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Atmospheric Measurement Techniques
Discussions

Interactive comment on "A two-channel, tunable diode laser-based hygrometer for measurement of water vapor and cirrus cloud ice water content in the upper troposphere and lower stratosphere" *by* T. D. Thornberry et al.

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We thank the reviewer for their close reading and insightful comments on the manuscript and have endeavored to address them in order to improve the accuracy and clarity of the paper.

Response to specific comments:

(Section) 2.1 / (Page) 8276 / (Line) 21: units of weight, although often colloquially given in kg, should be N. Or change "weight" to "mass."

We reflexively referred to instrument weight because that is the term commonly used by the aircraft personnel ("weight and balance")–mass is indeed the correct term and the text has been changed. Providing weight in N would seem to be less intuitive for readers.

2.4 / 8279 / 5: suggest removing "37s" as it is not a common descriptor. The interested reader will see what the package is if he/she looks online for the specific detector model, which is given.

The 37s package does seem fairly specific to Teledyne-Judson. The "37s" reference was omitted as suggested.

2.4 / 8280 / 2: "direct absorption" is more commonly used to refer to a detection method in which modulation is not used. I think what the authors mean here is better described by something like "DC," in contrast to the "AC" components produced by the modulation. And this brings up a question: how is that DC component measured from the detector signals?

This is a very valid point. We do typically refer to the signal as "DC". The text has been changed from "direct absorption" to "DC" and description added that the DC signal is the signal from the photodiode low-pass filtered to remove the modulation.

2.4 / 8280 / 3-4: the asymmetry described is really a result of the fact that the observed 2F signal is a combination of the second harmonic pure wavelength modulation signal and the first and third harmonics coupled through an amplitude modulation effect. The authors might consider changing the wording to something like " . . . due to combined wavelength modulation and intensity modulation effects," and reference P. Kluczynski and O. Axner, "Theoretical description based on Fourier analysis of wavelengthmodulation spectrometry in terms of analytical and background signals," Appl. Opt. 38, 5803–5815 (1999), or similar.

The text has been changed to “arises from contributions to the second harmonic signal from the first and third harmonics due to co-modulation of the laser intensity with the wavelength modulation” and the suggested reference added.

2.5 / 8280 / 10: Is the airfoil-shaped pylon angled to null nominal aircraft pitch angle effects?

The long axis of the airfoil is aligned with the aircraft longitudinal axis. We do not have CFD modeling of the flow around the Global Hawk to estimate the influence of the fuselage on the angle of the flow (which is likely to occur) and so opted to not correct for the average angle of attack (as has been done for inlets mounted in the nose or in the fore section of pods on other high altitude aircraft). We use the range of measured aircraft angles of attack to estimate the uncertainty in the eTW measurement from a non-aligned flow angle. The discussion of IWC measurement uncertainty includes a statement on the negligibly small contribution to the TW sampling uncertainty from the small angle of attack of the Global Hawk (< 4° in the TTL). Statements were added to the text specifying the alignment of the pylon and TW sampling inlet.

2.5.2 / 8282 / 28: What is the largest particle the authors would expect to be ingested by the instrument in TTL cirrus sampling? Would such a particle likely be fully evaporated?

The various previous measurements of TTL cirrus crystal size typically show mode diameters near 10 μm, but the distributions extend out past 100 μm (albeit with very low concentration/frequency). A 50 μm particle sampled into our inlet will strike the 180 °C tubing at the bend at over 100 m/s, where it may stick or shatter. From cloud probe measurements, ice crystal shattering typically results in large numbers of small fragments, which will carry significant energy and likely experience multiple wall collisions of their own, possibly sticking and rapidly vaporizing. There are no adequate models to predict the shattering distribution or collisional energy transfer for ice particles impacting the hot tubing walls, but the evidence is that the shattered fragments are likely to face rapid evaporation in the hot sample flow, on the wall or in the additional 40 cm of warm tubing past the 180 °C region. Discussion of the fate of larger ice particles in the inlet was added.

