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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global retrieval of ATSR cloud parameters and evaluation (GRAPE): dataset assessment" *by* A. M. Sayer et al.

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We would like to thank Drs. Kokhanovksy and Walther for their helpful comments on the manuscript. We have addressed these as detailed below.

1 Review 1: Alexander Kokhanovsky

The paper is of high quality and can be published in ACP after minor corrections.

We thank Dr. Kokhanovsky for his kind words.

p.25628, please, do not use log(tau) on this page (and also on p.25632) but just tau itself. It could be useful to give also CTHs ranges (not just CTP ranges).

We quoted the log of COD, and the CTP, as these are the ways that the constraints are formulated within the retrieval. The limits on $\log_{10}\tau_c$ correspond approximately to 0.5$< \tau_c <$256. The conversion between pressure and height is more complicated, as it depends on the temperature profile and so varies in space and time, but the pressure limits correspond approximately to 0 km$< h_c <$6.5 km for water clouds and 3.5 km$< h_c <$16 km for ice clouds. We have added absolute COD, and CTH, to the revised paper.

You upper border for the effective radius of water clouds must be increased and the borders for the effective radius of crystals must be widen (see MODIS ATBD and results of retrievals).

These changes are being considered for the next version of the dataset.

p.25639, is it really that oblique observation conditions (large values of VZA) lead to an increased path length through clouds? It is expected that photons need a lot of time (and path) to return back to outer space for the nadir illumination and observation conditions (due to predominanetly forward scattering by droplets and crystals). Please, give more details on this issue and make references.

This was a speculative point and so we have reworded the text as such. We feel it is a reasonable argument although at the near-nadir viewing angles sampled by ATSR, it

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may be that the effect of the increase in path length is very small. We feel that as the trends in pressure are small it was not worth extensive analysis, particularly given the already extensive length of the manuscript.

Tables 4-6. I advice to include also average heights and other parameters and also coefïňĄcients of variances (stdv/average) in these table (or, please, create additional tables). Please, use not only CTP but also CTH in the tables.

We have added the mean and standard deviation CTH to Tables 4 and 5. We chose standard deviation rather than coefficient of variance as it is the more frequently-used statistical measure, and the coefficient of variance can be computed directly from these quantities.

We decided not to make changes to Table 6. Since the cloud datasets compared retrieve CTP rather than CTH, and because of the already extensive length of the manuscript, we did not feel it would be worthwhile to add CTH to this table. For the same reason, and also because of the large regional variability in cloud parameters meaning a global mean may not be a useful measure, we did not add a table of average and standard deviation or coefficient of variance for the same reason. Regional mean values can already be seen in Figures 13, 14, and 15.

Fig.2. Any idea why you have the yellow colour left of Africa and S. America in the last ïňĄgure in the row (large failing to converge)?

For South Africa, we believe this is related to biomass burning aerosol being misflagged as or mixed with cloud, as discussed in the text. For South America, we are not certain at present, but one possibility is a similar reason (mixture of transported

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aerosol from mining/smelting processes in the mountains).

Fig.3. I think, you need to give an equation for the normalized difference in the caption to Fig.3. What are units? I think that the much better representation is the stdv(in microns for CER) or coefïňĄcient of variance (stdv/average) and not the representation selected by you.

The calculation of the normalised difference is described in the caption text. This is a dimensionless quantity. We chose this representation as it shows how the difference between the sensors corresponds to the variability within each grid box and feel it serves its intended purpose. The caption text has been amended in the revised version for clarity.

Fig.8. I think, this ïňĄgure needs more explanations. For instance, for ice clouds, ratio of COT in the IR range to that in the visible is almost 1.0 being a little bit larger for smaller particles. It is really so for the red line in Fig.8. It is not the case for the black line (water clouds). I would suggest that you just plot the ratio of extinction coefïňĄcients at the wavelength 1.6micron to that at 0.67 micron in the spectral range 1-50 microns. So your black curve must be continued or extrapolated to 1.0 at 50microns.

The figure plots the ratio of the extinction at 0.55 microns to that of the average of the values for the 10.8 and 12 micron channels, using the optical properties computed for the retrieval. This has been clarified in the text of the revised manuscript. We did not show results for water clouds with effective radii larger than approximately 23 microns because these are not permitted in the current version of the retrieval. We do not think that plotting the ratio of 1.6 microns to 0.67 would be useful, as neither of these

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are relevant for the calculations being performed: we are translating the retrieved COD (which is referenced to 0.55 microns) to an equivalent COD in for the thermal IR channels (10.8 microns and 12 microns). Since the retrieval uses all channels simultaneously, and these are the optical properties used in the retrieval, this is the self-consistent way to convert between the two.

2 Review 2: Andi Walther

The topic meets the aim and scopes of the journal. The paper is well structured. It clearly describes all methods. Images and tables are in a good quality and support the text well. The article presents many interesting scientiïňĄc studies and methods. In my opinion the article could be published as it is.

We are glad that Dr. Walther finds our work interesting and favours publication.

