

Interactive comment on “On the calibration of FIGAERO-ToF-CIMS: importance and impact of calibrant delivery for the particle phase calibration” by Arttu Ylisirniö et al.

Anonymous Referee #2

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General:

The authors investigated the effect of different calibration procedures for deriving vapor pressures of oxygenated organic volatile compounds by FIGAERO-CIMS measurement. They find that the structure of the dried deposited calibrants together with the heating rate affect the vapor pressure by up to a few orders of magnitude. FIGAERO-CIMS developed recently to a method with important impact on understanding the formation of secondary organic aerosols (SOA). On one hand FIGAERO-CIMS offer great opportunities for direct measurements of SOA composition and partitioning of oxygenated organic volatile compounds. On the other hand calibration and use of the

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FIGAERO are not trivial, especially as the target compounds have low volatilities and filter sampling with thermo-desorption is involved. Despite efforts by the FIGAERO community there remain discrepancies and open questions, which are important for a larger community as FIGAERO CIMS are one of current state of the art data providers for understanding SOA and related topics. From this point of view the paper is timely and helpful. It is interesting and overall well written and clear. It addresses important issues and will add to reliability of FIGAERO-CIMS use. The paper should be published in AMT, after the authors considered one major and a few minor points.

Major comment:

The manuscript deals with calibration issues, which means it deals with quantitative issues, and it compares to results from other references. Although the authors made aware of suited fitting procedures for data with errors in y and x (line 168f) there is no detailed error analysis nor are error bars shown in the Figures 1, 4, 7, 8, S2, S3. The only errors given are the statistical errors from averaging single measurements. I find that strange for a paper that deals with quantitative analysis and urgently suggest to add more detailed error analysis' and -discussion.

Minor comments:

line 44: References should be given already here, in addition to the link to section 2.5.

line 82: Are these heating rates really so accurate (2 decimal digits)? The experiments do not lead to the same maximum temperature, does that effect the integrals under the thermograms?

line 97: From PEG-4 to PEG-8. In integer steps? Please, specify precisely which PEG's you used.

line 115-120: Did you use a neutralizer? If yes, which? How did you handle multi-charged particles in the selection by DMA. This needs to be explained, and potential errors need to be estimated and discussed.

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Figure 6: I don't see a real difference between with and w/o particles. What made you think that the marked white blobs are particles? The marking with the red circles is too suggestive. I propose to leave them out. In this context: In the ambient with aqueous particles and RH, wouldn't the particles flow together and merge anyhow? I would still believe that even considering that, particle deposition should lead to finer structures compared to evaporating droplets deposited by syringe. What could be the influence of a fast evaporating solvent the structure of the dried deposit? Did you do experiments with others solvents?

line 216 and Line 219: "We were largely able to reproduce our measurement results. . ."
and "with practically no free parameters", what do 'largely' and 'practically no' mean in this context, please, rephrase or specify.

line 326-329: This statement should be appear in the result section before.

Typos:

line 30: the these, cancel "the"

line 52: "stems", turn to plural

line 62: Bannan et al. 2019, add the brackest to year

line 70: "that atomizer method", add "the"

line 486(Figure 1): "for the further divergence", didn't you mean "larger divergence?"

line S48: "d", should be "f"

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