8: Reporting NME and NMB values for the data in this figure would help to quantify the improvement. It would be helpful if the results of this model application were

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placed in context with other recent efforts to extend the SOA treatment in GEOSChem (e.g., Pye Seinfeld) and regional scale models (e.g., biogenic SOG is not aged in PM-CAMx). On line 1 of page 19815, please replace the reference to Schell et al. with (Carlton et al., EST in press).

Good points. Will incorporate these in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 19811, 2010.