The article is mainly based on a retrieval, which is not published yet. That's why some of the presented results were often hard to judge and to interpret. Some details I would have been liked to address, especially in chapter 2 and 4, will be supposedly explained in the Poulsen et al. 2010 paper or should be addressed there.

We are sorry that the algorithm paper (now Poulsen et al., 2011) is not yet available (although submission is imminent). The algorithm and validation papers were written in parallel; more recently, we decided to extend the algorithm paper to include some results of multi-layer simulation studies (referenced in some sections of this validation paper), which has led to a delay in submission. We decided that despite this it was most sensible to submit the validation paper, since the dataset is currently being used

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for scientific applications (including the GEWEX cloud climatology intercomparison) and it was therefore of interest to make validation results and usage recommendations available as soon as possible. Reviewer comments related to the cloud algorithm have been addressed in the forthcoming Poulsen et al., 2011 paper.

1. Page 25626 bottom line: You use Sx as the covariance matrix of the a-priori xa. This is not consistent to Rodgers where Sx is used for the solution error covariance. I would recommend to use Sa instead of Sx here and in equation (1).

The notation $\mathbf{S}_x$ was used in the initial submission as internally the ORAC team had used both $\mathbf{S}_x$ and $\mathbf{S}_a$; we agree that it makes most sense to be consistent with Rodgers and so have changed notation to use $\mathbf{S}_a$ for the a priori covariance matrix.

2. For equation (1) I would recommend to add a short remark that also the forward model uncertainties are stored in Sy , and not only the measurement errors. This is supposedly explained more in detail in the algorithm paper.

We have reworded the text to this effect.

3. Chapter 4 shows an interesting study. However, I am wondering how low the CER uncertainties for thin liquid clouds are in Fig5 and 6. Assuming that the information about CER comes mainly from the 1.6-micron channel and considering the typical Nakajima-King image, there is not much information about CER at thin clouds. Thus I would expect much higher values. The fact that CER uncertainty is bigger for thick clouds than for thin clouds is under these considerations hard to understand. However, this is a point, which cannot be discussed without details from the algorithm paper, where the measurement and forward model errors are speciïňĄed.

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It is correct that the majority of effective radius information comes from the 1.6 micron channel, and we agree that this result is surprising. Refinement of the retrieval error budget is being researched for the next version of the cloud retrieval algorithm: it may be that some of the assumptions relating to this channel are not appropriate. However, as noted in the paper, the direct validation of such uncertainty estimates is difficult.

4. Figure 2 shows the distribution of the cost for passed and failed convergence tests. Can the distinct land-sea distribution mainly explained by a higher likelihood of multilayer clouds over oceans? This could be tested with CALIOP and CPR. However, I have some slight doubts about the use of the cost as a general quality parameter of the results, especially the use of ïňĄxed thresholds. I agree that the cost is a handy one-number estimate of the retrieval quality. But, it is also a function of Sy and Sa those may be very different for each pixel. I would assume that the global pattern in Fig.2 have no physical reasons, but shows the pattern of different set-up of the retrieval parameters. The unrealistic high low-cost partition over Antarctica may be an evidence for it.

We agree with these points and mention them in section 2 (e.g. forward model error over the ocean and Antactica). A sentence has been added to the conclusion to mention the scope for refinement of the error budget, and so cost statistic. Figure 12 does suggest an increased incidence of multi-layer clouds over ocean as compared to land in many regions (from CALIOP).

5. It would be probably interesting to see if global maps for the solution uncertainty values (the diagonal elements of Sx) for all converged pixels show similar pattern. These uncertainties are much better to interpret than the cost because they have a

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physical unit and meaning for each state vector element. A high uncertainty in CTP is probably more a sign of multi-layer clouds than a high cost. The correlation between the errors of the state vector elements could be interesting as well. I would be, as an example, very interested in the question if the error in COD is correlated to the error in CTP.

In Section 2.3.3 we recommend considering only retrievals where the uncertainty estimate on cloud-top pressure is smaller than 50 hPa. This is discussed in the context of broken cloud fields, although it was also found to identify some cases of multi-layer systems. In the revised paper, a sentence is added to mention this. The algorithm paper (Poulsen et al, 2011) includes simulations of the retrieval uncertainty for multi-layer cases. Unfortunately, uncertainty correlations between state variables are not output by the current version of GRAPE due to storage constraints, so these cannot at present be examined over the whole dataset. However, simulations do show correlations between the uncertainty in state variables for cases of thin clouds, low fractional cloud cover, or multi-layer cloud systems. A comment to this effect has been added to Section 4.

6. Table 7: You recommended not to use multi-layer cloud water path. How can a user know whether a particular pixel is a multi-layer cloud or not?

This is a difficult problem. Through application of the recommendations made in the paper, some of these cases can be identified (e.g. high cost or uncertainty on cloud-top pressure). However in some cases additional information about the scene may be needed. If users are performing an individual case study, it is likely that they may have such information (in the form of simultaneous radar/lidar measurements, or additional satellite cloud products such as microwave liquid water path). A comment has been added in Table 7, and the conclusion, to reflect this.

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