

Interactive comment on “The airborne mass spectrometer AIMS – Part 2: Measurements of trace gases with stratospheric or tropospheric origin in the UTLS” by T. Jurkat et al.

Anonymous Referee #2

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The paper describes the setup and initial results of an airborne CIMS instrument measuring SO₂, HNO₃, HCl and HONO. The instrument inlet, ionization region, calibration and background conditions are described and implemented for initial field measurements. The work utilizes a previously known ionization method which it describes in detail in the introduction but does not continue to discuss much comparison of their results to previous work. I have only minor problems with the manuscript and recommend it for publication once the questions/comments below are addressed or amended.

P13568 L22. Replace “exemplarily” with “for example”

P13569 L13. Replace “great” with “large”

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P13570 L23. The author cites a number of nitric acid measurements, although other possible references which are more recent and more relevant to the work here for airborne measurements), for example Le Breton et al (2014). It is worth comparing other works measurements of HNO₃, HONO (Zhang et al 2009 as an example), HCl and SO₂ etc in the results section particularly with respect to instrument statistics such as sensitivity, LOD and e-folding time where possible.

P13572 L7. Change “.Especially” to “, especially”

P13572 L20. Were there any tests done to see how much residual calibrant gas was in the inlet line after the calibration was stopped? If the calibration gas is to flow through the inlet line, surely the sticky nature of the gasses in question would hold a memory for a substantial time period when performing calibrations in flight. This links to a question further in the paper; it would be useful to see in flight calibration data rather than show and talk about implementing laboratory calibrations as they are not representable for atmospheric flying conditions. Further information and graphics are needed for the reader to appreciate the validity of the in-flight calibration system rather than referring to laboratory calibrations. An e-folding time for the in-flight calibrations should also be calculated.

P13573 L18. What is the highest altitude this instrument can be operated at? The 33hPa inlet pressure maintenance will give an absolute theoretical limit, but is there a limit set by the smallest flow you receive due to the dilution flow?

P13575 L5. Have any in-house comparisons of this ion source to a Po210 source been performed? It would be nice to see some evidence of the reduction in noise and in comparison to the radioactive source and also proof that radical backgrounds can be reduced by applying a lower potential.

P13576 L5. Replace “in” with “of”

P13578 L9. The author infers a low resolution of the mass spectra. Can we have an

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idea of what “low resolution is”? Can the author calculate an average peak width of FWHM. It is hard to put the infringement of other masses into perspective if the reader does not know if the resolution is what to be expected of a quadrupole.

P13580 L14. The addition of the calibration directly to the ambient sampling line can cause large errors, as the author states, if the ambient concentration changes. Line 7 states the HNO₃ calibration flow concentration is 2.9 ppb. Figure 7 shows a time series of data with an average concentration of 3 ppb. Correctly, the author states the calibrant concentration should be similar to that of the ambient, although the variability in figure 7 would give a very large error if a calibration was performed during this flight period. The author needs to state when calibrations were performed, how times were chosen to limit the ambient variability and also why it would not be a better option to have a calibration line or overflow through the background line so the ambient concentration was not a factor in the calculation.

P13581 L1. An error of 12-16% is given for the concentrations in the calibration gas, is an overall % error for the measurement available?

P13586 L17. I disagree with the statement that noise calculations are generally determined in the laboratory. The paper is publishing aircraft measurement data, a significantly more difficult task than laboratory data and there is obviously a large amount of work to achieve peak performance of an instrument on the aircraft. I therefore believe that aircraft calibrations and backgrounds should be implemented into the data. The system has been set up to perform in flight calibrations; therefore the data is available to calculate in flight noise levels. The detection limits should also be recalculated using in flight calibrations.

Table 1. For a new instrument technical paper I believe these values should be compared to existing publications to allow the reader to put the statistics into perspective of how the work here has advanced the measurement capabilities of these compounds. I also feel the statistics should be calculated using in flight data.

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Figure 3. This graph would be much clearer to the reader if it had 3 separate y axis so each MS could be looked at separately and compared in the vertical component. It is currently difficult to see much of the information provided.

Figure 4. It would be better to see in flight calibrations and backgrounds here so the reader can evaluate the performance of the instrument in flight. What are the e-folding times of each species.

Figure 5. Could H₂O concentrations be shown on this plot. It is mentioned that it is important to the ionization efficiency so would be good to see that there are no strong correlations of t series with H₂O. Figure 7. The R² of this fit could be useful to show here.

P13593 L12. Replace “stabely” with “stably”

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 13567, 2015.

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