

Interactive comment on “The co-condensation of semi-volatile organics into multiple aerosol particle modes” by Matthew Crooks et al.

Anonymous Referee #1

Received and published: 4 March 2016

Solving the partitioning of semivolatile compounds between the gas phase and different sized particles is a challenging task. In this manuscript authors provide a new method for solving the equilibrium partitioning analytically. The method is thoroughly presented and compared against numerical solution. Overall, the topic of the manuscript is suitable for Geoscientific Model Development and the method presented might be valuable for scientific community using volatility basis set to represent organic aerosol in large scale models. I recommend the manuscript to be accepted after the comments below have been addressed.

My main criticism is related to examples and discussion presented in Chapter 7. After reading the manuscript the main question is if the parameterization is good enough in representing dynamic conditions so that it can be used in atmospheric applications? It is difficult to understand what is done in Chapter 7. What the modelling setup described

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at lines 390-394 actually mean? Does it mean that for the first 300 or 10 seconds the condensation is only allowed for the second mode and after that also for the first mode? Maybe more discussion motivating the modelling setup is needed to make it easier to understand. One way to estimate if equilibrium approximation gives reasonable results in atmospheric conditions would be to run the parcel model with varying temperature and relative humidity trajectories mimicking the turnover of boundary layer.

Minor comments:

I think it is quite uncommon to use R_H notation for saturation ratio. I would recommend changing it to S in equations as it is commonly used and because you are also discussing RH as relative humidity, which is usually expressed in percentage.

Line 49: Citation to Connolly et al. (2014) might be in a wrong place. At least I cannot find relevant discussion on aerosol forcing from that article.

Line 70: Why is Hildemann et al. (1991) article cited when discussing on the current status of knowledge on atmospheric organic components. Has there been any progress since 1991?

At least for me, the “Quasi-leading order solution” is a totally new term for describing the solution. Why “quasi”?

Figure 3: How many solutions there is actually presented?

Figure 9: Diameters are really confusing as they are different than given in the Table 4 and in the text.

Figure 13: It looks like the system is not evolving towards equilibrium conditions. Why not?

Figure 15: The change in the shape of aerosol size distribution in numerical solution is strange. Is there some reason why larger particles grow more so that the shape of distribution is not preserved?

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