Atmospheric Measurement Techniques Discussions

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Comment on amt-2021-28

Anonymous Referee #2

Referee comment on "Airborne Mid-Infrared Cavity enhanced Absorption spectrometer (AMICA)" by Corinna Kloss et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-28-RC2, 2021

Review of Airborne Mid-Infrared Cavity enhanced Absorption spectrometer (AMICA) by Corinna Kloss, et al.

The paper describes the AMICA instrument which uses OA-ICOS to measure a variety of trace species in the atmosphere using two distinct axes. Details are given on the mechanical, electrical, and optical configuration of the instrument as well as preliminary laboratory calibrations and descriptions of flight campaigns the instrument has participated in.

The paper is well written. However, the focus seems to be on the mechanical and electrical details (10 pages) rather than the optical setup, calibrations, and justifying the uncertainty numbers given in the paper (6 pages). For example, there are several paragraphs that go into detail on the screws and washers used and how they were tightened, but little detail on the optical setup including the cavity mirrors. I am sure a lot of time was spent on the mechanical and electrical details and it's tempting to want to have the paper represent the time spent on the instrument development.
While these details are important to provide the aircraft team when the instrument is integrated into those aircraft, it's not important or necessary to go into that much detail in an instrument paper. The paper would read better if the authors spent as much time discussing the spectroscopic details of the the instrument.

Overall, I think this paper is a good fit for AMT and can be published after some major revisions are made to the sections discussing the spectroscopy and calibrations as well some other minor points that should be addressed. I would also suggest to the authors that much of that detail of the mechanical and electrical hardware be put into the supplementary section or removed.

One major change that I feel I need to highlight upfront is the abstract and text list three

axes and associated molecules but only one of those axes has been characterized for all molecules. Even then, as I point out in the detailed comments, I have real concerns about the usability of some of the data, particularly the CO_2 and CO. Drifts in these molecules are larger or on the same magnitude as expected atmospheric changes and no in-flight calibration system is discussed.

For the second axis, O_3/NH_3, only ozone has been measured. NH_3 has never been observed even in lab and therefore I think it should be removed from the list of molecules.

The third axis is a concept for which you have a laser, but no mirrors capable of measuring at atmospheric relevant concentrations. This should be removed from the manuscript.

Detail Comments:

Line 14: I'll make more specific comments on this later, but it's not clear to me that the instrument can measure all these molecules at real atmospheric concentrations and with required precision or accuracy.

This becomes obvious as you only list precision for one of the axes and say the others are in development (line 20). If they are not fully developed, they shouldn't be included in this paper.

Line 19: Please give the time measurement for the precision as well as how many standard deviations. Is this 1-sigma, 1-second? 1-sigma, 2.5-seconds?

Line 27: Suggest giving the typical flight speed of the aircraft and computing the resulting horizontal resolution. You do this later on in the paper but would be good in the abstract as well.

Introduction: No motivation for measuring these particular molecules is given.

Lines 78 to 88: More detail is required to understand the optical configuration. Are lenses used to focus or collimate the beam emitted by the laser? Even if they are integrated into the laser housing, please give size, focal length or equivalent, part numbers. Details on the 90° deflection mirror: is it round, oval, flat? What cavity mirrors are used. Their R is given in Table 3, but you should also provide the ROC and manufacturer/coater.

Collimating lenses on the detector side should also be listed with details.

Line: 82: You mention using piezo's but no information on why and how well they performed. Was fringing/etalons reduced by using the piezo's and if so by how much?

Laser issues: Was laser feedback an issue? It often is in cavity systems, but I see no mention of it nor any typical optical solutions such as an isolator between the laser and cavity.

Was ASE an issue with these lasers? If the ASE is broad enough it can get around the cavity mirror coating and cause a few percent offset in the apparent light hitting the detector.

Lines 159 to 212: I would move these sections to supplemental or remove. This is information that aircraft operations will want, but isn't needed in an instrument paper. For the main body of the paper, one paragraph could suffice for describing the how the instrument is mounted in the two aircraft.

Line 232: Do you see any pressure oscillations due to the diaphragm pump?

Line 244 and 249: Are the pressure regulation uncertainties the difference between the actual and the setpoint or is pressure varying by these amounts over time as a result of either pressure fluctuations from the pump or fluctuations in the response of the proportional valves or noise on the pressure transducer?

Pressure and temperature calibration: I see no mention of how pressure and temperature were calibrated? Were these devices purchased with calibration. What is the accuracy of that? The zeros of pressure transducers drift. Is that checked periodically?

