

Review of “The Airborne Chicago Water Isotope Spectrometer: An Integrated Cavity Output Spectrometer for Measurements of the HDO/H₂O Isotopic Ratio in the Asian Summer Monsoon” by Clouser et al., <https://amt.copernicus.org/preprints/amt-2024-98/amt-2024-98.pdf>

The manuscript describes an update of the well-known Chicago Water Isotope Spectrometer ChiWIS for measuring the stable isotope ratio HDO/H₂O ratio in water vapor. Due to the extreme technical challenges to precisely measure the H₂O isotopologues in the dry mid-/upper troposphere and lower stratosphere, only a very small number of relevant instruments have been developed. The paper thus gives an important contribution and as being well structured and focused, it's a pleasure to read.

Still I have a couple of issues that need to be clarified (see minor concerns) and one general comment.

General comment:

- 1) I like the paper very much. It's very detailed and nicely describes the instrument and its performance during three campaigns. However, one crucial issue is missing, namely the measurement of an isotope standard, e.g. SLAP diluted to low mixing ratios. This is usually provided and some/many PIs just use the measurement of a standard (in regular intervals of minutes to ~1 hour) to verify and demonstrate the quality (uncertainty) of the data. I see that this would be a very difficult or maybe impossible exercise during flights on Geophysica / WB57 (with considerable additional uncertainties), but in the lab such a calibration should be feasible, although not simple with the voluminous cavity. I do not request such an additional lab test, but you should add a section where you discuss this point and provide all the information you gained (e.g. at the AIDA chamber) to enhance the trust in the data and your uncertainty estimates in section 6.2., also as the isotope data you show in Figure 14 are on the very high (isotopically rich) side of the data collected so far.

Minor concerns

- L. 26/27. This increase with altitude in the TTL is not a general feature, but (likely) indicates the recent influence by convectively lofted air masses. Please correct.
- L. 29. Add citation.
- L. 41. Add after table title “(references further below)”.
- Move the sentence “Limited in situ isotopic measurements of water vapor isotopologues ... and briefly described below.” above the table.
- After L. 53, add something like “commercially available in situ laser isotope instruments (e.g. by Picarro or Los Gatos) are not adequate for H₂O mixing ratios of below ~1000 ppm”.
- L. 64. ... and the inadequate/poor vertical resolution of ACE-FTS (although for a satellite instrument it's quite good)
- L. 91. You state that “OA-ICOS is necessary to satisfy this requirement ...” so that only OA-ICOS can do the job? The OF-CEAS technique invented by Danielle Romanini could do it, too, but was (to my knowledge) never been applied in the mid-infrared. And there may be other potential techniques.

- L. 118. As MIPAS will not be known by all, use quotation marks ... “MIPAS dome”.
- L. 127ff. The title list of the subsections A – F does partially not agree with Figure 4 and the subsections further below. Please make them (more) equal.
- L. 132 or at L. 155. Specify the lasers (manufacturer, type).
- Figure 4 (or somewhere else). Give dimensions and weight of ChiWIS ... and it would even better to have an instrument spec sheet (table) with most relevant parameters, i.e., also measurement range, response time, measurement speed, ...
- L. 175. Section ??
- Figure 5. Give dimensions.
- L. 188. To avoid confusion with Figure 5, I would use M-in and M-out (or so) for the two mirrors, instead of M1 and M2.
- L. 190 (or around L. 200). “tiny fraction”. Do you know how much of $(1 - 0.9998) \cdot 100\%$? Or what is the transmission T ?
- L. 217. Here you write that the surface roughness by Optimax is ~ 0.3 nm, but on L. 210 you write that such a roughness already causes a decrease in the path length from 7.4 to 2.8 km. And already in the abstract you wrote that the path length is 7.5 km. I don't get it here.
- Table 3. Explain the parameters / abbreviations.
- L. 235ff. If I interpret this para correctly, the “set of eight identical wedges” is identical with the nonax component in Figure 6. First, use an identical name / specification and secondly, shortly explain how this component increases the signal-to-noise ratio by a factor of 3 (by an improved focusing onto the detector?) and by how much you have increased the number of photons on the detector?
- L. 241ff. The sentence “Even though better noise characteristics could be had from a smaller, colder detector, calculations indicated ...” I don't fully understand. And the consequence? Next time you would use a larger detector?
- L. 333. “... can be changed prior flight through ...” or it's during flight via access from the ground?
- L. 394. “... that valve angle is incrementally stepped up to 90 degrees over the course of several minutes” ... in order to allow the top valve to independently control the cell pressure? Please, add a subclause.
- L. 396. Exchange “very wet” with “humid” or so. Above ~ 400 hPa all airmasses are quite dry.
- L. 416. What means: “Spectra ... are rebinned in software”? It's apparently not a simple “binning” (including how many individual data points?)!? Shortly specify or refer to the relevant section.
- L. 437. Shortly comment, whether there was a power reduction of the pump due to overheat.
- L. 450. Exchange “less than two seconds even at the lowest concentrations” with “of 0.? s (at x ppm) and 2 s (at 3 ppm)” or so.
- L. 460. This means that the contamination occurs between the campaigns and in the lab? Please specify and explain.
- L. 472/473. Where the procedure and equation come from? Add citation or whatever.
- L. 487. Here you write 15 ppm, in the figure caption 18 ppm and in L. 494 18.5 ppm.