2.6 / 8284 / 13: Are the stepwise calibration values always changed in the same manner / direction? If yes, would the authors speculate on possible systematic errors associated with that procedure?

By encoding the calibration sequence in a loop, the calibrations are indeed typically performed with the same sequence of flow steps, although the sequence has been altered on occasion. The key is that the on-board calibration sequence is the same when we run it on the ground during the lab calibrations against the MBW as during flight. We do not see any systematic error in the on-board calibration on the ground vs the MBW calibration, and it is the consistency between the ground and in-flight calibrations that provides evidence for the stability of our MBW reference calibration during flight. Statements highlighting this were added to section 2.6 and section 4.

3 / 8285 / 27: Referring to Figure 7, why do the data show only values down to 2 ppm when the calibrations were done down to 0.5 ppm? Also, on Figure 7, why is N2f given in “arbitrary” units? Since it is a normalized quantity, it shouldn’t have units (for example Volts/Volt). And the values should represent a real, understandable quantity, based only on the conditions in the cell (pressure, temperature, concentration, pathlength), laser modulation parameters, and (perhaps) lock-in and/or A/D gains.

Individual calibrations were not carried out over the entire range–calibrations were typically 0.5 to 100 ppm or 2 ppm to 500-2500 ppm. The calibration used for the example in Fig. 7 had a low value of 2 ppm and a high value of 2141 ppm and was chosen to illustrate both the strong line and weak line calibrations. Text was changed to specify that the figure shows an individual calibration.

The units of the N2f are technically scaled ADC counts. The normalization is 2f * (I_{ref}/I_{DC}), where I_{ref} has been arbitrarily taken as 1, and the normalization is calculated as 2f/I_{DC}. As we do not rely on highly accurate knowledge of the modulation depth and relative gains on the DC and 2f signals we do not attempt to assign a meaningful spectroscopic quantity to the signals. The meaning of the signal instead is derived from the calibration against known mixing ratios of H_2O in the absorption cell. The “(arb.)” has been removed from the axis label in Fig. 7 (now Fig. 8), making it consistent with Fig. 8 (now Fig. 9).

3 / 8286 / 23-25: I may have missed it, but it seems you refer primarily or exclusively to the cell temperature, not the gas temperature. The temperature stability appears to refer to the cell temperature. Is the gas temperature (inside the cells) measured? If so, is that the number that is being reported to be stable to 0.03 deg C? If it is not being measured, why was that choice made and what might the authors speculate could be the errors associated with uncertain / variable gas temperature?

The discussion of temperature here is specifically the stability of the cell temperatures, measured at three points (mirror block, mid-cell, laser/detector block). The primary issue of temperature stability we have found for the instrument is related to changes in the physical light path due to thermal expansion that cause the optical fringes to shift in the spectra, not changes in the spectroscopy itself. The heated inlet tubing and various elements (fittings, valves) along with the low heat capacity of air serve to thermally disconnect the temperature of the air entering the optical cells from the ambient sampling temperature. Calculations and lab tests show that the 20 cm length of inlet tubing for each channel located inside the isothermal sub-enclosure housing the absorption cells is sufficient to reduce changes in the temperature of the sample flow induced by variability of the main instrument enclosure temperature (10-20 °C in flight) to less than 1 °C, which results in less than a 0.4

3.1 / 8288 / 2-7: Does sample temperature belong on this list? In laboratory calibrations, did the authors ever introduce a sample with a variable temperature into the temperature-stabilized system to quantify any effects?

As noted in the response to the previous comment, the sample flow temperature is

close to isothermal due to heat transfer to the sample flow from the inlet tubing within the temperature-controlled optics sub-enclosure. This result was further verified by the lack of any observable change in the calibration with and without the inlet and flange heaters operating, which results in a > 25 °C temperature change in the sample flow in the pylon. Discussion was added to section 3.1.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 8271, 2014.