Atmos. Chem. Phys. Discuss., 13, C10752–C10756, 2014
www.atmos-chem-phys-discuss.net/13/C10752/2014/

Atmospheric Chemistry and Physics
Discussions

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

G. McFiggans (Referee)

g.mcfiggans@manchester.ac.uk

Received and published: 8 January 2014

It is indeed a daunting and ambitious task to propose a naming convention for organic aerosol and this attempt is one that should be applauded. The convention starts from the useful and pragmatic VBS approach, defining organic particulate material largely in terms of volatility as the major determinant of the phase state of the organic material. Again, this is largely appropriate and most of the concepts within our current understanding of organic aerosol seem to fit reasonably well within this context. I am fully in agreement with the need to be much more rigorous and systematic in classifying and describing both the particulate and vapour components in organic aerosols. I am also in agreement that the broad classes of descriptor that are described are also

broadly the ones that need to be reported in modelling and measurement studies; it is in the exact terminology that I have problems. In agreement with the other review, I feel that time will provide the most challenging test of the proposed convention. Only if it is comprehensively capable of encompassing current and emerging understanding of this complex area should its adoption be encouraged and persistence be guaranteed.

Before publication, I would like to solicit responses from the authors on the following points:

p29990: I am unsure of the reasoning behind use of the alphabetic volatility descriptors. They appear rather arbitrary and relative. The basis set has already clearly defined the decadal bins of saturation concentration at 298K which are objective and absolute and hence not open to interpretation - there is no need to try to impose new strict boundaries here to what is subjective terminology. I have been uncomfortable for some time with the quite arbitrary low volatility cutoff of the SVOC class definition, where historically any particle component that non-negligibly equilibrates with the vapour phase might be considered "semi-volatile". I foresee the same sort of problem here as has occurred with the terminology ultrafine, where UFPs have been defined as particles less than 3 nm, 10 nm, 50nm or 100nm in various recent publications to my definite knowledge, with probably very many more. Indeed, this is already happening (see the sentence starting line 18 on p29993). For these reasons, I am not convinced by the sentence starting on line 25 on p29991 "In general, the framework allows the communication of available information about volatility and source while avoiding confusing and possibly contradicting terminology". It would be more convincing if numeric saturation concentration descriptors were used in the prefix and suffix. For example a prefix or suffix of "1,2" meaning a component between 10 and 100 $\mu g m^{-3}$ or of "1.5" referring to a classification of saturation concentration 32 $\mu g m^{-3}$. A cynical rephrasing of the statement in line 23 of p29993 might be "uses the quantitative effective saturation concentration to distinguish between classes, and replaces them with qualitative, subjective descriptors with artificial strict definitions".

p29990: I have several problems with the second letter in the source root name: i) M representing mass could easily be confused with be matter or material in other literature (particularly that concerned with air quality and policy). Indeed, use of OM to represent organic mass (in contrast to OA or OG) appears to erroneously imply that OA or OG do not have mass based units. ii) since an aerosol comprises the entire suspension of particles in the carrier gas, OC for condensed organic or OP for particulate organic would appear to be preferable to OA (though the former would then be inevitably confused with organic carbon of either phase and the P in the latter could be confused with primary). This is actually at the root of a problem with the interpretation of primary emission ageing, where the primary components that are oxidised prior to condensation could either be the gaseous components in the original primary organic aerosol (using its conventional definition to mean gaseous plus particulate components) or evaporated condensed components. The failure to adopt this definition of aerosol to include all phases is an extremely widespread source of error and confusion and it is extremely important that "aerosol" is not used to mean only the condensed material in the aerosol. iii) OG is probably not as precise as OV, since vapour is a condensable gas (i.e. at a T lower than its critical point). Since we are concerned with the organics in the aerosol that can partition, it might be considered useful to separate the non-condensed organics into those that will always remain as "carrier gases" and those that could condense with changing T, RH and P under conceivable atmospheric conditions. iv) OM has previously been widely used to denote only the particulate organic mass (to contrast with the particulate mass of organic carbon, OC) rather than the total organic mass in both phases. I don't have any clever suggestion to resolve these problems, but would probably resort to subscripting this second letter, using for example POM_T, SOM_V and SOC_P to mean Total Primary Organic Material, Vapour Phase Secondary Organic Material, Particulate Phase Secondary Organic Carbon etc...

p29991, line 3: The absence of the "P" or "S" indicator meaning that the classification is "both" primary and secondary could present difficulties. It could instead be that it is unspecified or unknown, which could well be the case unless the classification is only

generated by a model.

p29991: Similarly difficult is the lumping of the "aq" pathway modifier alongside the "a" and "b" source modifiers. Surely both "a" and "b" can also be "aq" in origin, for example. Theoretically, there is also no reason why some biomass burning derived VOCs could not be oxidised to produce "aq" particulate. Similarly, surely all "c" particulate is also "a" (likewise all "m" being a subset of "b"). Whilst all the modifiers are useful, I don't think their meanings are quite comparable in what they are classifying. Also, the meaning of the absence of modifiers is not defined - is it that the origin is unknown or that it is not of the origin of the absent modifier.

p29993: I am not sure I understand the statement starting on line 27, where the naming convention allows for operationally-defined measurement nomenclature in the source root term. The convention seems to lose some of its unifying capability by allowing these more imprecise terms to replace the source type. I am not fully convinced that the community will not continue to use their own favourite nomenclature for particular OA specialisms, particularly in field studies, routinely dropping the suffix and seldom providing measurements able to define the prefix. However, there may be a perfectly reasonable argument for allowing this which I have missed. Is it simply to recommend a formalism to allow the prefix (and suffix) to be added?

p2994: I particularly like section 4, which pragmatically identifies the need to map the proposed convention onto the traditional POA/SOA model and the recommended approach is promising. The caveat in the last sentence of section 4 is also appropriately strong!

Along with the other reviewer, I am also curious about why the authors have chosen to omit explicit reference to the elements of the 2-D basis set encompassing the O:C ratio or oxidation state space, which allows further description of aerosol properties in terms of increasingly measurable / predictable quantities. I think a naming convention that used numerical prefixes and suffixes representing the coordinates on the 2-D VBS and a source root name that was a little more precise and consistent (e.g. 1,2,0.6-POM_P-2,3,0.4 representing mass of primary particulate organic material currently in C* bin 10-100 $\mu g m^{-3}$ and of O:C ratio 0.6 but emitted in bin 100-1000 $\mu g m^{-3}$ with O:C ratio of 0.4) would make a valuable and probably more future proof contribution.

Throughout the manuscript, it is not completely clear what is being classified. Is it envisaged that the convention is to be applied to aerosol bulk mass, a particular fraction of the mass, individual particles of variable mixing states or specific components within either individual particles or ensembles of particles? Real air parcels may contain, for example, some particles with secondary components of biogenic origin condensed on anthropogenic mixed BC/OC combustion particles in an ensemble that also contains particles that have been long-range transported across the marine environment from biomass burning regions. The classification of bulk samples in terms of the convention would become very unwieldy if it were to consider all of the sources and properties in an ensemble, but will lose history and mixing state information if it is used to specify a mass weighted average value. More importantly - how do you calculate the mass weighted average of a non-numerical descriptor? The authors should include a discussion of the possible uses and applications of the convention and what information would be preserved and lost in various example cases.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 29983, 2013.