- L. 490-495. Give the ranges where you average over 2s, 5 s, and 10s. Cold/depleted is quite subjective.
- L. 496. “raw data (gray)” ... I see black.
- Section 6.1 (Precision). In this section, some things I don’t understand.
 - o L. 512. “The measurement precision σ_{χ} is defined as the standard deviation of the mixing ratio as a function of time”. What means “as a function of time”? It’s the standard deviation e.g. in a 60 s interval or so? ... and includes “atmospheric” variability (not “natural”). Please specify.
 - o L. 516. You write “for all field data sets the H₂O measurement precision is significantly larger than the detection limit”, but in L. 520 you write “measurement precision of 3.6 ppbv. This is roughly half the detection limit” which likely shows that during flight atmospheric variability dominates, right?
 - o Assess how the instrumental variability in the lab compares with the one at flight and then you could/can differentiate between instrumental and atmospheric variability.
 - o You should better explain all this based on the numbers shown in Table 4, e.g. “see line/column x/y in Table 4”.
- Table 4. Add in the caption: “ χ is the H₂O or HDO mixing ratio”. The unit in column 4 (χ) is wrong.
- L. 543. Exchange “natural” to “atmospheric” everywhere in the text and again, you do not well distinguish between instrumental and atmospheric noise. Fig. 8b indicates that ChiWIS basically works during flight as in the lab, because the atmospheric variability of HDO is much smaller than the instrumental noise. You can thus argue that the much higher Allan variation in H₂O (blue trace) compared to the lab is atmospheric variability ... and the “removal” of the fringe at longer integration times you have described ... I would stress the great performance in the field, that is, the “external Piezo” (aircraft, vibration,...) that makes your instrument even better in the field than in the lab.
- L. 558. I guess, the difference is mainly due to the general higher noise level or lower signal, respectively, which let us see the fringe at higher averaging times and lower Allan deviation, resp.
- Sect. 6.1.2. Usually one compares the measured noise (e.g. when the laser is off) with the theoretical noise sources, such as shot noise and Nyquist noise which can be calculated easily (in contrast to the 1/f-noise). Can you add this, please?
- ... and then you have nicely separated the different noise levels: electrical noise (is apparently constant or only depends on temperature), optical noise (alignment dependent) and atmospheric variability. It would be cool to see this noise partitioning somewhere at the end in the conclusion or presented in a table or even with additional columns in Table 5 (it’s up to you).
- Section 6.2. All/most the uncertainties discussed and shown in Table 6 cannot be assessed by the reader. Thus, write at the beginning, how the uncertainties have been retrieved. For instance, some can simply be measured such as the cavity length uncertainty, others are given in the literature (e.g. Hitran), others come from retrieval algorithms and some may be estimated.
- L. 619. Again “very wet” in the atmosphere means everything above 25000 ppm or so.

- Section 6.3. Here you don't discuss the cavity flush time (it can simply be calculated based on the pump volume flow ... add this), but the cavity or better instrument response time, which is also dependent on memory effects of the sampling lines and the cavity. Please correct!
- Fig. 9. Give the temporal resolution of the shown data. The ChiWiS data appears smoothed and with an averaging time of >2s and I actually wonder (having the short cavity flush time and high precision in mind) why you don't provide the H₂O data with ~2 Hz (the cavity flush time / instrument response time). It appears at least, that the "data averaging" smears out fine atmospheric structures.
- Fig. 11. Write what low, medium, high N₂O m.r. mean in ppb/ppm/%
- L. 688. Write where the bandpass filter has been placed. Refer to L. 175 and to Fig. 5.
- Fig. 12. Give the fit results (red line) in the graphs.
- Fig. 12 caption. Bottom panel. Add "data without correction" or so.
- Fig. 13 caption. After "... because contamination", add "(likely from memory water at the cavity wall)" or so
- L. 737. Exchange "real" with "atmospheric"
- Fig. 15 caption. Delete "natural".
- Fig. 14/15. Understand it correctly, that in Fig. 14 only the stratospheric data (i.e. above 400 k) are shown, but in Fig. 15 that are very close at the troposphere and at theta-levels below 400 K. Please detail this a bit. I also ask as at 17.5 km you should have seen pot. Temperatures close to 400 K.
- Table 2. Add theta (K), see my question before.
- Fig. 14. I know it's an AMT paper, but clear is that the dD-values are high compared to most other data taken at these potential temperatures (altitudes). Thus, at least a short discussion is required. Do you e.g. believe that your data / profiles are already representative and your instrument is so accurate that you can claim that the picture has to be revisited a bit, that is that water may enter the stratosphere more heavy than previously thought ... as we have the most accurate device that has been existed on this planet ...
- L. 765. Do you think of silcosteel, which can be used for aluminum? Please write.
- L. 769. Instead of "clumped", is "multiply substituted" or "doubly substituted" the better word?
- Fig. 16. If you can measure even at 3500 ppm, the cavity is apparently not optically thick or lines saturated and the instrument could potentially be used in almost the entire free atmosphere, right? ... see my hint to the spec table at the beginning at, to L. 785 where you write that it can be operated only above 12 km and to the entire conclusion.
- Conclusion. I haven't checked all other relevant publications, but your instrument is likely the most precise and maybe most accurate one worldwide, no? If so, write or at least add a sentence that outlines how ChiWiS performs compared to others.