

Atmos. Meas. Tech. Discuss., referee comment RC1
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Comment on amt-2020-496

Anonymous Referee #1

Referee comment on "UAS Chromatograph for Atmospheric Trace Species (UCATS) – a versatile instrument for trace gas measurements on airborne platforms" by Eric J. Hintsä et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2020-496-RC1>, 2021

The paper by Hintsä et al. attempts to present the NOAA UCATS instrument and discuss its measurement quality. The instrument was deployed on a series of missions over the past 15 years and has evolved considerably during this period. This latter fact presents the main problem I have with the paper. It does not describe one state of the instrument to a degree where this could be seen as a documentation or possibly as a reference for other instrument developers. Rather the paper describes in partly qualitative form the evolution of the instrument over time, partly in a narrative manner. As this evolution spans 15 years, it is virtually impossible to document everything with the desired accuracy. My suggestion would, however, be to add more detail to the description of the most recent set-up. In particular I would like to see more quantitative description of some of the GC parameters (column lengths, flow rates, temperatures) and also chromatograms of the GC channels. In particular I missed information on detector non-linearity for the ECD channels and also on in-flight calibration frequency and in-flight precision vs. laboratory precision. If the authors estimate all this to be too much detail, I would also be happy to see this as supplementary material. I also suggest that table A1 should be moved to the main part of the publication, whereas the remaining figures in the Appendix could also be moved to a supplement. Next to these more general comment I have a couple of more specific comments given below. Overall the paper fits well into the scope of AMT and I believe it should be published once these points have been addressed.

p. 3. l. 14-18. I suggest to include some references to ECD detector doping here.

p. 3l. 35: is there a model Nr. for the Maycomm hygrometer?

p.5. l. 34.: as such papers will be around for a longer time period, I suggest not to use term like "upcoming", as this will be outdated in a short time. Rather use planned for the year X. Similar issues are found at other places in the manuscript. (e.g. p.19., l. 29, p.

20., l. 10)).

p.8. l. 48.: I find this "trend-correction" very problematic. While this may be o.k. for SF6 (rather constant trend, very small chemical loss), it is not appropriate for N2O, where due to chemical loss, such a "trend-correction" should be done using a percentage increase. In both cases, this is very crude and it should be clear that some differences may remain which are due to trends.

p. 10. l. 22: and Fig 4: It seems to me that the QCLS data are systematically higher in the troposphere. Have you checked absolute calibration scales? Please also discuss possible ECD non-linearity as mentioned above. This might be able to explain the significant deviations at low N2O values.

p. 12. l. 17 and Figure 5: The correlation between N2O and SF6 looks quite remarkable. Could the authors add some more discussion on this? Are these data from different hemispheres?