Section 2.5: Again, an instrument paper does not need to include the jack numbers and which pins are connected, especially when it's all referenced to looking up what the letter/number designations are referring to. Most of this could be shortened to a few short paragraphs. Also avoid referring to everything by its letter/number designation. It is very confusing for someone not intimately used to the instrument.

Lines 438 to 455: I'm confused to which method of fitting is used to fit the spectra shown in section 4.

Line 462 to 464: These are individual ro-vibrational absorption lines, not bands.

Line 477: It's not clear what your resolution is to the problem you are describing with CO_2. Fitting an 80% deep line is challenging. Earlier in the paragraph you state that the fitting is done with full forward simulation which would therefore take into account the effects you describe later. But lines 475 to 477 seem to indicate that you are only fitting this with the approximation described by equations 2 and 3.

The fit itself presented in figure 6 is not very convincing. While the scale makes it hard to read there seem to be numerous differences between the data and your fit not just for the CO_2 line but for the other lines as well. It would be useful to plot the data zoomed in to the weaker lines and/or plot the difference between the data and the fit.

Line 478: You list it in the abstract but it would be good to give the precision numbers in this paragraph as well.

Line 495: The Allen variance plot for OCS looks like what I would expect with noise averaging along the white noise line. For CO and CO_2 however, there is considerable structure in the data that does not average away which is why averaging past a few seconds does not improve the Allen variance plot. For CO_2 there seems to also be a longer drift at least equal to 2-sigma of the 1 second noise. No mention of this is made in the manuscript but these problems make the data unusable for scientific interpretation as the non-white variability (or drifting) is larger than expected atmospheric variability. A typical CO_2 instrument needs to measure sub-part per million to be useful; for CO it's part per billion. You state your group's interest is in OCS. What are the scientific questions that you intend to answer and what variability do you expect to see in OCS? Does the precision of this instrument allow you to answer those questions?

There also is no mention of in-flight calibration that could be used to correct the long term drifts seen in CO_2, though the short term drifts are also problematic. Typical flight instruments would have a gas standard deck that would periodically be used to check instrument operation and correct for slow drifts.

Section 4.2: It seems like a lot of work needs to be done on this axis before it could be used for scientific investigation. You list NH_3 as a molecule but according to line 525 you have not observed it in flight nor tested it in the laboratory. I don't think you should be listing it as a molecule you measure which was implied in the abstract.

For ozone, you're spectral fit does not fit the data. All the lines in the spectra appear broader than your fit which would mean you are underestimating the concentration of ozone by the fit, yet no mention of this is given in the text when discussing why your fits underestimate the ozone mixing ratios. How much ozone is supposed to be in the spectra?

It looks very noisy compared to the depths of the lines, but perhaps it's not that much ozone.

What is the line width of the laser being used?

Section 4.3: This section and references to this wavelength should be removed from the paper. You have not measured, as you state in section 4.3, any of these molecules at atmospheric concentrations, nor can you with the cavity optics you have. Even in the lab you have only put in one gas to show that a laser you purchased in lazing at the wavelength the company said it did. That is not new nor worthy of being published. Having it greatly detracts from the paper.

Line 558: It is hard for me to imagine that with reduced precision from that shown in section 4 that data from this campaign would be of scientific interest. Unless these data are being used in papers this whole section should be removed. It is clear that a lot of work has gone into the building and testing of this instrument. However, a published paper should not contain every thing that went wrong and didn't work and needed to be fixed.

Line 591: There is no von Hobe et al., 2020 in the references. Perhaps you meant 2021?

Table 3: Bandwidth of the detector/preamp combination should be given. I'm assuming it's much faster than the ringdown time of the cavities?

Figure 5: I would move to supplemental as it's unnecessary detail for a paper.

Figures 6 and 10: These figures are hard to read. A third panel showing data minus fit would be useful. The vertical colored lines are not explained in the caption. It would be better for the x-axis of all the plots to be the same. The wavelength scale is going in the opposite direction in plot two compared to plot one making it hard to match up lines.

All figures: Axis labels and numbers are hard to read. Please enlarge the font.

Figure 13: The pressure measurements show fast oscillations that I would think complicate and add uncertainty to the measurements. Is this from ringing in your pressure control algorithm or feedback between the two solenoid valves? As the cavity pressure and temperatures are the most important for fitting the data, I would suggest just showing those and perhaps the enclosure temperature. Or if you want to show all of

these making more sub plots so that the details of the measurement can be seem. For example, do you see a similar ringing of temperature in the cavity?