Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2018-189-RC1, 2018

Atmospheric Measurement Techniques Discussions

Interactive comment on "Spatial Heterodyne Observations of Water (SHOW) from a high altitude airplane: Characterization, performance and first results" *by* Jeffery Langille et al.

Anonymous Referee #1

Received and published: 27 August 2018

GENERAL COMMENTS

This nice paper describes the level 0 and level 1 processing scheme for a new limb-sounding heterodyne spectrometer called SHOW. It proceeds to describe the first test campaign, where the instrument worked well enough such that first water vapour retrieval results could be gained that compare favourable to co-located radiosonde measurements.

The paper is well written and its scope fits very well into AMT. It describes the individual steps of the processing in a very approachable manner (commendable!), but leaves often small but important details unmentioned or implicit. A minor textual revision that ensures that all algorithms have been described with more rigour could turn this good paper into a great one. Specific notes below have been made below for the most important issues.

Further, there are several comments listed below, which must be addressed before I can recommend publication. I do not foresee any problem on the author's side to do so easily.

SPECIFIC COMMENTS

page 23, Section 11.2:
I find the use of the term "degraded vertical resolution" with respect to the pointing uncertainty misleading. The vertical resolution of the measurements is primarily determined by the FOV/PSF of the instrument. In case that the line-of-sight cannot be actively managed, aircraft movements during a measurement also worsen the vertical resolution. Another factor in determining the vertical resolution of the final product (compared to the measurement) is also a potentially employed regularisation.

However, here these factor are intermingled with an uncertainty in absolute pointing, which seemingly stems from inaccurate pitch readings by the ER-2 attitude system. Further factors may also contribute here, e.g. thermal contraction of the instrument. Such a bias in the pointing would be assumed to be constant during the interferogram acquisition. Such a bias would generally cause a shift in derived features (in contrast to a smearing out) as well as likely quantitative errors due to the wrong assignment of measurements to tangent altitudes and corresponding problems with pressure and temperature assumptions.

From the variance in radiation, you properly estimate an estimate for the precision of the employed line-of-sight angle, which shouldn't enter the vertical resolution. In contrast, I miss an estimate for the accuracy of the line-of-sight angle, which obviously can be calibrated on ground, but may change slightly upon mounting the instrument or may change due to thermal effects. Also, can the wings move in relation to the

Printer-friendly version

Discussion paper

main body of the aircraft depending on speed and prevalent winds? I also miss an estimate for the intra-interferogram variation of the pitch angle, which might be derived from highly-resolved aircraft pitch measurements that would, in fact, worsen the vertical resolution of the measurement.

page 25, Section 11.3
The section does a good job of giving a good overview of the employed technique but skimps on the detail. I assume that the computed correction factor for each spectral sample is of a multiplicative nature. While the method obviously works, it remains a question of how well this models the underlying failure and what error remains in the final data for proper error analysis.

The error may likely be assumed to be multiplicative in interferogram space due to the applied flat-fielding with potentially remaining higher-order effects (see Sect. 6). Such a multiplicative error in an interferogram sample will cause a sinusoidal error in the resulting spectrum. Due to the nature of the multiplicative error, I'd expect the influence of the pixels close to the ZOPD to be higher causing the error pattern to be of a more low-frequency nature and thus coloured noise.

So, I am looking for a physical motivation to the correction technique. If the effect is stable during this window, a point-wise multiplication in interferogram space should solve the issue, which would correspond to a convolution in spectral space.

Please motivate the correction technique and describe its application better to make clear its nature and implementation.

page 30, lines 1ff:
The agreement between the radiosonde and the SHOW retrieval results is indeed remarkable. However, the error bars due to noise errors are not applicable here, as noise is likely one of the smallest error sources of your retrieval compared to many of the instrumental uncertainties you (partially) corrected for and other inaccuracies by the forward model (e.g, line strength uncertainties, single/multiple-scattering) or other

influencing factors (uncertainties in pressure, assumed aerosol profile, solar radiation variation, horizontal homogeneities along the line-of-sight, . . .).

The actual extent of the error bar is quite difficult to see on the plot. A logarithmic plot might be helpful here.

