

Atmos. Meas. Tech. Discuss., 3, C1912–C1919, 2010

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AMTD

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Comment

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

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Received and published: 26 October 2010

The authors thank the referee for his helpful and carefully considered comments.

(Changes to text, either in content, or simply in rewording or reformatting, are given in red text in the attached new version of the paper).

In response to your general comments:

On Method: The algorithm described was chosen to be a non-scattering model because adding scattering into any calculation increases the computational cost of the

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problem dramatically, and would disable anything near operational processing, whereby data must be dealt with effectively and quickly in terms of processing. The forward model described here (and the inverse problem of the retrieval of the parameters discussed) had been compared to single-scattering cloud simulations from KOPRA (Hopfner and Emde, 2005) – but hadn't been included in the previous draft, in an attempt to keep the paper brief. However, details of this validation of the forward model are now shown in Section 3.3, with discussions of the limitations of the forward model (in terms of assumptions of homogeneity and scattering, especially in terms of the range of extinction coefficients for which the forward model can be expected to reasonably estimate a real scattering cloud) in Section 2.5.5.

On Validation: Much of the application and validation sections (Section 3.XXX) have been reworked and rewritten. These sections were never meant to act as conclusive validation of the retrieved parameters, but rather to show sample application, and to note that generally the retrieved parameters were in the right ballpark. That said, upon rereading the description of the ISCCP comparison, the discussion was not detailed enough to describe which specific product we used etc. As you correctly note, the full ISCCP dataset is not a good candidate for comparison with MIPAS – however, in this work, we have used the ISCCP high-cloud dataset, as you suggested (this was not made clear in the original text, but has been rectified now), which should be a better choice for comparison. There are some issues with the reported ISCCP data, which the authors did inquire with ISCCP about (namely strange cloud top heights and sometimes unphysically high cloud top temperatures, as you've mentioned), however whilst our query was logged, there was no explanation given. In some respects, it might be a useful activity to highlight these unreasonable values from ISCCP, as often they are rather blindly used as input to global climate and chemical models, and clearly they may not in fact be complete. Finally, the authors agree that CALIPSO would make a good comparative (at least statistically) dataset, however would rather do a more detailed intercomparison/validation in the future, as this paper is meant mostly as an algorithm description – but have mentioned this in the conclusions.

And the specific comments:

I have changed R_c to R throughout this. ie. 'R' means continuum radiance – so R_c , R_u , R_l etc are continuum radiances for different tangent heights etc. The only exception to this is R_{ν} for which it is not a continuum radiance, but this should now be duly noted.

Page 3878, abstract: The discussion of macro- and micro-physical parameters has been reworked, as I think that extinction is perhaps best described as a combination of different properties. I've reworded this discussion such that we call extinction a macrophysical property in this work because – due to our assumption of horizontal homogeneity – it is taken as a bulk property of the cloud.

Page 3879, first paragraph: I meant, rather, that its orbit/inclination allow for global coverage (as opposed to just looking at the same geographical location) over the span of about a month (orbital repeat period of 35 days). This has been modified.

Page 3880: You are most certainly correct! I have included an old list, and not the newest compilation of datasets, which I had meant to compile. This is now fixed.

Page 3880, line 3: True – I meant rather than satellite rather are better suited to looking at general/overall large-scale features because their FOVs cover such large areas that really what they can measure are average properties based on the bulk of the cloud measured.

Page 3880, line 13: Yes, of course this is true. I've reworded this so that the example is clearer (ie. that microwave (mm and sub-mm) instruments aren't sensitive to small ice particles, such as those found in thin cirrus).

Page 3880, line15: I mean that, given a particular satellite instrument, you should only choose to retrieve certain cloud parameters, as not all instruments can hope to retrieve the same properties due to inherent differences in sensitivities in spectral range, viewing geometries and so forth – as the body of that paragraph details examples of which

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types of instruments are good/bad for different types of cloud properties. I've modified this text to make this clearer.

Page 3880, line 26: Well, I guess very thin clouds limit to aerosols – and aerosols are certainly a hotbed for interest at the moment! My background is not in radiative forcing, but I thought that thin clouds, such as high thin cirrus, were quite important radiatively, in the sense that their contributions are still relatively poorly characterised?

Page 3880, line 28: Again, this is a remnant of my out-of-date lit review which I'd erroneously attached. I've modified this text).

Page 3881, line 3: mechanism → method

Page 3881, paragraph 2: Yes, it is true that often detection does not need to be done before cloud retrievals are done – but they frequently are, as retrievals are computationally expensive, and they provide a first-guess on some of the typical cloud parameters. I've modified this statement slightly.

Page 3881, line 11: The previous sentence to this tabulation indicates that we're only describing instruments akin to MIPAS and says that this means 1) limb-or-solar-occultation geometries and 2) IR – so the increase in radiance and extinction should be fine, as those are the only geometries and spectral ranges considered in this sentence. I've broken this up into sub-tabulations for ease of reading.

Page 3881, line 20: I believe it is due to signal attenuation throughout the cloud, so a 'virutal' decrease.

Page 3882, section 1.3: I didn't mean a conclusive overview, just a sampling, rather. I've changed the wording/layout slightly to reflect this.

Page 3882, line 25: 'under certain circumstances' → 'given appropriate a priori atmospheric information'

Page 3883, line 6: 'standard' retrieval theory would mean that you put in a measure-

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ment, and invert the measurement to get the parameter you're after – whereas the adaptation refers to the fact that we have to retrieve sequentially (as noted in the second part of the sentence) in order to get what we're after.

Page 3883, section 2.1: The number of Mws chosen was arbitrarily chosen to be ten, and the 930-960cm-1 range given represents the spectral range the selected Mws span – not the range from which they were chosen. I've clarified this phrase.

