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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Determination of the biogenic secondary organic aerosol fraction in the boreal forest by AMS and NMR measurements" *by* E. Finessi et al.

Anonymous Referee #1

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This paper is the first to simultaneously compare organic aerosol components derived from factor analysis of both AMS and NMR-WSOC filter data. The measurements were conducted at a well-studied site in the Finnish boreal forest. The major result from the study is the factor analysis from the NMR data set that yields two factors, amine-related and terpene-SOA related, that are apparently biogenic in nature. The value of this work is that the NMR factors may ultimately help in biogenic organic aerosol source apportionment. While the data set is quite short and there was not a lot of variability in the data from day-to-day, which limits the information that can be arrived at from a factor analysis, there is merit to publication based on the NMR data.

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Interactive Discussion
Discussion Paper

However, I find that the AMS data really do not make the paper all that much stronger, aside for a general correspondence between the more highly oxygenated AMS factor and the HULIS factor from the NMR. In particular, the AMS factors - OOA1 and OOA2 - are a bit too "blunt" to be able to learn much from, when compared to the NMR factors.

A few points:

- P22625 – What collection efficiency was used for the AMS data, and with what justification?

- P22626, line 6. The AMS and DMPS data can not “agree” with each other – they measure different quantities, mass and volume.

- P22632, line 5. How much of this difference could be due to different size cuts on the inlets?

- P22635, line 13. For the NMR F1 factor, I am surprised that the authors appear to consider that this factor is contamination, given that they do not see it in their control blanks. Indeed, later, they do not include these data in their analyses (P22637, line 10 and afterwards). Without firm indications that these are contaminants, should not a full data analysis be also conducted with these data included? Also, are the spectra so specific that only n-butyl glycols are identified, or could other varieties be possible too? I guess the question I am asking is whether the authors have been to quick to rule out the possibility that this is a real atmospheric signal?

- P22638, line 6. Better justification both here and earlier for why the factor of 1.8 is used to convert carbon to mass.

- In general, can the AMS factors be compared to other AMS factors reported in the literature for biogenic SOA in mid-latitude forests (e.g. Slowik et al.)?

- In general, there is implied in the paper that the HULIS NMR factor has some correspondence to the OOA1 factor from the AMS, in amount and time profile, i.e. highly oxygenated material. However, there is a claim made that this is not biogenic in origin,

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whereas the less oxygenated material is biogenic. I don't see the justification for this. In particular, while the more oxygenated material may be arriving with winds from the south, what is to say that the carbon is not of biogenic origin (at least in part) that has been highly oxidized by anthropogenic oxidants?

- Figure 8. When I compare F1-F3 in the two sets of solutions, they look remarkably similar, which makes we question the validity of F4 in the 4 factor solution. Indeed, earlier in the paper, it is stated that this factor is derived at close to the noise level. Although there is a nice correspondence between F4 and a lab terpene SOA, I nevertheless believe this caveat (that this is a pretty low signal-to-noise factor) needs to be more clearly stated in the Abstract and Conclusions.

- Figures. I found many of the axis labels and figure line weights too small (e.g. Figure 1)

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 22619, 2011.