

Interactive comment on “HAI – A new, airborne, absolute, twin dual-channel, multi-phase TDLAS-hygrometer” by Bernhard Buchholz et al.

Anonymous Referee #1

Received and published: 28 June 2016

This manuscript describes a multi-channel spectroscopic measurement system for the airborne detection of water vapor and total water content. The authors describe in detail the requirements for such a system, the setup characterization of their system, and example data from flight testing of their system. The manuscript has strong technical merit and represents a significant contribution to scientific progress. However, the manuscript suffers from some balance issues with respect to material, and the writing has some fluency issues. While I strongly believe this manuscript is ultimately worthy of publication, I recommend that the manuscript be returned to the authors and reconsidered after the address of some of the below issues.

Major comments:

- The authors spend a proportionally large amount of the manuscript on what I

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would consider background. Section 2 seems to act as a second, instrument-centric introduction. While this background is generally accurate (save some specific comments below), it seems more fitting for the start of a review article, which does not seem to me to be the intent of this manuscript. I would recommend greatly condensing section 2 and combining it with section 1 to create a better proportioned background section.

- I would shift the extra room in the manuscript to adding additional details with respect to the instrument system itself, which I found to be somewhat sparse at points. In Sect. 4.1, the authors assert “Many of the modules contain innovative developments, which are or will be published individually to prevent an overload of technical details in this paper.” I believe that this argument is better suited for analysis-centric papers, whereas being a technical manuscript in AMT, this is exactly what this paper should discuss
- There are numerous places (I tried to list them in the comments below) where assertions were made that were not fully explained or where additional evidence would better support the assertions. For example, the authors make a strong argument for the “calibration-free” method for their instrumentation. While I agree there is sound theoretical evidence for this approach, I feel it would make a stronger argument to have, not a calibration, but a validation for the approach other than the internal validation which is susceptible to common biases. This also seems to be easy to accomplish for the closed-path systems. Other examples are listed in the specific comments.
- The manuscript also needs additional editorial support before it will be ready for publication. While most of the paper is intelligible, there are many instances of awkward language and some instances where this inhibits comprehension. I have denoted some cases below, but it is not an exhaustive list.

Specific comments:

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- Line 51: missing parens
- Line 58: 800 km/hr?
- Line 93-94: cite aircraft by organization (and perhaps in addition nationality) for clarity
- Line 94: It seems there should be a better HIAPER reference
- Line 100-101: meaning of “aircraft in the contemporary working equipment” unclear, translation issue? Could not tell from context.
- Line 105: “besides multipliable deployments” confusing word choice
- Line 196-197: Cite Thornberry et al. (2013) or Thornberry et al. (2014) for in flight cal system
- Line 206: NMI?
- Line 350's: The authors should provide additional information about the lasers (e.g. manufacturer, power) and the optics (e.g. focal lengths).
- Line 390: does 10-4 refer to optical depth?
- Line 405: Relative internal vs external path lengths for the $2.6 \mu\text{m}$ laser? Only the $1.4 \mu\text{m}$ laser is specifically mentioned here. Is the $2.6 \mu\text{m}$ optics the same?
- Line 422: Cite 2014 reference for optical airborne pressure measurements here
- Line 487: This doesn't make sense to me. Where is the 70 ms coming from? If only 1/6 of the spectrum is necessary, why scan more than that?
- Line 501-506: Why does WMS specifically require lower fringe levels? This requirement would involve many factors, and the authors do not present any evidence or reference for the claim.

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- Line 509: ramp -> ram?
- Line 511-512: OP 6x greater 1s residual than CP? This doesn't make sense with the Sect. 5.2 results
- Line 512-513: How does the wind speed affect this? Is it pressure deviation? Cavitation causing misalignment? Wall effects from the airfoils? Have you determined this from performing speed experiments on the aircraft?
- Line 513-520: This seems an odd place for this discussion. Also, much of the temperature and pressure difficulty with open path systems arises from the uncertainties in the temperature and pressure fields themselves, not instrumental artifacts, which would result in the same issue here. Spectral effect in temperature and pressure can be minimized through spectral line choice, which would also be similar for either method.
- Line 543-546: This precision is for $1.4 \mu\text{m}$ or $2.6 \mu\text{m}$ system? What is the precision of the other? Maybe a table to summarize which precisions/detection limits correspond to which channel
- Line 598-601: it would be nice to see a figure with a greater summary of the intercomparisons between the 4 channels than the singular example shown in figure 10
- Line 604-609: this statement should be made with the caveat that the 20
- Line 637-652
 - The instrument in (c) has flown many campaigns on the NASA ER-2 (see JPL Laser Hygrometer, or JLH).
 - The authors omit the NASA DLH hygrometer (Diskin et al., 2002), which has frequently flown missions for over 20 years.



- Line 644-645: again recommend that aircraft be referred by their affiliation, not nationality. HAIPER -> HIAPER.
- This 2

- Line 678-684: this would benefit from an additional figure
- Figure 1: I'm not sure I see the need for this figure, other than to show that the same laser can be used for multiple channels simultaneously. I recommend removing it. Maybe the intent of this figure could be added to Fig. 4 by adding the parasitic water and stabilization channels.
- Figure 2: This would be more beneficial if this were split into two bottom axes, then zoomed in on each water line, showing only the lines immediately around it.
- Figure 5: I don't feel this figure adds significantly to the manuscript, and I recommend removing it.
- Figure 7: cursing -> cruising

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Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-167, 2016.

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