Replies to reviewers of Technical Note: 30 years of HIRS data of upper tropospheric humidity

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1 Reply to Review 1

Page 2, line 2:
The central achievement of our paper, that is, the regression beween HIRS/4 and HIRS/2 channel 6 brightness temperatures is independent of the special choice of humidity. Equation 3 is valid for both UTH_w and UTH_i. That we are especially interested in supersaturation is due to historical reasons; the first author works on this topic since the late 90s. However, we make it clear in the paper that the regression is valid for both kinds of humidity measure, see text directly before eq. 3 in the original paper.

Page 2, line 4:
The central wavenumbers (cm^{-1}) for the two channels are as follows:
Channel 6: The designed central wavenumber is 733 for both HIRS/2 and HIRS/4. However, there can be small difference from two different instruments due to slight shifts after launch and other onboard factors.
Channel 12: The designed central wavenumber is approximately 1480 for HIRS/2 and 1530 for HIRS/4.
We add this information in the paper.

Page 3, line 8:
We will mention the change of IGFOV from 20 km to 10 km in the revised version. Of course, the effect of such a change had to be corrected for with the intercalibration and this was done.

Page 3, line 21:
We agree that it might be convenient for the general reader to explain shortly the main ideas behind the retrieval formula. We will do this in the revised version, see text after equation 1.
In short: It is evident that we need a signal from a water vapour channel, i.e. channel 12. It is less obvious why we need a signal from the CO2 channel, channel 6. In fact, ch. 6 is not used to diagnose CO2, but rather to determine a temperature in a thick layer in the atmosphere. A water vapour and a temperature measurement are the usual ingredients for the determination of a relative humidity. Comparing channel 6 with other temperature channels that can potentially be used, Jackson and Bates (2001) found that channel 6 had better correlation and smaller RMS error than other channels.

Page 6, line 20:
Cloud clearance means that only clear measurements are selected.

Page 6, line 22:
The T_{12} difference between a nadir measurement and a measurement at 30° for channel 12 is approximately 1.2 K in average. However, all the measurements used in this study are limb-corrected (Jackson and Bates, 2000).

Page 8, line 24:
It is improbable that the increasing T_{12} variances in the 2000s have anything to do with the halving of the IGFOV in HIRS/4. If this would indeed be the case we should expect a similar increase in channel 6, i.e. increasing T_6 variances. However this is hardly the case, see Figure 8, middle panel.

In order to see whether the HIRS data are consitent with other data sets we checked temperature data of ERA-interim and NCEP re-analyses. Both re-analyses show consistently that the temperature variances are increasing on all pressure levels from 300 to 700 hPa in those areas where we find a statistically significant increase of UTH. The temperatures themselves increase mostly, consistent with expectations from global warming. These results corroborate our statement in the paper that the significant increase in UTH originates mainly from the increase in temperature variances. We have added a paragraph on this in the paper.

2 Reply to Review 2

2.1 Investigation of super-saturation conditions

Although ice supersaturation is one of our main interests and is accordingly mentioned in the introduction, the study is NOT dedicated to the investigation of such conditions. The study is dedicated to the regression of channel 6 brightness temperatures between HIRS version 4 and 2 in order to enable the application of equation (1) in a consistent way. That is, the study is dedicated to the derivation of equation (3) with appropriate regression coefficients.

This is the reason why the paper is called a technical note and not a study or an investigation.

2.2 Clear-Sky bias

We agree that the Clear-Sky bias can be a big problem, not only in the tropics, but as well in mid-latitudes (John et al. 2011). We have to take this into account in future studies on ice supersaturation, because this bias is especially large under moist conditions. However, although ice supersaturation can occur (and ofted does so) in clouds, classically, ice-supersaturated regions are cloud-free and in a state where cirrus clouds can form later when supersaturation increases further beyond a nucleation threshold (145% RHi and more). Thus, a study of ice-free ice-supersaturated regions *before* cirrus clouds form should be possible with IR data. For the more general case, i.e. ice-supersaturation including clouds and contrails one should consider microwave data, which can help to mitigate this bias problem.

