Atmos. Chem. Phys. Discuss., 14, C2451–C2454, 2014
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Volatility basis-set approach simulation of organic aerosol formation in East Asia: implications for anthropogenic-biogenic interaction and controllable amounts" *by* H. Matsui et al.

Anonymous Referee #2

Received and published: 16 May 2014

This is a very well written manuscript that applies the volatility-basis set parameterization over East Asia and studies the controllable amounts of organic aerosols, both from anthropogenic and natural sources. The work is very well explained, clearly falls within the scope of the journal, and I recommend publication after addressing the minor comments listed below. I particularly liked section 5.2, which is extremely informative.

- The introduction is a very thorough discussion on the interest of the topic. While reading I got the sense that there were too many references there; although this is not

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a bad thing, since this is not a review article, I found it a bit distracting. The authors might want to reduce the number of references listed there, by maintaining the focus on the questions answered in the manuscript, rather than doing a full literature review of the topic.

- Although the statement in p. 6207, l. 6-8 is true for Asia when considered explicitly, the two studies of Pye et al and Jathar et al (referenced in the manuscript) were global, thus Asia was implicitly included. The statement is not adding any more value in the manuscript from what it currently has, and can easily be dropped. The fact that a regional model was used over Asia probably for the first time can then be stressed out in the following page, line 3: "We simulated. . .".

- The model resolution (mentioned in section 3.1) and the aerosol solubility per volatility bin should be mentioned in section 2.1.

- Why a constant kappa value was selected for all volatility bins, instead of a varying one?

- P. 6208 l. 24 mentions aqueous chemistry, but p. 6211 l. 27 says the opposite. Is the statement in p. 6208 about inorganic species only? Also, citing 5 papers for something that is not included in the model, is a good example of the excessive number of references used in the introduction, as I mentioned in the first comment.

- In the same sentence, all these processes are also forming new OA mass from VOCs that in the current model state do not.

- Please expand with a sentence or two on the "other uncertainties" mentioned in p. 6212, l. 20, as was done in p. 6211, l. 15-16.

- What is the "custom solution procedure" in p. 6213, l. 23? Factor analysis?

- Same page bottom, please quantify "correlated well".

- The numbers mentioned at the bottom of p. 6214 apply to all simulations?

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- P. 6216, l. 2-3: This means that there are no dust and sea salt particles in the model, or they are offline?

- The over/under estimation of results during day/night (p. 6217 middle) is very interesting. Does that mean that the aging is too fast, or there is very fast removal during night? Please say more about this.

- SIVOCs and OVOCs must be considered for Tokyo (p. 6217 bottom) but OPOA only account for 6-7% in other places (next page, top). Does that happen due to the different precursors present in these sites?

- Section 4.3: why not using a 2-bin-step aging, as the new VBS schemes?

- P. 6221 bottom: Which volatility bins receive most of the mass? In aging-off it should be the same with emissions, but how about the aging-on experiments?

- P. 6225 top: A factor of 3 lower is impressive. Why not emit gas-phase species and neglect them when calculating the aerosols?

- Same page, 2nd paragraph: this is an excellent result. Can you attribute that to oxidant changes (like in the previous paragraph) and larger mass of pre-existing particles?

- Next page, 1st paragraph: Please expand this. Do you expect higher/lower contribution during other periods?

- I am a bit confused on the POA/SIVOC explanation in Table 1. SIVOCs can be secondary, but they also form POA which are exclusively primary? This is not what Fig. 1 shows, where SIVOCs are only primary.

- Do the lines in Fig. 5 suggest faster aging than it should be? Also, why not have intercept (y=ax+b) when fitting the slopes?

- I believe that the non-equal white piece on the 3rd pie and blue piece on the 4th pie in Fig. 9 show the non-linearity of the system, which is very interesting, and can be discussed a bit more.

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Technical corrections

- P. 6206, l. 26: factor OF 5.

- P. 6212, l. 17: delete second “also”.

- Same page, l. 2-3, delete “in and around Tokyo”.

- Fig. 8, 10, 11 legend: no need to mention the sigma levels in parentheses.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 6203, 2014.