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Atmospheric Chemistry and Physics Discussions

Interactive comment on "First Odin sub-mm retrievals in the tropical upper troposphere: humidity and cloud ice signals" *by* M. Ekström et al.

Anonymous Referee #2

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Review of the manuscript entitled " First Odin sub-mm retrievals in the tropical upper troposphere: humidity and cloud ice signals" by Ekström et al.

The manuscript by Ekström et al. presents the first measurements of humidity and ice cloud signals derived from the microwave radiometer SMR aboard the ODIN satellite. The space project launched in 2001 was initially dedicated to both astronomical and, stratospheric and mesospheric scientific issues for an estimated lifetime of 2 years. After 5 years of operation, the ODIN-SMR is still properly working and new programs have emerged. The present paper for instance shows that, when considering the measured brigthness temperatures at tropospheric tangent heights at 500 and 540 GHz,

signatures of clouds can be detected along the line-of-sight. Indeed, at these frequencies and heights, the signal is saturated unless a cloud is present acting as a cold black body. A look-up table between the expected clear-sky brigthness temperatures and the measured lower brigthness temperatures combined with a very detailed 3D radiative transfer code including scattering helps quantifying the amount of relative humidity and flagging ice clouds. A detailed error analysis both in term of systematic and random components shows the impossibility of using information contained in single measurements. Although a great care has been taken in the calibration procedure, a statistical comparison with MOZAIC data in the tropics shows ODIN-SMR relative humidity much more intense above 120% than MOZAIC, with the reverse below 20%. The monthly evolution of the tropical relative humidity at 200 and 150 hPa together with ice cloud signals are globally and qualitatively in agreement with what we expect i.e. high amounts above intense convective systems.

The manuscript is well written (with the exception of Delta-T or TB depression), the abstract synthesizes the globality of the work (but the seasonal and longitudinal variability of the relative humidity and ice clouds are not mentioned), Figures are clear (except Fig. 5 and the color Tables of Fig. 7-9), references are well presenting recent works on mathematical tools for retrieving this kind of information. Nevertheless, the paper is greatly focussed on the methodology while the scientific outputs are very weak. In other words, a qualitative approach is proposed while a quantitative discussion might be expected. For instance, Fig. 7-9 are presented with no real comparisons with the locations of convective systems and/or results from other sounders as AURA/MLS. No recent papers on the same subject focussing into quantitative comparisons are referred to. Furthermore the results are fully dependent on the temperature fields taken from the ECMWF. A slight cold or warm biais in the temperature data set might definitely change the conclusions or at least this systematic bias needs to be studied. In conclusion, this paper is certainly worth publishing after some revisions (detailed below) have been performed.

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Major points

1. Methodology

The methodology is based upon the difference between the measured and the expected brightness temperatures (Delta-T ot Temperature depression) combined with a very detailed 3D radiative transfer code including scattering that helps quantifying the amount of relative humidity and flagging ice clouds. As the authors mention it, it is very sensitive to the absolute brightness temperature.

a) In case baseline undulations are present, spectra are averaged within a 100-MHz band. This indeed reduces the random noise but not the systematic biaises that can be as high as ˜8% at 500 GHz and ˜6% at 540 GHz. Could you first comment on that and second visualize the 100-MHz band you selected in Figure 4? Could you also explain why N2O and O3 lines are visible when ice cloud signal is strong in Figure 4?

b) Weighting functions are negative. Could you explain why? Kernels are the derivatives of y (spectra) wrt x (species) and are of the form dy/dx. Thus their units should be K/% (% being the unit of relative humidity), or maybe K/%.km if you integrate over the vertical. Could you comment on that?

c) Figure 5 is basically very difficult to understand. Is the x-axis Delta-T? If yes (put it on the Figure), could you quantify the value of Delta-T for which saturation 100% reaches a weight of 1?

d) P. 8657, L. 16. the term "window channels" is employed without being defined. I guess it corresponds to the channels covering the 100-MHz band.

e) In the quality criteria section (3.5), you write "the TB range of the transfer function exceeds 15 K for 501 and 11 K for 544 "(you forgot GHz). I guess you mean Delta-T instead of TB because on Figure 3 the range of TB does not exceed 15 K at 500 GHz and 10 K at 540 GHz. Could you also comment on that and finally clearly explain both the method and the quality criteria you have actually used?

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Interactive Comment

2. Error budget

a) The error budget is performed in great detail. Unfortunately, the systematic component of the total error appears not to be taken into account when interpreting the results. Indeed, the authors explain that they have been obliged to average a huge amount of measurements in order to lessen the random error. I understand this point, but quid of the systematic error that is indeed unaltered by the averaging process since some components can reach 8.4% at 500 GHz and 6.4% at 540 GHz.

b) The analysis is based upon ECMWF data. It stated that ECMWF temperature has good precision (1%). On the other hand, since ODIN does not actually measure temperature, there is no other possibilities except using meteorological analyses, or maybe re-analyses. But it is also well known that some biases might exist in the analyses. At least, it seems very important to check whether the methodology used is sensitive to a warm or a cold bias in the temperature field. This should add another component (Temperature) in the systematic error Table.

c) A cold or a warm bias in the temperature might also have an effect in the statistical comparisons between ODIN-SMR and MOZAIC (Figure 6) by reducing the fraction of occurrences of Humidity greater than 120% and increasing below 20%. The Figure 16 caption mentions "mu and sigma (see further in the text)" but there is no mention of these variables in the text. Furthermore, how many points are used in this comparison for Odin? I guess MOZAIC is 455,000.

d) Another parameter might also be listed in the sytematic error component, namely the pointing offset. Is it negligible or do the authors forget to mention it?

e) Quid of the total systematic error that is not mentioned at all in the paper? And how does that affect the statistical results you have obtained?

3. Results

a) I find the ODIN-SMR results presented in Figures 7-9 very encouraging. Color

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Table might be changed in order to highlight the great dynamical range of the Relative Humidity fields. But the interpretation of the results and/or their consistency with papers published recently on that domain is very poor. For instance, I could propose to the authors the following recent references for putting more insights in their results.

References: (http://mls.jpl.nasa.gov)

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b) The authors could also use Outgoing Longwave Radiation data for highlighting regions of deep convections and check whether Relative Humidity fields from AURA/MLS are consistent with their statistical analysis.

c) Finally, the authors do not mention whether the nature of the cloud can be discriminated from the information contained in the ice cloud signals.

Minor points

a) I would write in the whole manuscript "ice cloud" instead of "cloud ice".

b) P. 8656, L. 17: Explain "level 1b data".

c) P. 8659, L. 28: "s" is missing after "consist".

d) Section 4.1.2. Table 5 should be Table 1.

e) "GHz" are missing after "501" and "544" in P. 8658, L. 20 and P. 8663, L. 21 and 22.

f) P. 8665, L. 6. "to" is missing between "found" and "be".

g) Caption of Figure 1. Add "Vertical dotted lines represent the frequency of the lines considered in the spectral band and the associated species".

h) Figure 6: "Occurrence" instead of "Occurance".

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 8649, 2006.

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