

## ***Interactive comment on “Evaluation of the performance of a particle concentrator for on-line instrumentation” by S. Saarikoski et al.***

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Received and published: 27 May 2014

Final Author Comments to amt-2014-39

Referee #1:

We thank Referee #1 for the valuable comments on the manuscript. The comments are numbered and each of them is followed by a response from the authors.

General comments:

1. COMMENT: “Rather than originating from the cellulose substrate, I would venture that the ammonia is dissolved in the recirculating water in the mVACES, as it is a highly soluble gas that tends to accumulate in water held in indoor environments. It would

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make sense if it was released in the saturator as the temperature increased.”

REPLY: Referee is correct that there is a possibility that ammonia do not originate from the cellulose sponges. The statement that ammonia could have been dissolved in recirculating water in the m-VACES was added to text: “Gaseous ammonia could also have been dissolved in the recirculating water in the m-VACES, as it is a highly soluble gas, and released in the saturator as the temperature increased, however, that should be verified by additional laboratory and field tests. Jung et al. (2010) have tested the enrichment of gaseous ammonia in the VACES. They observed a slight enrichment of ammonia in their experiments but concluded that the saturator water bath was not a major sink for gaseous ammonia”

2. COMMENT: “I would also venture that the enhanced organics too are possibly not originating from the cellulose, as they could be introduced in the gas phase in the sample. Various chemical species that favour the gas phase (due to a low molecular weight and/or polarity) will partition into the particle phase at higher humidities (e.g. doi:10.1038/ngeo1809) and these may not be completely released upon drying.”

REPLY: We agree with the referee that some organics could have been introduced in the sample in gas phase as there were no denuders upstream of the m-VACES. That was already stated in Chapter 3.1.3 (Particle size) when the size shift of small AS and DOS particles was discussed. New text regarding the partitioning of gas-phase organics was added to Chapter 3.2.4. (OA and r-BC): “Hydrocarbon-like compounds can also originate from more efficient partitioning of SVOCs into the particle phase in the m-VACES. The artifacts of SVOCs in the VACES have been investigated in Wang et al. (2013). They showed a positive total organic carbon sampling artifact of 15–20% for a typical ambient TOC concentration of 10  $\mu\text{g m}^{-3}$  in Los Angeles in summer season. However, their experiments were conducted at high ambient temperature and photochemical activity so lower levels of SVOC artifacts can be expected if the sampling is conducted in a colder season, as in this paper.”

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3. COMMENT: "I do not find myself convinced by any of the discussion regarding chloride. If the ambient aerosol is acidic, NH<sub>4</sub>Cl should not exist in the particle phase at all, with thermodynamics dictating that the chloride instead favouring gas phase HCl. Chloride is also missing from figure 5 due to low signal-to-noise and is supposedly only 1% according to figure 6. Under these circumstances, I would strongly doubt the reliability of this data product, as there are a number of potential interferences (from organics or rBC), or it could be a signal from NaCl, which is detected with a very low sensitivity by the AMS due to its refractory nature and is not generally regarded as quantitative. As such, I would recommend removing this entire line of discussion. Ultimately, if the data is of too poor a quality to be presented in a figure, then it is probably too poor a quality to form the basis of any conclusions."

REPLY: The discussion on the behavior of chloride has been removed from the manuscript as suggested by the referee. Figure S3 was also removed from Supplements.

Specific/technical comments:

1. COMMENT: "Rather than refer to cation to anions, it would be more appropriate to refer to cationic to anionic charge."

REPLY: All "cations to anions" have been replaced by "cationic to anionic charge" in the manuscript.

2. COMMENT, Page 2744, line 2: "Surely the mVACES temperature is higher, not lower, than VACES?"

REPLY: Referee is correct that the cooling temperature is higher in m-VACES than in the VACES. Sentence has been corrected.

3. COMMENT, Figure S8: "The Q/Q<sub>exp</sub> for the ambient dataset is much lower than what would be expected (less than 0.1), which may suggest there is a problem with the data (normally error estimates being too high). This is important, because one

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might only expect an increase in Q/Q<sub>exp</sub> of around 5, based on the improvement to signal-to-noise. Can the authors comment on this?

REPLY: Fig. S8a was wrong in Supplements and it was replaced by a correct figure. In new figure Q/Q<sub>exp</sub> value for ambient data is ~0.5. The fact that the Q value is below the unity indicates slightly overestimation of the error values. The ratio of Q/Q<sub>exp</sub> for concentrated to ambient data is ~12 (6/0.5), which is quite close to the expected improvement in the signal-to-noise ratio (average SNR<sub>conc</sub>/SNR<sub>amb</sub> ~9, Figure attached). A sentence regarding the errors was added to manuscript: "For ambient PMF solution Q/Q<sub>expected</sub> was much lower (~0.5) than that for concentrated (~6), which indicates slight overestimation of the error values for ambient data and larger signal-to-noise ratios for concentrated data."

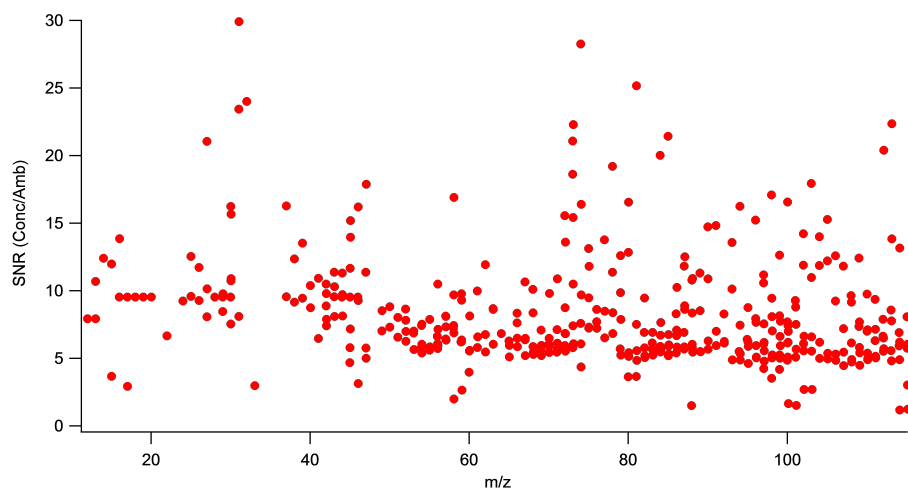
References

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Interactive comment on *Atmos. Meas. Tech. Discuss.*, 7, 2737, 2014.

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**Fig. 1.**

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