It is certainly not expected that the SHOW retrieval results agree with any other measurement within error bars induced by noise alone. The retrieval paper (Langille 2018) has, e.g., a sensitivity towards aerosol of 1 ppm, which also fully explains discrepancies at higher altitudes. For lower altitudes, the larger variability of water vapour in the stratosphere may, as rightly noted, explain observed discrepancies.

The paper should take systematic errors identified by Langille (2018) into account for the comparison. The plots should make identifying the errors better.

MINOR REMARKS

page 1, lines 33ff:
Amongst others, especially ACE-FTS has also significantly contributed to our knowledge about water vapour in the UTLS, partially due to its significantly higher resolution compared to the listed instruments. See for example doi:10.1029/2008JD009984. In contrast, it lacks in temporal and spatial coverage, obviously.

page 6, lines 5:
Reference of (Harlander, 1990) is not defined.

page 6, lines 32:
Are the ranges of the integral correct? With the variable transformation from sigma to kappa, I'd expect a corresponding change of integration range. In addition, I suspect also a missing factor. This makes only sense under the assumption of an appropriately chosen passband filter, which is only introduced later.

page 9, line 24:
Why is a nearest neighbour interpolation and not a linear one between the upper and

lower neighbour used? Especially in case of only a few bad pixels, this should reduce the error introduced due to the increase of radiation with decreasing tangent point altitude.

page 9, lines 30ff:
It is not clear what correction is performed on the interferograms with respect to the changing aircraft pitch. I interpret it in the way that not the interferogram is changed, but that each interferogram is associated with a tangent point altitude determined from the current aircraft pitch.

page 9, lines 30ff:
In what way is a possible bias between the LOS of SHOW and the given pitch angle of ER-2 determined? While the pitch variations of ER-2 can be readily applied, any systematic bias needs to be ruled out to properly assess the quality of the tangent altitudes.

page 10, lines 4ff:
It is unclear, what correction is described here. Section 11.2 describes a means of estimating remaining uncertainties in pitch/LOS after taking ER-2 pitch data into account, but does not describe correcting these. Different effects that are difficult to differentiate may cause the variation in radiation (changing scenery/clouds as well as inaccurate or imprecise pitch values from ER-2) and may a correction in contrast to an error estimation difficult.

page 10, lines 19:
For a non-linear retrieval, the matrix K should be marked with an index i or as being dependend of the current iterate x_i as $K(x_i)$ or $F'(x_i)$. with F' being the Jacobian of F.

page 12, lines 25ff:
From the description, it is unclear what uniformity or non-linearity correction is performed. The sentence that no uniformity correction is done contradicts a statement of

page 8 line 23.

page 17, lines 3ff:
One may also estimate the noise from the imaginary part of the spectrum after phase-correction. You typically operate on magnitude spectra. Could one still compute the phase from calibration measurements and use this to both reduce and estimate the noise?

page 27, lines 8f:
It is never spelled out precisely, how the correction is applied. It seems to be a multiplicative factor?

page 27, line 17f:
There is a difference in scales between the two panels of Fig. 16. As such it is not apparent that the effect is minimal in wavenumber space. A delta plot would show that better in both instances. Especially the absorption feature at 1364.25 seems affected, but the l.h.s. might be affected by aliasing more. However also at 1365 is a visible different place with a large correction.

page 29, line 10f:
As shown in Fig. 18, the regularisation is insignificant and doesn't control or seemingly even influence the resolution.

page 29, line 10:
It is unclear what "damping" refers to and how it relates to the regularisation. One typically uses a trust-region method like Levenberg-Marquardt which contains something like a dampening factor. Here it sounds like the dampening would affect the result of the retrieval, which weren't the case for Levenberg-Marquardt.

TECHNICAL CORRECTIONS

page 10, line 14:
The cite of Rodgers should be "described by Rodgers (2000)".

page 10, line 19ff:
The used mathematical notation is formatted differently in the equation as in the main text (cursive vs. regular and probably in a different font).

page 17, Figure 7:
Especially the "(c)" in image (c) is barely readable. Potentially a white rectangle behind the letter or a "path effect" of matplotlib would help here.

page 18, Equation (9):
There are two consecutive "+" in the formula.

page 24, Figure 13:
Especially the "(b)" in image (b) is barely readable.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2018-189, 2018.