

Interactive comment on “A compact PTR-ToF-MS instrument for airborne measurements of VOCs at high spatio-temporal resolution” by M. Müller et al.

Anonymous Referee #3

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Müller et al. “A compact PTR-ToF-MS instrument for airborne measurements of VOCs at high spatio-temporal resolution”

This is a clearly written, well structured paper detailing the construction and first performance characteristics of a PTR-ToF-MS system for use in airborne measurement. The component parts are described and then several data examples given to highlight this system’s superior performance relative to GC, whole air sample canisters, and equivalent quadrupole systems. The focus here is on the instrument’s impressive capability yet further detail of the external details mass, dimensions, power and inlet layout are also needed. Several other points (raised below) need to be addressed before publication. However, this is a very interesting and informative paper, certainly suited to AMT, and likely the first of many to come from the dataset mentioned.

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1) It would be insightful for the reader to know the overall, mass, dimensions (volume), power requirements. These are critical aspects of aircraft measurement and would indicate the compactness of the system. Perhaps a table can be included? (If the other VOC systems on-board take more space/weight it could be mentioned in the text.). Perhaps the new EUFAR book (Wendisch and Brenguier) Wiley Airborne Measurements for Environmental Research could be used to broadly compare this instrument with others currently in use?

2) There are several occasions when it is claimed that the instrument resolves isobaric ions (e.g. abstract line 4, Introduction, page 5536, line 6, section 3.2 CO₂ and acetaldehyde (line 13,14). While I understand what is meant (two species having nominally the same mass when unit amu mass scales are used) I am uncomfortable with the wording since true isobaric pairs cannot be resolved with this method. While it may seem tedious to always add the caveat given unity mass resolution or nominal mass, it needs to be clear.

3) In section 2.7 the total accuracy of the measurement is given as 5.2%. How accurate was the gas standard itself? Normally this is 5% which would be 0.2% inaccuracy in dilution flows which seems to be too little given normal MFC variance. 4) A nice example is given showing the relative importance of furan to the m/z 69 signal in biomass burning plumes. In order to also represent the more “normal” situation could an additional plot be added to figure 5 to show the typical look of the peak (i.e. isoprene dominated) outside the plume. It may be worth noting that there has been some recent evidence of a soil source of furan (Huber et al ES&T 2010). This information further strengthens the need to separate both isoprene and furan. Finally a reference to furan being “known” to be emitted from biomass burning plumes should be given (page 5542, line 19).

5) Page 5543, line 10. Why was the ToF instrument originally conceived for nominal mass analysis only? Did the instrument represent a step forward in other aspects such as weight, volume, power ? (See also point 1).

6) In Figure 3 it is clear that both methanol and acetaldehyde exhibit significant offsets in their calibration plots. What is the reason for this ? It is mentioned in section 3.1 that power downs increase the background (hence detection limit) of the OVOC yet acetone appears to pass perfectly through the origin?

7) In figure 4 the resolution of the two mass peaks HO_2^+ and CH_5O^+ is shown. To what extent does the protonated oxygen-17 mass contribute to this peak, i.e. molecular oxygen with one atom with mass number 17 ?

8) Figure 10 shows ammonia data. How were these data calibrated, presumably not from the same Apel Riemer VOC standard ? Were inlet losses taken into account. Can it be added to table 1? This molecule is known to be problematic and so greater detail in the quantification method is required.

9) The sensitivity (in cps ppbV-1) given in Table 1 is impressive. In order for future practitioners to compare with these figures it would be helpful to add typical primary ion counts of the system, so normalized counts can be compared. Were these sensitivity figures corrected by the duty cycle correction mentioned in Müller et al. 2013 ? If so it should be stated (also if the primary ions are corrected in this way) so that newcomers comparing initial sensitivity of their system are not disappointed.

10) The paper would benefit from a schematic figure of the component parts (inlet, cal connection, catalyst, MFCs, pumps etc) inserted between figures 1 and 2 or as an extension to figure 1. This would make clear to what extent the inlet was calibrated, show the overall layout better than the 3D sketch and be easier to refer to in the accompanying text.

11) Please explain briefly why glycoaldehyde is believed to be low during wintertime, the reader is currently left wondering why.

12) Could add the words “robustness” to introduction, p 5534, line 23 after “time response.”

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13) Page 5536, line 6. Insert “nominally” before “isobaric.” Also page 5541, line 16.

14) Page 5543, line 27 “signals” should be “signal”

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 5533, 2014.

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