For the regression from $T_{6/4}$ to $T_{6/2}$ this problem does not arise at all.

For the first application in Section 3 it could be a problem if the cloud-free fraction would change significantly from the 80s to the 2000s, either in total amount or in pattern. We computed the fraction of grid boxes without valid data (because of presence of clouds or other reasons) in the daily files for both considered decades. This fraction changed from 54% to 50%. On a monthly base, the fraction with no valid data changed from 5% in the 80s to 4% in the 00s. This shows that the data are fairly complete on a monthly basis even if there are about 50% gaps on a daily basis. Within a decade we can expect even smaller gaps (although not tested). These numbers provide no strong argument for a clear-sky bias in our application of the regression to the determination of decadal means.

We added this discussion to the end of section 3.

2.3 Extratropical application of the UTH retrieval

It is true that the Soden and Bretherton retrieval is better in the tropics than in the extratropics. This is also evident from inspection of figure 1 of Jackson and Bates (presenting the UTH Jacobians) where the weighting functions for the mid-latitude profiles reach quite low altitudes in the troposphere. Nevertheless, for studies of global distributions of UTH and in particular if interest is in long-term changes there is no other choice than using satellite data. The longest time series we have is the HIRS data. Other long data sets like METEOSAT MVIRI/SEVIRI data and microwave data (183.1 GHz) suffer from the same problem. Data with stated higher vertical resolution (AIRS, IASI) are probably better in the mid-latitudes, but they have relatively short time series. The best will probably be, to combine data with high and low vertical resolution in the extratropics and to try to learn something that cannot be learned from one data source alone.

For the current paper this is not really an issue, since the derivation of equation 3 is independent from such considerations. However, we agree with the reviewer that some words on this issue can be spent when further plans are discussed in the final section. See the new text in section 4. Fortunately, the Jacobian's maxima rise in altitude with increasing moisture and the probability to get a signal from the boundary layer or even the ground gets smaller. Thus the problem is presumably smaller when the interest is focussed on the moist tail of the humidity distribution.

2.4 Jacobians

Jacobians (weighting functions) are needed to compute the regression coefficients, a and b in equation (1) from radiative transfer forward calculations, using given profiles of temperature and water vapour concentration (or any other humidity measure). Jackson and Bates (2001) have shown how this works, and how the regression coefficients, rms errors and so on differ for various choices of the Jacobian. There is no need to repeat this here. We do not deal with profiles and do not perform forward calculations. The question as it is posed is irrelevant. The only thing that may be added is that the regression coefficients are those given in Table 2 of Jackson and Bates (2001), an issue that seemed self-evident to us.

2.5 Validation

We agree that self-consistency is not validation. But we do this for a simple reason: Self-consistency is possible, validation is not (see, e.g., Oreskes 1994). Validation implies that there is a truth to which data can be compared. Unfortunately, there is no truth in this respect. All data sources, like radiosondes and other satellite instruments, research flight campaigns, long-term campaigns like MOZAIC, etc. have their own problems and error sources. Since there is no truth there is no validation. Self-consistency and consistency between different data sources is all that we can hope for and this is difficult enough. However, since the expression "validation" is typically used while it is not possible in a strict sense, it can be assumed that it actually means achieving consistency between different data sets.

G-VAP is an international effort just to do this, that is, to achieve consistent data sets. If validation is mentioned in G-VAPs work programme, then a benchmark data set serving as "truth" has to be agreed on, see below. G-VAP is a five years project, and certainly much more time will be needed to reach all the goals in its work programme. The reviewer cannot expect that the problems faced by this international effort will be solved in a single technical note.

