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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A cloud filtering method for microwave upper tropospherichumidity measurements" *by* S. A. Buehler et al.

S. A. Buehler et al.

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We would like to thank both reviewers for their constructive comments and suggestions. Reviewer comments are repeated here, our reply is typeset in bold font.

Anonymous Referee #2

General Comments

I have reviewed the manuscript 'A cloud filtering method for microwave upper tropospheric humidity measurements,' by S. A. Buehler, M. Kuvatov, T. R. Sreerekha, V. O. John, B. Rydberg, and P. Eriksson. The method is based upon an absolute threshold and also upon the difference in the brightness temperature between two of the 183.31 GHz channels of the Advanced Microwave Sounding Unit - B. The method is tested on

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Discussion Paper

a mid-latitude winter case-study.

The paper then builds upon the work of two of the authors who derived a method for estimating Upper Tropospheric Humidity using 183.31 GHz brightness temperatures. They examine different sources of bias in UTH climatologies, such as the dry bias that will occur when cloudy regions are excluded from the climatology.

This paper is in general well written. It is laid out in an orderly fashion, and the methodology is sound.

Throughout the paper the term 'brightness temperature' is used. Strictly speaking, the brightness temperature arises from applying an antenna pattern correction algorithm to the antenna temperature, which in practice is determined by applying the calibration coefficients to the instrument counts. Have the authors applied such a correction to the antenna temperatures? Is so, please give a brief description of the technique. If not, it will suffice to make a short statement to the effect 'While we have not applied an antenna pattern correction to the antenna temperatures, we will refer to these as brightness temperatures.'

We added two new paragraphs:

'These infrared and microwave instruments passively measure thermal radiation emitted by the atmosphere and the Earth's surface. The radiances, which have the physical unit Watt per square meter, Hertz, and steradian, are measured by a receiver, which is calibrated in terms of brightness temperature (T_{B}) in Kelvin. In this article we will use the terms radiances and brightness temperature more or less as synonyms, with the distinction of the different units.

More strictly speaking, the instruments do not measure the real brightness temperature directly, but the antenna temperature, which must be bias corrected and antenna pattern corrected. The microwave

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data used in this article were calibrated to brightness temperature with the AAPP software package, which applies these corrections. (For details on AAPP and the corrections applied see Labrot et al. (2006).)'

The reference is to: Labrot, T., Lavanant, L., Whyte, K., Atkinson, N., and Brunel, P.: AAPP Documentation Scientific Description, ver- sion 6.0, document NWPSAF-MF-UD-001, Tech. rep., NWP SAF, Satellite Application Facility for Numerical Weather Pre- diction, http://www.metoffice.gov.uk/research/interproj/nwpsaf/ aapp/NWPSAF-MF-UD-001 Science.pdf, 2006.

Specific Comments

Page 7513, lines 20-25. This section presents an explanation as to why a negative deltaTb18 value could be indicative of surface influence on Tb18. While there is a kernel of truth in this explanation, I find the overall reasoning to be unsatisfying. When I break down the respective terms of the radiative transfer equation in the deltaTb, I get terms that can easily counterbalance one way or the other. Please expand this section a bit, and perhaps show a figure that supports your argument.

The best 'proof' is from looking at AMSU data over the polar regions, where one can observe negative deltaTB18 for situations that are obviously cloud clear. We realize that this explanation is not very satisfying. Will try to illustrate this further with some RT simulations, and open a new discussion thread when these are ready.

Page 7514, lines 4-5. Please specify which version of RTTOV that you used, and make sure the accompanying reference applies to that version.

Done.

Page 7515, line 2. 'the similar scatter plot presented in BJ' Please give the exact figure

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number in BJ, so the reader doesn't have to guess when referring to that paper.

Done. The figure we are referring to is Fig. 4

Page 7521, lines 17-19. Units for IWP are variously given in g/m2 and mg/m2, while in Fig 6 label it is given in Kg/m2. Please use consistent units.

We would argue that mg, g, and kg are not different units. They are used in this instance in the text in order to have more natural numbers, and to make the text easier to read. This seems an editorial issue. Since we do not know ACP's policy here, we suggest to leave the decision to the copy editor.

Page 7534, figure 6. Inconsistent units for IWP in figure axis label and figure caption. See above.

Fixed caption to kg/m2 here.

Technical Corrections

Page 7515, line 11. Suggest re-write: 'To put the results of the cloud impact in perspective,'

Done.

Page 7516, line 18. Suggest change 'size distribution of the cloud particles and their shape and orientation' to 'size distribution of the cloud particles or their shape and orientation'. 'Shape' and 'orientation' cluster together, whereas 'size distribution' is a separate category.

Done.

Page 7520, footnote. Note spelling of emission (emssion)

Done.

Page 7522, line 9. Suggest replacing 'strong' with 'heavily laden'. Similarly on page 7523, line 2.

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Done.

Page 7522, line 16. Suggest replacing ‘strong’ with ‘intense’

Done.

Pages 7527-7526, Tables 2&3. The superscripts and subscripts on UTH run into each other. Please space the rows out in the vertical so that the reader can easily discern which is a superscript and which is a subscript.

Done.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 7509, 2007.