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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Fast cloud parameter retrievals of MIPAS/Envisat" *by* R. Spang et al.

Anonymous Referee #3

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'Fast cloud parameter retrievals of MIPAS/Envisat', by R. Spang et al., submitted to Atmos. Chem. Phys. Discuss.

This paper summarizes the technical and validation aspects of detecting the presence of ice cloud and the retrieval of various ice cloud properties from MIPAS spectra (cloud top temperature, height, ice water content, and effective radius). There have been previous published efforts to either detect or retrieve various cloud properties (and PSCs, too) from MIPAS, but this is a first comprehensive description of a prototype retrieval that will produce cloud products available for the scientific community. The retrieval of MIPAS cloud properties culminates a large effort by many involved on the MIPAS team, and certainly the cloud properties will make important contributions to cloud and climate science.

There are some important weaknesses in this manuscript that make it difficult to find

it acceptable for ACP in its present form. First, this paper has a tremendous amount of technical detail (albeit that is relevant and useful), and a substantial cross-validation of the cloud detection approaches and cloud frequency distributions with other sensors, but this manuscript is lacking new scientific insight. This paper appears to be more appropriate for Atmospheric Measurement Techniques, rather than Atmospheric Chemistry and Physics. Second, although the manuscript is not poorly written, it is unclear at times, and the flow between different sub-sections is at times poor. There are some occurrences of colloquial language and jargon terms that could be modified. Third, and similar to the second point, it is frequently unclear why a given subject is mentioned or a figure is shown (20 in total, and most have multiple panels). There is a lot of information to process, and perhaps much of it could be condensed into references or in a more concise manner. The Appendices are a difficult slog in addition to the main body of the paper.

Specific comments:

p. 33015, lines 16-19: This is pretty fluffy language. Arguably, researchers have been working with high spectral resolution data more for than a decade (aircraft and satellite observations.

p. 33016, lines 12-13: '. . .future ozone loss.' ?

p. 33018, line 7: '. . .either water or cirrus clouds.'

p. 33019, line 11: What is an 'FR-mode'? An example of technical jargon.

p. 33023, lines 5-12: Is this seasonal and latitude-varying climatology sufficient? If the trace gases (including water vapor) and temperature vary significantly, doesn't this impart important biases in the calculated radiances? Can't these variations be on the order of several Kelvins, and this can lead to missed/false cloud detections, and biases cloud retrievals? Perhaps this is not an issue because of some well-justified reasons, and the reviewer missed something along the way, but this would be a good example

of a lack of clarity on this issue.

p. 33023, line 13: The index here says “GI” (for gas index), but it says “CI” (for cloud index) on figure 3. Which one is it supposed to be?

p. 33028, line 5: ‘extinction’

p. 33028, Section 3.2.2. This is another point of confusion. Earlier in the cloud detection methodology, the cloud top temperature and height were obtained (see lines 12-15 on p. 33020). Why is an additional retrieval of these properties needed at this step? Were the original CTT and CTH initial guesses to the actual retrieval of these properties, or did I miss something? Some clarity in the detection and retrieval flow is definitely needed here.

p. 33028, line 19: The effective radius is not listed here. Is this retrieved after this retrieval step, separate from the CTH and CTT? This is quite confusing, although the flow chart of Fig. 1 suggests it is retrieved two steps later.

p. 33030, lines 17-18: How about ‘A more detailed description of the classification is found in Appendix B and C.’

p. 33031, lines 6-7: It is never mentioned why only the top three cloudy MIPAS spectra have effective radius retrievals, and the lower spectra do not.

p. 33031, lines 9-10: Are the authors suggesting that they can retrieve liquid water cloud properties from MIPAS? The reviewer is not aware of any limb emission or transmission study that has justified that this viewing geometry is able to determine anything about low altitude liquid water clouds. Please clarify this comment.

p. 33032, line 23: ‘a significantly more compact’

p. 33033, line 24: ‘However, the currently. . .’

p. 33033, line 24 to p. 33034, line 4: How do the authors know that this is a low bias? Have they made comparisons with other retrievals of ice cloud effective radius

from other instruments? Or is this simply a guess given some initial results? It is true that the three reasons listed are all possibilities for a low bias, but did the authors actually quantify whether any of this is true, or is this simply speculation on their part? What about the fact that MIPAS effective radius retrievals are only obtained from the uppermost three layers? If effective radius is stratified with height (and this is suggested by in situ observations of cirrus clouds), couldn’t this also be a factor, too?

p. 33034, line 6: What does ‘simulated cloudy radiances for well-known cloud conditions’ mean?

p. 33034, line 11: ‘and is based’

p. 33035, line 16: Liquid water clouds are mentioned again here. How are they detected in the initial stages? Are they assumed to be liquid based on the temperature of the cloud/MIPAS tangent point? How is a liquid/ice mixture handled in the radiative transfer simulations? Are there mixtures along the line of sight, in the vertical – how are they handled?

p. 33038, lines 24-25: What does ‘. . .and it also has a tendency to. . .’

p. 33039, line 1: Once again, liquid water clouds make it into the discussion. Are these assumed to be ice when they are retrieved? This is very confusing.

p. 33040, last paragraph of Sect. 4.3.2: The values of IWC discussed by the authors could be compared with the range of in situ aircraft values reported in the literature.

p. 33041, line 9: What is a ‘mean median’?

p. 33050, line 4: ‘point to a hypersensitivity’

p. 33050, 12-13: ‘consistent, constant and small’ is unclear

p. 33051, line 11: missing parentheses

p. 33052, line 21: ‘of the SVC flag’

p. 33053, lines 8-10: How does an observation of cloud around 20 km imply that water vapor of sufficient amount is being entrained into the stratosphere, and presumably clouds form there in situ? This argument doesn't make sense and the authors need to support this with some evidence. Couldn't this be a vertical smoothing effect because the weighting function in the vertical is nominally 3 km? Or could it be that overshooting convective towers are directly injecting ice particles into the lower stratosphere?

p. 33054, lines 26-27: Fig. 19 seems to show that ISCCP has slightly more high clouds in the tropics than ATSR given the color scale.

p. 33054, line 27: You need to cite which AIRS product is being shown. Is this the AIRS NASA team, or the Stubenrauch et al. product?

p. 33056, line 15: What in particular is innovative with this confidence flag? MODIS has been taking this approach for years.

Figure 1. Why are there separate boxes for 'detection' and 'cloudy'? The 'cloudy' box is for the assessment of confidence only?

Figure 2. Why does strong vertical striping in the field appear?

Figure 7. Get rid of blue background – make it white. Also, the vertical axis should be changed to 0-20 km to be consistent with Fig. 8. Also, what is going on with the large amount of IWC below 5 km in the tropics? Is this really liquid water cloud and it is being called ice by the algorithm? This comment also dovetails with the previous comments about the lack of clarity on how liquid water clouds are handled in this algorithm.

Figure 12. The lines should be distinguishable from each other. Why not use color?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 33013, 2011.