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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Organic particulate matter formation at varying relative humidity using surrogate secondary and primary organic compounds with activity corrections in the condensed phase obtained using a method based on the Wilson equation" *by* E. I. Chang and J. F. Pankow

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Interactive Discussion

Discussion Paper

	page	details
Comment **1.**	3	*". . . hypothetical lumped product": Please add a reference here."*
Response		**Accepted – done.**
Comment **2.**	4	"*End of paragraph 1: Please add a brief discussion on the findings of the Bowman and Melton (2004) study (especially on the computational requirements).*"
Response		**Accepted – done.**
Comment **3.**	4	"*. . . is thus problematic.": It would be useful to report (if possible) a potential range of uncertainty in partitioning associated with this kind of assumption.*"
Response		**Accepted – done.**
Comment **4.**	9	"*The iteration process (Eq. 5 to 10) is important for the whole paper. It would be nice if the steps are explained a bit more.*"
Response		**Accepted – done.**
Comment **5.**	6-10	"*Acronyms such as "SIMPOL.1" and "CP-Wilson.1" should be clearly defined in the text.*"
Response		**Accepted – done.**
Comment **6.**	23	"*Replace 'significantly more computationally economical' with "significantly faster" "*
Response		**Accepted – done.**

ACPD

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Interactive Comment

Comment **7.**	13	*"Some degree of caution should therefore be exercised with considering predictions made using CP-Wilson.1 for nitrate-containing compounds."* *"The authors are clear in justifying the reason to be cautious when nitrate groups are present, but do not give any guidelines on caution should be exercised. Should activity coefficients computed with CNO2 groups be independently evaluated?"*
Response		**Accepted – the text has been changed to state that the calculated values for the nitrate-containing surrogate compounds may be less reliable than for other surrogate compounds considered.**

Comment **8.**	13	*"At some point . . . CP-Wilson.2". This is future work, and should be moved to the end section of the manuscript."*
Response		**Not accepted – this is minor editorial point. We believe the text is ok where it is.**

Comment **9.**	13	"*Second line before end: Would the system always separate in two phases?"*
Response		**Accepted – done – clarified.** Yes, if it is just polarity do to variable oxygen content that drives phase separation, the number of liquid phases is limited to 2. By comparison, if a fluorocarbon compound was also present, three liquid phases are possible, depending on levels. This has been clarified in the text.

Comment **10.**		*"The Gibbs phase rule allows for more than 2 or 3 phases to be present."*
Response		**Accepted – done – clarified.** See response to Comment 9.

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Interactive Comment

Comment **11.**	14	"*End of section 2.3: Was "further optimization" carried out by further reducing the step size?*"
Response		**Accepted – done – clarified.**
Comment **12.**	15	*"Is it possible that more than 2 aerosol phases exist?"*
Response		**Accepted – done – clarified.** See response to Comment 9.
Comment **13.**	17	"*was obtained 10,000 . . . system resource availability": this could be reduced a bit, perhaps as " was repeated 10,000 times to obtain a representative average calculation time*"
Response		**Accepted – done – clarified.**
Comment **14.**	19	"*End of section 3.1: Can you give a brief explanation why phase separation is likely at high* x_i*?*"
Response		**Accepted – done.**
Comment **14.**	21	"*End of section 3.4: Would P6 "not being comfortable" in each phase suggest that further phase separation is possible?*"
Response		**Accepted – done – clarified.**
Comment **15.**	23	*"The implication of the new method on predicted SOA is particularly significant, especially since the sensitivity at low* M_o *is enhanced. For this to have an even stronger impact to the reader, it would be useful to point out the SOA increase for "atmospherically-relevant" levels of* M_o*."*
Response		**Accepted – agreed –** the paper refers the reader to Pankow and Chang (2008) which has now been published.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 995, 2008.