

Review of Bergman et al.

This paper describes a new mechanism for formation of SOA and compares the results from the new mechanism to the old model. In addition both the old and new models are compared with observations.

The new mechanism is based on measurements of molar yields of SOA from Jokinen et al 2015 and Kroll et al (2005). In addition, the ELVOC and SVOC forms of SOA are also based on Jokinen et al 2015.

There are a number of figures and tables in the supplement which are never referred to in the paper. In addition, Fig 13 in the paper does not show the figure being described in the text.

In the following I detail changes/explanations that are needed:

Please explain how you calculate OH and O₃. Are these read-in?

Line 34: odd addition of numbers in “vegetation”

Line 46: please add references to models producing up to 121 Tg/yr SOA

Line 47: While VBS schemes are “more intricate” they are essentially parameterizations. You need to state this.

Line 52: “subsequently” is a strange word here. “In this theory”?

Line 60: remove first “of”

Line 69: add Wang and Penner, 2009:

Wang, M. and J. E. Penner, 2009: Aerosol indirect forcing in a global model with particle nucleation, Atmos. Chem. Phys., 9, 239-260, www.atmos-chem-phys.net/9/239/2009/.

After all, this paper was published almost a full year before Merikanto et al., 2009

Lines 101-103: You do not follow the mixing of soluble and insoluble particles? This seems highly unrealistic, since, for example SO₄ would almost certainly condense onto insoluble particles. Are simulating sulfate condensation only on the soluble modes? If on both types of particles, how do you separate into the modes after condensation, since sulfate and dust forming on an Accumulation particle, could, after separation, form 2 Aitken mode particles or 1 Aitken mode and 1 accumulation mode, depending on the size of the original particle after condensation.

Line 110: delete “more” in “A more detailed description”, since no description is given.

Figure 1: Is there some reason that the reaction of isoprene and monoterpenes with NO₃ is omitted?

From the figure the dotted line for the insoluble mode is only green for the Aitken model. So, it seems ELVOC and SVOC only condense on the insoluble Aitken mode. Why is this?

Line 139-140: does the diurnal emission variation for monoterpene at least depend on the local temperature variation? Surely the model has this variable available. Please explain why it should not take this into account, if not and give the max/min dependence of the emission.

141: This reads like the monthly mean isoprene emission is 572.3 Tg/yr, but I know that is the annual emissions rate. I think you mean you employ a monthly varying emission with an annual mean of....

175-176: Why wouldn't SVOC partition onto Accumulation and coarse modes if OA is present? (for example, by ELVOC condensation on these modes)?

215: I gather “all other biogenic emissions” really just refers to isoprene? Why not say this?

218-225: I think a paper should give sufficient information so that the reader does not need to read other papers in order to understand. Why not add a table with all annual emissions and references? After all, the emissions are one basic element affecting the evaluation

230: I don't think you mean SOA “on” the surface. Perhaps “in the boundary layer”? or in the first level of the model.

246: The factor of 1.6 should be referenced, unless there is no reference. If basically unknown, then say so.

247: do not use “on the surface”

250: change “overpresented” to “overrepresented”

313-314: here you first mention that there is no SOA formation from isoprene in OLDSOA. This should be stated earlier when talking about the sources from MEGAN (new and old).

326-327: can you speculate why the wet deposition fraction is higher in your model? Perhaps the vertical distribution places SOA higher in the atmosphere than in the other models?

Figure 7: Are (c) and (d) concentrations in the surface layer?

341-342: change to “using the additional particle formation” - I had to look back to see if you also include the Vehkamki scheme for sulfate in NEWSOA

352-357: please explain why southern ocean nucleation mode particle numbers are higher in OLDSOA than NEWSOA. After all, both these schemes include binary homogeneous nucleation

361: missing e in “increase”

365: “of of”

369-370: I do not understand why particle formation in NEWSOA isn't always larger than in OLDSOA, since the former includes RiCCO AND binary sulfate formation

371-373: what is meant by “compensated”? this explanation needs some expansion – it is hard to follow.

384: I think you do not mean observed N₂O, since this is a gas. Also, it appears CCN particles do not reach 5000/cm³: the scale only goes to 3000/cm³, but Vavill goes to 5000/cm³. BTW if you are referring to the top 2 lines of Fig 9b, there is no scale, so one cannot tell if the observation goes to 5000/cm³.

Figure 11: in (e) and (f), I assume the 2 NMB's and RMSE's are for PM_{2.5} and PM₁₀, but they are not labeled or in the caption. Also, what are the 2 numbers after R: in the upper 2 panels?

Figure 13: the label on the graph states that both a and b are the difference between NEWSOA and OLDSOA, whereas the caption states the correct difference.

472: Where is OLDSOA compared with MODIS and AATSR? The figure caption of figure 13 states that this figure is only NEWSOA. Also, it appears that both panels in fig 13 have most of the tropics and subtropics overestimated, so how do you estimate an underestimated AOD? Much of the paragraph after this sentence in line 472 cannot be understood based on fig. 13.

So, when you finally reference the supplemental figures, I see that the supplement Fig S10 and S11 compares OLDSOA and NEWSOA with MODIS and AATSR. I find it strange that this is perhaps the first supplemental figure (or table) that is meant to be referenced. Why have figures and tables in a supplement if they are not referenced?

BTW, I would at least include an annual average difference of OLDSOA and NEWSOA with MODIS and AATSR in the main text. As it stands, the main result of the data shown in the current Fig 13 was already discussed.

552: add a comma after “events”

553: explain what the “numerical issues” are. This is the first mention of this fact. Also, I would add “perhaps” since you do not actually analyze the reason for low CCN sized particles (by comparison with observations, for example)

567: The change in AOD is ultimately from the change in particle number, but must actually be due to the change in size distribution, I think. This sentence should be expanded to remind the reader of the processes leading to the change in AOD