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Atmospheric Measurement Techniques Discussions

# *Interactive comment on* "Retrieval of macrophysical cloud parameters from MIPAS: algorithm description and preliminary validation" *by* J. Hurley et al.

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Generally the authors agree with your helpful comments, and thank you for your attention and interest. There are places in which more rigorous explanations are required, and these have been (hopefully) addressed (with new/heavily-revised text highlighted in red in the attached manuscript). The specifics of the changes made in response to your comments are detailed below. Those explanations which the authors have not explicitly made in the text (for sake of brevity and readability) have been noted as such, and answered following as well.



Just a explicit note on the extinction: The MWs in which the retrieval has been run have been chosen such that, in the presence of no cloud, the transmission if greater than 0.95 – ie. less than 5% of incident radiation is absorbed by the clear atmosphere and its gaseous components. In the presence of cloud, the additional absorption can be almost entirely attributed, thus, to the cloud presence. Whilst, rigorously, indeed the extinction coefficients reported are those pertaining to the optical depth of the whole limb path, including that of the clear atmosphere above the cloud top, they may be really be taken to be those of the limb-path through the cloud, as atmospheric contributions will be negligible (as discussed). The cloud extent, as you note, is not known, as stated in the definition of basic assumption of the cloud forward model – however, unless the cloud is a very thin vertical layer, it is unlikely that it will not extend from the cloud top to at least the bottom of the FOV in which it is first detected – and in any case, the limb path will become opaque to the radiation before the cloud bottom is reached. Since the model will integrate along the limb-path from the retrieved cloud top until the cloud becomes opaque, the authors assert that it is reasonable to call the extinction coefficients retrieved just that – although they agree that there ought to be more emphasis on the limb-path.

Abstract: the limitations and application of these values have been clarified

P3879L12: slide =moving part of the interferometer which changes the pathlength over which measurements interfere. This has been reworded.

P3879 'Overview of Mipas-Envisat' now includes description if the FOV

P3879L14: have mentioned the lower stratosphere as well

P3879L17: I've reworded this section . . . hopefully the changes are tighter, and extinction is described as a bit of a hybrid property as it derives from basic macro-and-microphysical properties, but we would argue that it is indeed a macrophysical parameter as it describes the bulk optical thickness of the cloud mass in the assumed model.

P3880L8: sentence modified

P3880L13: I've added a bit about this, although it was meant just as a quick example. Millimeter wavelengths are typically not used to look as ice cloud particles, as longer wavelengths show more effects from scattering from such particles (although there is scattering at these wavelengths too). VIS/IR wavelengths are good only above typical clouds because cloud presence causes large opacity along the path at these wavelengths, whilst higher frequencies are able to penetrate further into/below the cloud itself.

P3884L19: Water vapour included

P3885 Eq2: Slight changes have been made to highlight that whilst there will be some radiation coming from higher (and possibly warmer) atmospheric layers, we feel they don't need to be rigorously included in the model as 1) the MW have been chosen for minimal molecular input (Transmittance>0.95) and 2) because limb-viewers have tight vertical resolution/weighting functions, the proportion of radiation coming from those layers will be negligible.

P3888L20: Our 1 km value of a priori uncertainty doesn't really have a reference. We argue that 1 km is a reasonable value, because given the vertical extent of the MIPAS FOV (3-4km), and since the cloud detection method is trustworthy (please see Hurley et al. 2009, AMT on SVD detection), we see no reason why the a priori uncertainty should not reflect the extent of the FOV, since we are fairly confident that the cloud top will be somewhere within 1 km of the central tangent height of the FOV (which is taken as the a priori value of CTH). The discussion of this hasn't been greatly changed in the text, however mention of the cloud-detection method has been made, and perfunctory reason for the choice stated.

P3893L19: We haven't changed this formulation, although we do recognise your point. Our intention is that the error assigned to the combined value represent the most conservative estimate of uncertainty – and hence the choice of the maximum value of all



the individual components.

Your comments on Section 3: I have added a paragraph at the beginning of the section saying what is covered in each section and how the application/validation section is laid out.

paragraph 3.1: This was only ever meant to be a preliminary application of the code to, in effect, see the retrieval algorithm in action. Any comments on how the retrieval itself has done have been taken out and it has been made clearer that this section simply shows a sample set of results from the application of the algorithm to real MIPAS data, without trying to say anything about the qualtity of the product.

