

Interactive
Comment

Interactive comment on “The AquaVIT-1 intercomparison of atmospheric water vapor measurement techniques” by D. W. Fahey et al.

D. W. Fahey et al.

david.w.fahey@noaa.gov

Received and published: 16 August 2014

Anonymous Referee #2 Received and published: 24 July 2014 The manuscript as written discusses an important ground facility based blind intercomparison activity held at the AIDA chamber in Karlsruhe, Germany. In this intercomparison, many different instruments were intercompared, including a "core" set which have extensive heritage and experience measuring water vapor in the UT/LS and TTL, from airborne platforms. The impetus for this intercomparison was, in large measure, the large disagreements among these instruments in prior airborne campaigns. One primary result of the intercomparison study, reported for the first time in this manuscript, is that the differences observed in flight cannot be explained by the performance in the lab setting at the AIDA

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



chamber.

The paper is well written and clear. The appendices, which describe each instrument, add valuable information to the understanding of this activity. Many instruments were required to operate, by the nature of the facility, under conditions not identical to those experienced in flight. Conditions within the chamber ranged from those found in the UT/LS/TTL to some that are not generally found in the earth's atmosphere. For example, conditions included water vapor mixing ratios below 1 ppm at pressures exceeding 150 hPa.

The authors would like to thank the reviewer for his/her careful and thorough reading and supportive comments on the manuscript.

A statement is made at the end of section 6.3.1, "Although mixing ratios in this range occur rarely in the UT/LS, these measurements help define the detection limits and performance limits of the instruments." This statement may not be true in general, and in some specific cases during this intercomparison. Many instruments are optimized for performance in certain regimes while possibly sacrificing performance in other, less important, regimes. To force all instruments to make measurements under conditions never encountered does not necessarily generate information on their detection limits, accuracy, precision, and performance under more realistic conditions. And such a strategy may well penalize some instruments that are so optimized (for example, optical measurements) with regard to instruments that are not optimized. I suggest rewording or removing the statement.

In general it can be helpful to study instrument performance beyond atmospherically relevant conditions. This is the case for "not optimized" instruments but also for "optimized" instruments since they can better demonstrate the limits and validity ranges of optimization. It was the aim for AquaVIT to make this as transparent as possible. For newly developed instruments this is especially true since AquaVIT-1 provided opportunities to identify potential uncertainties sources, which in some cases are hard to

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



distinguish at higher concentration levels. Therefore, we have modified the sentence in Section 6.3.1 as follows:

"Although mixing ratios in this range occur rarely in the UT/LS, these measurements help to identify potential sources for uncertainties."

The lack of an agreed-upon reference causes additional concerns, as each instrument is being asked the question how well does it agree with the others, not the more important question – how accurate is the measurement being made. It is understandable that the collective reference strategy used in this intercomparison was the only fair choice, but it should be made very clear that agreement with the ensemble is not intended to imply accuracy in the strictest sense.

Agreed. This is already stated clearly in Section 6.2.c, 6.5, and 8 and now more clearly in the Abstract and Section 4.

Regarding the collective reference method, the method for determining precision is not as clear as it could be. At the end of section 6.2 (in subsections d and e) a method is described which uses the ensemble mean slope vs time as a reference. From the difference between an individual instrument's measurement during that segment and the ensemble mean, one would expect to see a contribution not only from the Gaussian width of the distribution but also from the varying offset (unless all slopes are the same). This additional difference can't, in general, be lumped into the Gaussian, but it is not clear how the authors handled that time-varying offset. The actual equation used is given in the caption for Figure 4 and it doesn't seem to include the offset. I suggest making these sub-sections more clear and perhaps inserting the appropriate equation(s) in the main text body.

We agree that the difference Gaussian derived from an individual instrument time series and the ensemble mean time series for a segment could be skewed if the former has a significantly different mean slope compared to the ensemble reference. This possibility, already mentioned in Section 6.2e, albeit not clearly, could occur if one of

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



the core instruments is not performing well. Mathematically, this situation represents an additional linear term added to a true probability distribution function. Calculations show that the error in the Gaussian width is less than 5% as long as the amplitude of the linear offset is less than about 40% of the Gaussian peak height. The figure below shows an example for a 40% offset, which causes a very visible difference in the PDF but only a 5% change in the width. A linear offset of 40% is much larger than observed for any instrument time series.

See attached figure

The caption of Figure 8 now includes the clarification:

“The symmetry in the Gaussian fits for each instrument indicates that the mean slopes comprising the ensemble mean are similar and, hence, slope differences do not significantly affect the Gaussian widths.”

Some of the figures (notably 4-6) are too small to be easily read and understood. I suggest remaking those figures with readability in mind.

If the figure panel sizes are increased to fit the page width in publication, legibility will not be an issue. A request will be made to AMT to increase figure sizes.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 3159, 2014.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

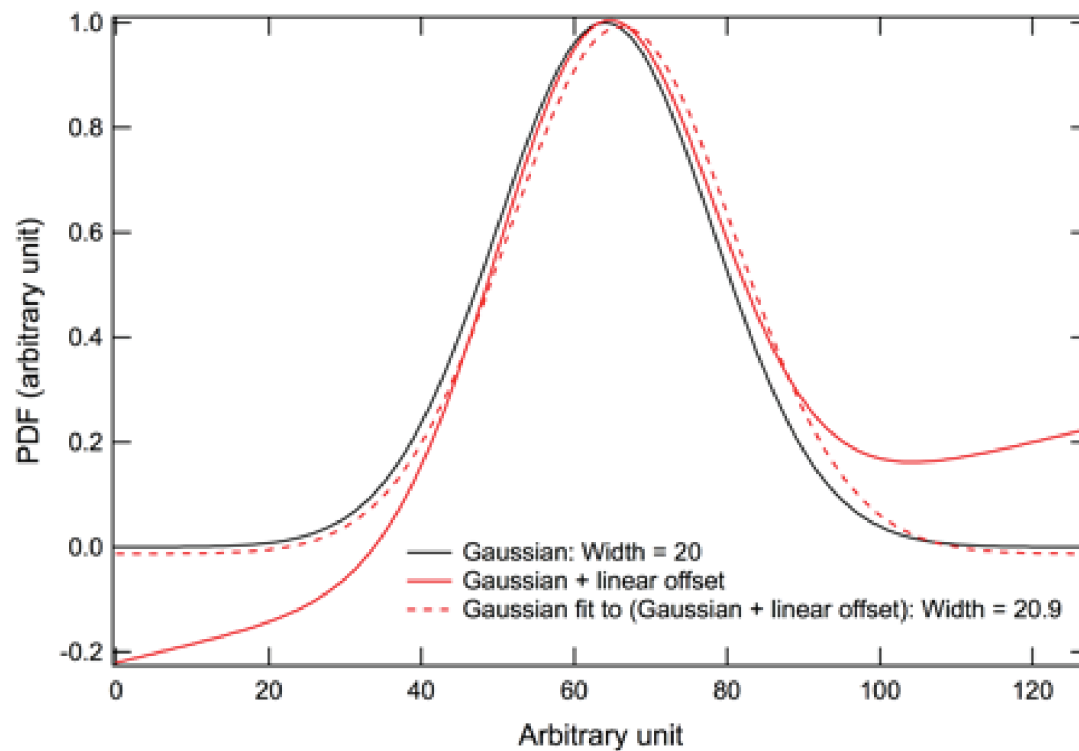
[Interactive
Comment](#)

Fig. 1.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)