Page 3884, section 2.2: I've made the definition of continuum radiance more explicit – and I've added a plot to show how this is done for a particular MW. But basically: for each MW and altitude, a spectral mask is pre-computed, and then R_c and its associated error are calculated in each MW using the spectral mask. Page 3885, line 3: It has a more physical basis because it seeks to parametrise the physical (geometrical + optical) amount of cloud. Modified.

Page 3885, paragraph 2: MIPAS tangent altitudes now made explicit in Section 1.1 – and the detection algorithm is allowed to identify cloud anywhere below about 25 km. R_c is the continuum radiance calculated for the measurement using the (hopefully now clearer) definition and procedure. At these wavelengths, clouds do increase the continuum radiance, and when we've calculated CEF, we don't get $R_c > B_c$, because we've set the threshold to limit the range of applicability of the retrieval algorithm so that only clouds for which scattering is not dominant are sampled. Presumably cloud scatter will increase the continuum radiance, and hence would increase the CEF.

Page 3885, paragraph 5: Section 2 outlines the sequential retrieval process – saying that first step is to calculate the R_c for each tangent height spectrum in the given MIPAS scan. It then says to retrieve the CEF (which is what is being done now), and then this retrieved CEF is put in the main retrieval as an extra piece of information from which to retrieve the macrophysical parameters. So, forward model estimate R_{nu} is the radiance spectrum modelling that which is measured in each MW (τ_{nu} is obviously spectrally varying, as was defined in the Section 2.2, and errors therein should

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reflect the range in which atmospheric trace species such as H₂O, O₂, CO₂ etc vary – but since the Mws have been chosen to have little atmospheric absorptions, this should be negligible). So, the $CEF = R_c/B_c$ is the a priori for the CEF retrieval using this forward model, and the definition of R_{nu} is only used in this section of the paper, because it is the forward model for only this part of the sequential retrieval. (I've made some changes to the text to highlight this). “Upper” and “lower” here have been modified – but they correspond to the upper and lower fractions of the FOV ie. Lower = filled with cloud, upper= no cloud, just molecular. Other FOV distributions of cloud have been addressed in Section 2.5.5, after the description of the macrophysical forward model (ie. step 3/3 in the algorithm).

Page 3886, line 7: I've moved the description of optimal estimations retrieval formulation to here, instead of in the next section, and been quite explicit about the input parameters, constraints etc.

Page 3886, line 10: Yes: as MIPAS is a limb-viewing IR instrument, only information above the cloud top is any good.

Page 3887, line 7: Yes, modified.

Page 3887, line 7: Please see my note at the beginning of the specific comments section – but yes, all R_c , R_u , and R_l are continuum radiances, as calculated from the previous definition from each real measurement, but for three different tangent heights (as described in this paragraph including line 7).

Page 3887, line 17: No, α has been derived from the spectrally varying radiance R_{ν} . Hopefully the section describing the CEF is clearer now.

Page 3888, line 12: This comment is probably dealt with the in more explicit CEF retrieval section.

Page 3888, line 28: I believe so – it certainly allows us to retrieve cloud parameters in the expected range. (I've not modified the text to add this explanation.)

Page 3889, line 12: I think it holds pretty well – certainly well enough that the radiance difference it would make is lower than the expected NESR (of about 50 nW). Any errors thus incurred should thus be covered in the measurement error allowance. (I've not modified the text to add this explanation.)

Page 3891: No, the a_j are basically coefficients of the normalised FOV convolution function at each pencil-beam z_j altitude multiplied by the 'infinitesimal' integration step. So, below the cloud top, because the integration occurs at finite points (and is evaluated numerically) you assume that the radiance varies linearly between any two integration points – but at the cloud top (should it occur between two integration points), there is a step function in radiance. I've made this a bit clearer in the text.

I have grouped the comments on the validation/application sections together, as I've addressed and rewritten much of this, adding extra details and material. (The following points are addressed now in the text) Page 3896, line 9: I have included a plot of ISCCP and MIPAS frequencies. Page 3896, line 18: A longer discussion of errors (from sources such as 1) retrieval process, 2) scattering, 3) forward model assumptions, and 4) pointing has been added. As well, I've put in a comparison we did with KOPRA simulations (single-scattering) to show the discrepancy between the simulated values and those retrieved by the algorithm, which should give an estimate for the magnitude of errors if the algorithm was applied to real scattering clouds. Page 3898, line 2 ; Page 3898, line 5; these have hopefully be dealt with, given the more complete preliminary validation section now given.

Page 3897, line 7: The CI with its operational threshold of 1.8 is to remove clouds which will have a sufficient radiative effect on the subsequent spectra registered lower down in the scan pattern. Reference in. In principle, yes, the CI threshold should be a function of tangent height; however operationally, this is not done. This is not meant to be a comparison of cloud detection methods – rather we do have to mention that there will be some difference in the values reported depending upon the detection method used.

Page 3901, caption: the priority of selection is to do with the MW selection algorithm, which is referenced in the same sentence as the Table is introduced. I have made this more clear.

Page 3902: Only the continuum components of the Mws are used – so the CO2 lines are masked out spectrally. Perhaps this question is in response to misunderstanding the definition of continuum radiance, which is hopefully now clarified?

Page 3903: I have changed the symbols as suggested.

Page 3904: ISCCP optical depth is defined at approximately 0.6 microns (I've put this in).

Page 3905: The peak in CEX errors is at about 16% relative difference – I don't have an explanation for this. It must be something to do with the a priori value of extinction?

Technical corrections: Have done these, except in italicising 'a priori', which I think AMT wants unitalicised.

Please also note the supplement to this comment:

<http://www.atmos-meas-tech-discuss.net/3/C1912/2010/amtd-3-C1912-2010-supplement.pdf>

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