Section 3.2 has been extended and your concerns with the extinction retrieval have been addressed (hopefully!). I've taken out the statement you objected to in comment P3895L14. 1)More details given on ISCCP dataset and how the averaged products have been obtained (comment P3896L3...) 2)a clearer discussion of geometry differences has been formulated (comment P3896L3...) 3)I've made changes throughout the text to highlight that the extinction coefficient which we retrieve is that corresponding to the absoprtion along the full limb path (but I don't see the problem in calling it any extinction coefficient as long it's clear which extinction it quantifies.), both the atmospheric and cloud components. (This is discussed in more detail in my note at the beginning of this response, and in section 2.4.1). This is in response to comment P3895L21. (and partially to P3896L20) 4)P3896L9...: Only type 1 data has been used for the extinction analysis, as this is the only one for which k_c is actually retrieved and not assigned. I agree that clouds will be opaque from 0.1 km-1 onwards, and I've included a discussion of the limitations of the forward model in section 2.5.5 explaining the range of extinction for which the model is applicable – which is why all the plots of extinction range from 10-4 through to 10-1 km-1. Section 2.5.5 also discusses the effect of not truly knowing the physical extent of the cloud, in deriving extinction. (This is in response to 3896L20 and and to 3896L9...) We argue that the thicker extinction/water content values you quote may in fact come from studies which may have missed the very thin cloud that we purport that our detection method can identify, and that thus would be included in a climatology compiled using this algorithm. A discussion to this effect has been added, in greater detail, to this section. 5)Comment on Fig. 3 is addressed: yes, I agree that this must be an ISCCP-issue (I'd enquired about this issue with ISCCP, but got no explanations other than that) - but I've included a statement in the text highlighting that those values aren't believable. 6)Have emphasised that the quoted errors are those output from the retrieval process, and that there will in fact come significant errors from the incompatibility between forward model and reality. In response to 3896L19: the pointing error for MIPAS is of the order of several hundreds of metres (and the associated uncertainty introduced into the CTH by pointing is the same amount) – this has been included in the text. Furthermore, I have sought to quantify the magnitude of errors between retrieved and true values by applying algorithm to set of single-scattering cloud simulations to confirm that these errors capture the type of real errors we expect (please see Section 3.4). So it appears that the algorithm can be used to retrieve pretty consistent parameters from (more) realistic scattering clouds, with errors (ie. retrieved-simulated) approximately equal to those provided by the retrieval error covariance matrix. Hopefully this helps to confirm the validity of the extinction retrieval? I think perhaps your objection to the extinction retrieval is in Calling it extinction, whereby you say it is opacity – but I think that as long as we're very explicit about that the retrieved extinction coefficient is in fact for the whole limb path, and that it can be mostly contributed to the cloud, as we're in atmspheric windows of very low gaseous absorption, it is ok? That our retrieved values of extinction are lower than current climatologies – but I think this actually has more to do with the detection method used and geometry of limb-instruments than with the retrieval itself, and it may well be possible that we are simply seeing thinner clouds – but I have now included a discussion on other sources of errors. Is that ok?

P3897L7: Correct – I have changed this sentence to make explicit the 1.8 threshold.

P3898L2: Hopefully your doubts about the extinction retrieval have been convinced by



the added validation activities, and amendments? I've added bits and pieces alluding to sources of error and limitations of the model, so hopefully this tightens up the conclusion?

P3898L12: Yes, I agree that the error stemming from the basic assumptions (as opposed the retrieval process) should be discussed earlier than the conclusion – and have done so in Section 2.5.5. The relationship between CEX and CEF is defined in Section 2.3 – CEF can be thought of as the 'effective blocking power' of a cloud, and as such is dependent upon both the extinction and the extent of the cloud. So CEF, I guess, is somewhat comparable to the transmittance of the cloud, and separable from CEX in the sense that it takes into account the physical extent of the cloud, which we can estimate. In terms of spectral differences: it is worth noting that in this model, cloud radiance is a continuum radiance, and so increasing the CEF generally will have the effect of increasing the continuum radiance, although depending upon the combination of CEX and CTH, the amount which this is increased depends upon how far the radiation can penetrate and the effective radiating temperature of the cloud. (I haven't included this discussion in the paper, as I hope that the mathematical linkage between CEF and CEX is sufficient for most purposes). Concerning your comments about the water-vapour continuum and its distinguishability from cloud continuua: At altitudes sampled by the lower tangent heights in the vertical MIPAS scan pattern (e.g. those less than about 6$\sim$km), the water vapour continuum is difficult to distinguish from the continuum radiance introduced by emitting clouds. Due to this difficulty, the water vapour continuum becomes a potential issue for reliable cloud detection, and for retrieval of accurate cloud properties. It is possible that the water vapour spectral lines contained within some of the the selected MWs could be used to characterise the concentration of water vapour locally in the atmosphere (at tangent heights immediately above that identified as containing the cloud top), which could then be used toÂă disentangle the effects of the water vapour continuum from the cloud signal. In the current algorithm, the absorption from the water vapour continuum is taken into account to some extent in the utilised molecular transmittance spectra, whereby the expected water vapour continuum is effectively 'subtracted' from the measured continuum to establish the cloud contribution.This has not been studied extensively in this work, although it warrants further study, and as such may introduce errors in application of the algorithm as currently described, as regions of large water vapour concentration could be erroneously selected as cloudy measurements.

Technical: P3878L19: 'arguably' is gone. P3880L6: no, I mean 'depth', as in top-bottom extent.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/3/C1702/2010/amtd-3-C1702-2010-supplement.pdf

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Interactive comment on Atmos. Meas. Tech. Discuss., 3, 3877, 2010.