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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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General comments:

The research paper "Global retrieval of ATSR cloud parameter and evaluation (GRAPE) data set assessment" describes the evaluation and assessment of a retrieval scheme for cloud properties. 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 scientific studies and methods. In my opinion the article could be published as it is. I have only some short comments and questions, which could probably strengthen the paper if addressed. I would also like to point to one weak point, which cannot be fixed in the review process of this paper. The article is mainly based on a retrieval, which is not

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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.

Special comments:

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).

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.

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 specified.

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 multi-layer 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 fixed 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

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10, C11663–C11665, 2010

parameters. The unrealistic high low-cost partition over Antarctica may be an evidence for it.

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 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.

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?

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