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Interactive Comment

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

K. Gierens et al.

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Ad General Comments (error analysis of regression error)

The regression noise, as evidenced by the scatter of the dots in Fig. 6 of Jackson and Bates (2001) could indeed be a problem for this analysis, since atmospheric humidity profiles with UTH differing by a factor of about e can result in the same T_{12}. However, we have shown in the paper that the TOVS UTHi retrieval underestimates the presence of ice supersaturation to a large extent, hence the question whether cases with UTHi > 100% but without ice supersaturation in the profile could affect the slope of the distribution of $U_i - 100$% seems to be irrelevant. We have demonstrated in Sect. 4.2 using the Lindenberg radiosonde data, that it seemingly does not happen that RHi profiles without any supersaturated layer produce a supersaturated UTHi. In order to be sure on that (and to avoid the lot of additional work the reviewer recommended) we asked Darren Jackson about his experience when making the UTHi product from the TIGR-3 profiles. He answered (Jackson, 2004, private communication):

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> When I computed the UTHi from the TIGR-3 profiles, I placed a requirement that the RHi could not exceed 100% at any level of the profile data. Therefore, computing UTHi from the RHi data did not give any supersaturated values from the TIGR-3 profiles. When I removed this RHi requirement from the TIGR-3 profiles, I found 104 of the 1614 Tropical + Mid-latitude profiles have UTHi exceeding 100% which would be 6.4% of the TIGR-3 profiles.

We see that UTHi in excess of 100% does only occur if there is indeed supersaturation in the atmospheric profile. And even in this case the supersaturated layer must fill a substantial part of the region where the weighting kernel has high values (see Sect. 4.2). This additional constraint makes it rather improbable (even including radiance noise) that we get UTHi > 100% without actually having ice supersaturation in the profile.

We have added a few sentences on this in Section 4.3.

Ad Specific Comments:

1. and 2. ok.

3. In the paper we write that the potential contrail coverage can be derived from local temperature and humidity. We also give its physical significance. Readers interested in more details should consult the quoted literature.

4. We make a comment in the legend of Fig. 4.

5. In a paper by J.R. Eyre (1987) we found the noise levels (for NOAA-7, should be similar for NOAA-14): these are 0.54 K for channel 6 and 0.85 K for ch. 12. These noise values include errors from pre–processing, cloud–clearing as well as radiometric noise. Hence, assuming 1 K in our paper gives a worst–case estimate. The paper by Eyre is now quoted.

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6. It is *not* stated that the results are the same, it is rather stated that the slope of the exponential does not change when varying degrees of cloud clearing are applied. The absolute numbers of events of course get lower when a more rigorous cloud clearing is applied. This still lets open the problem of why there is no noticeable influence on the slope of the distribution. The most probable explanation of this "miracle" is that most clouds are cleared already by the $T_6 - T_4$ criterion (Sect. 1 after Eq. 3), which — in our case — already throws away 84.3% of data contaminated by high clouds. A note on this will be included in the paper (Sect. 4.3).

7. There is no difference in the radiative transfer principles and equations from one channel to the other. Although channel 12 is a humidity channel, T_{12} measures the temperature at the peak of its weighting function (well, approximately). Of course, the altitude of that peak varies with humidity in the atmosphere. At every moment in time the collection of points where the kernel peaks forms a "surface" (altitude of the peak as function of time, latitude, and longitude). This "peak surface" varies in time up and down accoording to the humidity field. But at every moment there is a temperature field attached to that surface. The temperatures on the peak surface in areas characterised by UTHi > 100% obviously do not follow a Gaussian distribution, as shown in Figure 5. This is not surprising: Whereas we would expect instantaneous Gaussian temperature fluctuations for small areas, we do not so for larger areas. For example, in areas as large as 300×300 km^2 the instantaneous temperature and RH fluctuations follow more closely a Lorentz (or Cauchy)–distribution, that is characterised by heavy tails (Gierens et al., Ann. Geophys. 15, 1057-1066, 1997). There is no reason that a set of temperatures obtained from different times and various regions should follow a Gaussian distribution. The same argument holds for other large data sets like MOZAIC or MLS where we have obtained a similar exponential law for the distribution of ice–supersaturation, without having underlying Gaussian distributions of T or q (see e.g. the T and q distributions, Figs. 6–9, in Spichtinger, P., K. Gierens, W. Read, 2003: The global distribution of ice-supersaturated regions as seen by the Microwave Limb Sounder. Q. J. R. Meteorol. Soc., 129, 3391–3410. There are no Gaussians at all,

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yet the supersaturation follows almost exactly an exponential law, see Spichtinger, P., K. Gierens, W. Read, 2002: The statistical distribution law of relative humidity in the global tropopause region. Meteorol. Z. 11, 83–88.)

We do not claim that Gaussian T and RH fluctuations cannot lead to Gumbel brightness temperature distributions, we rather *see* that we do not have a Gaussian distribution of T_{12} in Fig. 5, and when we — as usual — identify T_{12} with the temperature on the peak surface, the evident conclusion is that this temperature is not Gaussian distributed but follows the peculiar and un–named distribution shown in Fig. 5. Since this all does not exclude that local Gaussian T–fluctuations and radiance noise contribute to the peculiar distribution of T_{12}, we have added a corresponding half–sentence at the end of the statement that the referee criticised.

8. Indeed it would be great when we could give at least a tentative explanation for the show–up of the Gumbel distribution here. Unfortunately, we do not have any idea (aside from the analytical derivation). The question is even more intriciate when we see that the distribution in eqs. 9 and 10 is actually not a genuine Gumbel distribution, but one with a cut–off. In extreme value statistics only the genuine Gumbel occurs. Hence a trial of an explanation via extreme value statistics will probably fail. Alas!

The Kernel function is a Gumbel distribution as soon as the absorber profile varies exponentially with altitude. As this is the case for water vapour (in a very good approximation), the kernels for water vapour lines have the shape of the Gumbel distribution. In this case this has nothing to do with extreme value statistics and it is merely a coincidence.

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