

Interactive comment on “A naming convention for atmospheric organic aerosol” by B. N. Murphy et al.

D. Simpson (Referee)

david.simpson@chalmers.se

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Firstly, the authors are to be congratulated on attempting to bring an improved system to the complex world of organic aerosol nomenclature. The paper is well written, makes many good arguments in a logical manner, and after some changes deserves to be published as a valuable contribution to an important discussion. My feeling is still that the organic aerosol world is so messy and changeable that it will outlive even this valiant new attempt at organisation, and while reading this paper I couldn't help but think of other ways to name such compounds. Still, the main test of such a contribution is time – whether the community finds it useful or not, and one can hope that in any case this paper will stimulate more careful consideration of OA properties and definitions.

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Major issues

1. The scheme is essentially 100% VBS in character, so most suited for modellers who make use of that framework. However, even the original 1-D VBS framework is giving way to 2-D versions, recognising that volatility alone is insufficient to characterise OA. Why didn't the author's propose a scheme which accounts for this 2-D framework?
2. I wonder in particular why a stronger link to O:C ratios was not made. This information is readily available from both models and measurements these days, and provides a natural link to both the 2-D VBS approach and observable quantities.
3. If using VBS species, one knows if the C^* value is $1\mu\text{g m}^{-3}$, 10 ng m^{-3} . Why lose information by converting to broad classes such as LV, SV etc. As an example, why does a shift in C^* from 0.1 to $0.001\mu\text{g m}^{-3}$ not result in a change of nomenclature (both are LV-), whereas one from 1000 to $100\mu\text{g m}^{-3}$ does, from IV to SV? (Why not use the $\log_{10}(C^*)$ values as an index instead of broad letter codes?)
4. The suggested nomenclature seems very logical in many ways, but then exceptions occur. Notably, on page 29987 the grouping aqSOA is introduced, with no distinction between anthropogenic, biogenic, biomass-burning or any other source. Why are aerosols produced by aqueous pathways not treated in similar ways to aerosols produced via the gas-phase? As another example, how should one denote an organic nitrate formed from anthropogenic NO_x and biogenic VOC? Or oligomers of both ASOA and BSOA compounds? These types of problems suggest to me that many papers will have to re-invent terminology anyway.
5. OM, OC, OCA? Here I am not sure. The traditional use of OM and OC has been for the particulate phase, synonymous with OA. The authors are both logical and

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consistent here in defining $OM = OG + OA$, but there is significant potential for confusion with respect to other papers. Why not simply add 'T' as prefix, thus TOM or similar to TOOC as used in Heald et al. (ACP, 2008)?

Personally, I would have preferred suffixes, similar to chemical practice, e.g. $OM_{(g)}$, $OM_{(p)}$, $OM_{(t)}$ could have represented the gas, particle and total concentrations. This could be easily extended to $OM_{(aq)}$, $OM_{(glassy)}$, etc., and would be consistent with today's use of such phase identifiers in the standard chemistry literature.

6. The definitions and discussion of POA assume that emission factors are largely derived from dilution sampling. Such emission factors are often derived from ambient sampling though, e.g. tunnel experiments or regressions compared to other pollutants. How should such POA emissions be defined and indicated? (The discussion leading to the $320 \mu\text{g m}^{-3}$ criteria for POA in section 4 is difficult to apply when dilution sampling is not the source of the emission factors.)
7. Table 2 POA, POG - what about compounds emitted as gases (POG) at high temp, but which condense on cooling? These are excluded from the POA and SOA definitions.

Smaller issues

- p. 29985 Add some references to support the statements on the 1st lines here
- p. 29985, last line. I think the sentence that "This view was at odds with what a traditional..." reflects only certain model setups. I think the reason for many of the low SOA/POA ratios found in some of the early modelling studies was the use of very low yields, and other model-specific assumptions. Even in the earliest EMEP OA models (Andersson-Sköld & Simpson, JGR, 2001, Simpson et al., 2007), we found very large SOA/POA ratios.

C9971

- p. 29988 Line 8. Lanz et al. 2007 is not a good reference for 14C work (they used AMS + PMF). One of the Szidat et al. papers would be better for the Swiss studies on this issue. It would be good with some non-European studies also.
- p. 29988 Explain what assessment methods 5, 201A and 202 are.
- p. 29988 Line 27, 'at' low concentrations.
- p.29990, line 10. Add base-10 to qualify logarithmic average.
- p.29991, line 5. Please give some hint as to how this alternative looks, it is frustrating to have to start guessing at this stage.
- p.29993, line 4. Vegetation (and other biological systems) can emit oxidised VOC, some of which must end up in aerosols even without chemical reactions. This would seem to be outside your SOA definition, but would be measured as OOA?
- p.29993, line 23. Here I disagree that one can call the proposed framework quantitative, when the span of volatility can be four orders of magnitude.
- p.29996, items 2 and 3. If SOA mass evaporates and reacts, then the loading must change too. These reactions normally add oxygen to the system.
- p.29997, line 5. The statement here that the proposed system is consistent with 'ongoing field and laboratory' studies is misleading I think, except in the sense that the very broad categories OA, SOA, etc. can still be used. As noted on page 29994, 'field campaigns will have limited or no access to' the information needed.
- p30000 The FR references need more information.
- p30007, Table 2. Better to have the Base Terms before the Modifiers, since the latter make use of the former.

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- I think Table 4 confuses more than it helps. The SV-OOA here includes both 0.01 and $100 \mu\text{g m}^{-3}$, which conflicts with Table 1, and the qualifier 'in the literature' is too vague to be helpful.
- Table 5 is also confusing. Here we can have compounds like SV-SOA-sv which have C^* of between $1\text{--}100 \mu\text{g m}^{-3}$, classified as POA in the 'traditional framework'. A problem here is whose traditional framework one refers to. Many emissions in Europe have been derived from ambient data with concentrations of order far lower than $100 \mu\text{g m}^{-3}$; the definition of POA in this case becomes rather tricky.
- Finally, and this is personal taste of course, I find these ascii text strings such as SV-bSOA-v a little inelegant, looking more like equations rather than chemical compounds. Again I would have considered some sub and superscript notation.

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