The technical note helped contributing to a couple of the G-VAP questions. But we are not sure why reviewer 2 mentioned about G-VAP in the validation paragraph. Though G-VAP includes validation activities, a large portion of the project is in inter-comparisons. The inter-comparisons in G-VAP is beyond the scope of this technical note's work. "Validation" is usually done using in situ measurements (defined to be the "truth" in this case). We don't think that this technical note should be expected to carry out a validation using global in situ measurements as it is not an article about a new retrieval method. Besides, cross-checking of UTH retrievals among several methods using TIGR radiosonde profiles was provided by Jackson and Bates (2001).

2.6 Non-gaussian pdf of UTH

It took us a while to understand this question. But reading the quoted paper clarified the issue. In this paper, John et al. (2006) state that average UTH values (say, monthly averages for instance) are usually calculated from

$$\ln(UTH) = a + b\,T_b \qquad (*)$$

(a slightly simplified version of equation (1)) in the following way: First the available values of T_b are averaged and then equation (*) is applied to compute a corresponding average of UTH. Mathematically:

$$\langle UTH \rangle = \exp[\frac{1}{N}\sum_i (a + b\,T_{b,i})].$$

They state further that this is only allowed if UTH is gaussian-distributed. (N is the number of data in the sum).

Hopefully, nobody ever applied this method. It is NEVER allowed to interchange a linear with a non-linear operation, neither for gaussians nor for non-gaussians. In this case the summation and the application of the exponential function must not be interchanged. The only correct method to compute average UTH is to apply (*) for any single values of T_b first and then to average the resulting values of UTH. Mathematically:

$$\langle UTH \rangle = \frac{1}{N}\sum_i \exp(a + b\,T_{b,i}).$$

This is the way we did it.

We believe also that the average values are computed correctly in the quoted paper by Schröder et al. (2014). At least these authors show averages (i.e. mean values), e.g. in their figure 3 (rhs), figure 4, etc.

Finally we would like to draw the reviewer's attention to several papers which show that we are well aware of the fact that UTH has not a gaussian distribution, see the references.

2.7 Minor questions and questions of style

We changed the wording where necessary and reasonable. However, questions of style are a matter of taste and we are not willing to accept lessons about scientific vs. non-scientific style. We are sufficiently long in this business to know what is appropriate and what is not.

page 5873, line 1:
Of course there is METEOSAT as well, but not with global coverage. But we can mention it.

page 5875, line 9:
The $T_{6/4}$ uncertainty can be computed from the noise equivalent radiance which is given in the NOAA-N Prime brochure (Appendix A) as 0.24 mW/(m^2 sr cm^{-1}). Assuming the Rayleigh-Jeans approximation for a wavenumber 733 cm^{-1} results in a noise equivalent $\Delta T_{6/4}$ of 54 mK. As you can see in Figure 1, the original differences $T_{6/2} - T_{6/4}$ have amplitudes of about 100 mK, while the residuals of the regression have amplitudes smaller than 50 mK, that is smaller than the noise equivalent temperature. This will be added to the revised version.

line 18:
Discarding measurements showing water supersaturation is of course necessary. It makes no sense to keep them. And it is no problem at all. The fraction of measurements with water supersaturation is of the order $< 10^{-4}$ in 4 months of data that we checked, namely January and July 1980 and 2000.

line 22:
All the measurements used in this study are limb-corrected (Jackson and Bates, 2000). As we use the same procedure for both decades, there is no impact at all.

page 5879, line 13:
It is correct that there are not always two observations per day over a single $2.5° \times 2.5°$ grid box. But this is not the point here, probably our text was a bit unclear in this respect. We will delete the statement within the brackets in order to avoid a misinterpretation. What we want to say is that the data we show, decadal averages, consist generally of measurements taken from a variety of satellites both on ascending and descending tracks. These satellites have different orbital drifts. The mixture of data sources mitigates potential problems that would arise from orbital drifting if our data would come solely from one satellite. We agree that the clouds and their diurnal cycle are a problem for analysing time series, in particular in the tropics. But for computing a decadal average the problem should be much smaller than for monthly averages.

References

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