

Interactive comment on “A method for extracting calibrated volatility information from the FIGAERO-HR-ToF-CIMS and its application to chamber and field studies” by Thomas J. Bannan et al.

Anonymous Referee #2

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General: To understand partitioning of (organic) substances in the atmosphere is a key issue. Recently FIGAERO CIMS was developed as a promising method. However to avoid misinterpretations of field and chamber measurements carefully characterization is needed. This manuscript clearly contributes to such a characterization. It addresses the derivation of VP from the conc. maximum of desorption curve in FIGAERO thermograms, utilizing a well characterized reference set of PEGs. The paper is interesting and compact and well written. I suggest publication in AMT after the authors addressed some minor points below.

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Minor points:

p2l7: I am wondering about the Ovadnevaite et al. 2017 reference in the context of gas-phase - particle phase partitioning.

p2l30: What is the reason / are the conditions for observation of Tmax? And related: what is the physics behind the expected (? , Gaussian, p4l47-50) shape of the thermogram? Schobesberger thinks of evaporating particles, but you seem to assume liquid states. Wouldn't the liquids spread and wet the filter fibers? I understand that those details are not really important for the results, but it may help to give an idea/introduction about your imagination of states and processes.

p3l37: Something is wrong with this sentence. Please, check and reformulate.

p4l13f: "200/300" I don't understand what is meant. $45 \text{ min} \times 6.1^\circ$ will get you 275° on top of the RT of 25° , i.e. to 300°C . You may split and separate the info you intended to give into two sentences.

p4l29: I don't understand what you want to say here. How can subtraction of a background fit improve instrumental noise. I guess you have to extend here a little bit more.

p4l47: Tmax for a fragment only reflects Tmax of the parent compound, if the fragmentation happened after evaporation, i.e. in the gas-phase. However, as far as I understood, there could be also fragmentation - at weak bonds - in the particulate phase, isn't it? Then Tmax of appearance of the fragment does not represent the thermal properties of the parent anymore. If so, you have to modify this statement in the manuscript accordingly.

p5l26: I suggest to include/show this fit in Figure 4.

Figure 3: Sample 1-3 where measured during the ramp. Sample 4,5 after the ramp stopped and the system stabilized (at a lower rate of t increase). Does the ramp rate has any effect on the Tmax? I guess so. But then sample 4,5, where not measured at same condition as sample 1-3. Please comment.



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Figure 6, p4l62: I don't understand what is shown here. In the chamber measurement you don't know if the detected formulas are the dicarboxylic acids as tagged? Or did you add the dicarboxylic acids as such. Please, clarify.

Typos etc.

Check co-author name "Krieger" vs "Kreiger"

Check the use of capitals in figure, it should Figure

Check the use of capitals in peg / PEG

p2l10: 'Such measurements "of are" ongoing...'

p3l9: maybe: "> 6 month"

p4l26: too many "averages" here

p4l49: "mass spectrometry and CIMS" is somehow double, you may want to modify this phrase

p5l18: "Tmaxes"

p7l12: the "Foley" reference is incomplete

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