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Atmospheric Measurement Techniques Discussions

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 #1

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The authors present a comparison of volatility calibration methods for the Filter Inlet for Gases and Aerosols (FIGAERO), which is usually used to measure the particle phase composition, but can also reveal information on the volatility of the measured compounds by analyzing the maximum desorption temperature. The authors demonstrate that the widely used syringe deposition method for volatility calibration suffers from systematic errors. The paper discusses an important aspect for the small (but growing) community using FIGAERO-CIMS and is hence within the scope of AMT. It is well written and scientifically sound. I only have some remarks here and there, which should be addressed prior to publication:

More general:

1) Dependence on the solution concentration. The authors demonstrate that by adjusting the diameter in the evaporation model, they can reproduce the results for different solution concentrations. In order to have this in line with the subsequent reasoning about the SEM image, they should show different SEM images with different solution concentrations showing that indeed smaller structures are deposited on the filter in the case of lower solution concentrations. If the diameter really controls this behavior, also a calibration with the atomizer and particles around 1 μm could support this.

2) The authors base their reasoning mainly on the SEM images of the FIGAERO filter for the atomization method and the syringe method. However, they only show SEM pictures for one substance. While I don't doubt their conclusion, other SEM images should be added. The authors make the statement that the vacuum in the SEM could evaporate all the other substances than PEG-8, but how can they than conclude that it is not the SEM sample preparation (i.e. bringing the filter into a vacuum), which causes the structures observed on the filter?

3) Impact of using different calibration methods. When showing the different VBS systems, I would like to see also a comparison to a VBS derived using a group contribution method or a fit to it (as e.g. in Stolzenburg et al., 2018 or Mohr et al., 2019). This would indicate which calibration method is more in line with this widely used approach, which does not rely on a direct volatility measurement.

Minor:

1) P.1, l.31-32: I am missing a short discussion on other volatility measurement techniques, e.g. VTDMA setups. Please add this here.

2) P.2, l.39: I am missing some laboratory studies from the CLOUD team published recently, e.g. Wang et al. (2020, Env. Sci. Techn. & Nature), Stolzenburg et al. (2018, PNAS). Also missing is Mohr et al. (2019, Nat. Commun.). In all these studies the

FIGAERO-CIMS was deployed quite successfully and they could be mentioned here for completeness.

3) P.2, l.49: Also Wang et al. (2020, Env. Sci. Techn.).

4) P.5, l.144: Did you constrain the width of the lognorm fit for the desorption? This could be necessary especially for unknown compounds, which might have isomers or fragments on the same mass yielding a bimodal structure.

5) P.6, l.185-187: If the inlet is initially at a different temperature, the supply of a constant heat rate will yield a different thermogram, as it takes longer to achieve the corresponding temperatures allowing more time for evaporation. Is this considered in the model? And how can we use calibrations performed at one temperature in comparison to measurements at different temperatures? Could the model resolve this?

6) P.7, l.202: Repeat the atomizer solution concentration to put it into the context with the syringe concentrations. Also mention here the mode diameter of the particles used for calibration or even calculate the deposited mass for this type of calibration compared to the syringe method. This would put the two methods into comparison here.

7) P.8, l.231: Instead of mentioning the different scale, I would like to see a fourth panel in Fig 4 showing the filter in the same scale as in Fig. 5c! This would help to directly compare the different structures deposed on the filter.

8) P.8, l.240: Also the larger diameters needed to explain the syringe calibration with model point into that direction. This is an important supportive argument and should be mentioned here.

9) P.8, l.251: Any hints why the different inlet behaves that way?

10) P.8, l.254: Why does it fail for PEG-8? Please elaborate on that.

11) P.10, l.295: Move “A more detailed description of the SOA production is shown in Ylisirniö et al., 2020.” in front of the preceding sentence.

12) P.10, l.299: 200 g mol-1 seems quite low for alpha-pinene HOMs, e.g. Tröstl et al. used 300 amu as mean mass.

13) P.11, l.355: Seems logical, but extremely difficult to realize in the lab. What would be the best alternative?

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