

Interactive comment on “Long-term volatility measurements of submicron atmospheric aerosol in Hyytiälä, Finland” by S. A. K. Häkkinen et al.

Anonymous Referee #4

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This paper reports an interesting 2+ years of aerosol composition and the physical property of volatility. It focuses mainly on the non-volatile fraction which it shows nicely but not surprisingly is correlated to and partially explained by BC. There is some attempt to explain the remainder of this non-volatile fraction of PM, much of which occurs in summer, and to link it to organonitrates. This part of the paper is interesting but speculative. It is worth publishing if the authors can clearly distinguish their clear results (i.e. the amount of NV that is BC) from their speculative discussion (about what the rest of it is). I think the former is interesting to the extent that they have been able to quantify the fraction explained by BC, and this result could stand on its own if it is written more clearly.

Referee 2 notes an important mismatch in the size range of the hygroscopicity and

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the measured NR mass. It's also worth noting the comparison to AMS is comparing the NV to the NR – i.e. what does volatilize at 600 to what didn't at 280. While these are somewhat subtle differences, they are actually quite relevant to the conclusions highlighted here. While I think the work still has merit (i.e. it is not necessarily “flawed” as Referee 2 asserts), I think the authors failed to do a serious job of clearly identifying these mismatches and discussing their impact on the conclusions (and they should be clear in abstract too).

OTHER SPECIFIC POINTS TO ADDRESS: Abstract line 14 – should be “other” not “else”

Fig 1 – are these the only 2 months that had an AMS at Hytialla? Fig 4 – interesting 3-season trend of high BCF in winter. Fig 5 – Milagro is misspelled.

P11204, L 25 - Report exact residence time in the TD.

P11205, L8 – “It has been observed that almost all submicron aerosol particles. . .” is an unverifiable statement. Should be tempered, something like, almost all the volatility observations of submicron aerosol particles to date have found they contain. . .

P11206, L4 – Add a statement to explain how this volatility study is different from others conducted at Hyytiala (e.g. Ehn et al., 2007; Raatikainen et al. 2010).

P11207, L25 – Why was it switched to constant temperature operation?

P11211 – what is size range of aethelometer – i.e. was an 1-micron impactor used?

P11211, L12 – What is the basis for this assumption? Were AMS data or past measurements at the site used to arrive at this figure? What are the consequences of assuming varying particle density for your analyses and conclusions, i.e. how sensitive are your results to the value you use for particle density? If particle density has a seasonal variation then it will influence the seasonal trends reported for MFR.

P11213, L10 – State heated to 250C

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P11213, L19 – Why? At least state how Ehn et al., (2007) shows this.

P11214, L8 – Despite the increase around 5 or 6/2008 the general trend to me seems to be decreasing MFR at all temperatures to 8 or 9/2008, hard to tell with data gap though.

P11217, L22 – State correlation coefficients here as well as in table 3.

P11219, L1 – A better section title is ‘Indications of the chemical composition of non-BC MFR.’

P11222 – reword “explaining factor”, “explaining compound” – what is meant? (The meaning is not clear in English.)

P11222 – polymerization is not the only possible explanation for non-volatile organics.

P11223 – “aerosol volatility. . . cannot be explained by any single parameter” – this is poor use of the word “volatility”; here and throughout most of the paper the focus is on the non-volatile fraction, as defined by the 1-sec heating to 280C. This constraint should be repeated for clarification to replace the non-specific term “volatility”.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 11201, 2012.

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