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Atmospheric
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Discussions

Interactive comment on "Ice supersaturation as seen from TOVS" *by* K. Gierens et al.

Anonymous Referee #2

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General Comments

The paper presents an analysis of TOVS data with respect to ice supersaturation in the upper troposphere. It is innovative since TOVS data has so far not been used for this application to the referees best knowledge. The authors' main conclusion is that ice supersaturation can indeed be seen in the TOVS data. This is concluded from the observation that the histogram of retrieved UTHi does contain a significant fraction of supersaturated values, and exhibits an exponential drop-off behavior at supersaturated values.

The only significant shortcoming of the paper at present is that it does not yet include a sufficient error analysis. Without error analysis, the evidence presented in the paper is not strong enough to support the main conclusion, as shall be explained below.

The problem is the exponential form of the retrieval equation (Eq. 2 of the paper). Because of this, the tail of Gaussian errors on the radiances will be mapped to an

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exponential drop-off in UTHi, so that it will look like supersaturation. Basically, there are two errors on the radiance that have to be considered:

a) Radiometric noise, as given by the instrument documentation,

b) "Regression noise".

Error (a) is obvious, but error (b) requires some explanations. The retrieval equation (Eq. 2 of the paper) is obtained by performing a linear regression on a set of atmospheric states and associated radiances. There is not really such a simple relationship between UTHi and brightness temperature, therefore the regression is quite noisy (Figure 6 of Jackson and Bates). This "noise" comes from the dependence of the radiance on atmospheric variables other than the ones considered in the regression (for example the shape of the profile). When applying the retrieval equation this has to be kept in mind. If this "regression noise" follows more or less a Gauss distribution, then its tail will also be mapped to an exponential drop-off by the retrieval equation.

In the paper, the authors demonstrate that adding additional radiometric noise (error (a) above) will change the observed slope of the exponential drop-off, but only slightly. However, it is not possible to conclude from this effect of additional noise to the effect of intrinsic measurement noise, since the dependence of the slope of the exponential drop-off on the noise level is expected to be non-linear. Furthermore, the magnitude of error (b), as discussed above, is expected to be significantly larger than the pure radiometric noise, and it has so far been neglected by the authors.

A comprehensive error analysis could be carried out as follows:

1. Take a set of atmospheric states that is thought to be realistic, but without oversaturation (for example the TIGR-3 dataset).

2. Use a forward model (for example RTTOV) to simulate radiance for each atmospheric state.

3. Add instrument noise according to the instrument documentation.

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4. Apply the retrieval equation and obtain the oversaturation statistics. There is expected to be apparent oversaturation with an exponential drop-off. The slope of this can be compared to that obtained from the HIRS data. If the slope for the HIRS data is significantly less steep than for the simulation, it is a proof of oversaturation.

My suggestion is to add such an error analysis to the paper. It would make the arguments presented more compelling and the conclusions "bullet proof".

Specific Comments:

1. Eq 2 and 3:

Suggestion: Point out already here that these equations apply only to limb corrected data.

2. Page 3, first paragraph about cloud clearance:

I suggest to throw in already a hint here, that the impact of clouds will be checked later on in the paper. This simply because that is such an obvious issue.

3. Section 3.1, last paragraph:

What is potential contrail coverage derived from?

4. Figure 4:

It is slightly confusing that humidity content increases to the left in this figure (with lower brightness temperatures), while it increases to the right in Figure 2. I suggest to either invert the x-axis, or make a comment about this in the legend.

5. Section 4.3, last paragraph:

See general comments. Additionally: Please make a statement how the chosen value of 1 K compares to the HIRS characteristic noise for these channels.

6. Same location:

It is stated that the results are the same, even if no cloud clearance at all is used. I find this hard to believe, since clouds do have a significant impact on IR radiances, hence they must also have an impact on retrieved UTHi. My recommendation is to re-check this.

7. Page 7, bottom left paragraph:

"Such a peculiar distribution, which must be the distribution of the atmospheric temperature at the peak altitude of the channel 12 kernel function, cannot be derived from Gaussian natural temperature fluctuations or temperature retrieval errors."

This argument does not hold, because Channel 12 is a humidity channel, not a temperature channel. The point is that the peak altitude of the Channel 12 kernel moves up and down with changing humidity content. For higher humidity it moves to higher (colder) altitudes, thus measuring colder brightness temperatures. The BT distribution is more closely related to the humidity distribution than to the temperature distribution.

The claim that Gaussian temperature and humidity fluctuations cannot lead to Gumbel brightness temperature distributions would have to be proven by model simulations.

8. On the Gumbel distribution:

This point is really very interesting. It would be great if the authors could elaborate more on possible explanations, why it shows up here. This just to satisfy the readers curiosity.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 299, 2004.