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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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	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 find-
		ings of the
		Bowman and Melton (2004) study (especially on the computa-
		tional requirements)."
Response		Accepted – done.
Comment 3.	4	" is thus problematic.": It would be useful to report (if possi-
		ble) 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.

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 cal- culated values for the nitrate-containing surrogate com- pounds may be less reliable than for other surrogate com- pounds 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 vari- able 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

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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 xi?"
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 par-
		ticularly significant, especially since the sensitivity at low Mois
		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 Mo."
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.