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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison between the first Odin-SMR, Aura MLS and CloudSat retrievals of cloud ice mass in the upper tropical troposphere" *by* P. Eriksson et al.

P. Eriksson et al.

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First of all, we want to thank the anonymous referees and A. Kokhanovsky for considering the manuscript in such a detail, and giving very valuable suggestions for improvements of the manuscript. Some of the raised points are discussed further below.

The results in the manuscript are formally correct, but we have anyhow decided to make a complete revision of the manuscript using newer data versions. In the preparation of another manuscript (at the Chalmers University) it was found that the used CloudSat retrieval version, R03, gives a biased view of the mean ice mass in the studied part of the atmosphere. The official CloudSat retrievals are performed on a profile basis, and it was found that failures in convergence were closely related to the atmospheric state.

It was believed that data gaps were distributed in a relatively random manner, but it became clear that a dominating part of the retrieval failures happened inside areas of strong convection. This had the consequence that the situations associated with the highest ice masses were lacking in the used data. It can be said that the data set had a bias towards clear sky and thin cloud conditions. Mapping of individual CloudSat data points to ice water content (by assuming e.g. the MH97 PSD) indicates that the overall mean value around 12 km could be a factor of 3-4 higher than obtained by R03. This has further consequences for the given SMR estimates, as the CloudSat data were used to estimate the SMR cloud inhomogeneity correction factor. This factor is now probably underestimated. That is, both CloudSat and SMR estimates should be considerably higher.

Since the submission of the manuscript quick progress was made around the next CloudSat retrieval version, R04. This product is now official with good coverage of the complete mission and it has been verified by us that the problem discussed above is not longer present. On the same time, considerable data from the second official MLS product, v2.2, are now at hand.

We could try to incorporate the CloudSat R03 problem discussed above in the manuscript, when discussing the errors of each data set. However, as the R03 dataset is of very little interest at this moment considering its problems, we feel that this is not worth the effort. We have instead suggested to the editor to make a complete revision of the manuscript, and then using CloudSat R04 and MLS 2.2 data. This suggestion was approved, and we will now start this revision.

All comments made by the reviewers will be carefully considered when revising the manuscript. It is not totally clear which of the raised points that will still be relevant after the revision. Anyhow, there are some points of more general character that can be commented or clarified:

* A detailed error estimation of the SMR retrieval approach is presented in Eriksson

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et al.(2007a). This including the impact a varying particle shape and assumed PSD, issued raised by the anonymous referees. This will be made clearer in the revised manuscript and the most important numbers will be repeated.

* The assumed particle size distribution (PSD) is an important potential error source in the retrievals, as pointed out in the manuscript and also discussed by the anonymous referees. CloudSat retrievals assuming the MH97 PSD (that is assumed for SMR and MLS) will be included in the new manuscript, for a comparison focusing on other retrieval issues. Usage of an alternative PSD in the SMR retrievals was considered already in Eriksson et al.(2007a), as mentioned above. We wanted, when preparing the existing manuscript, to make a more detailed analysis of this issue but could not find relevant data to use as input for the simulations. We have now been in contact with Andrew Heymsfield and discussed uncertainties around PSD estimates. He has even provided us with airborne in-situ data that could be used for formulating an alternative PSD. We will now reconsider to investigate more in detail the importance of the assumed PSD for the SMR retrievals.

* It is correct that a threshold value of 4 g/m2 is applied for SMR (discussed by referee 1). This is of course a problem for areas with consistently low ice water paths (IWP). However, for areas with higher mean IWP, most of the ice mass is found in cases well above the detection limit and a possible bias due to the thresholding should be small compared to other error sources. However, it is worth noting that this issue gives consistently an underestimation of mean IWP (with a relative importance increasing with decreasing mean IWP). All the considered instruments have a detection limit of similar size, and this issue is not critical for the comparison, but will be discussed more clearly in the new manuscript.

* The mean value is not a good overall measure for a quantity having a highly non-Gaussian PDF, as pointed out by referee 2. The median value could in such circumstances be a good alternative, but is here not a sensible measure. In fact, the median value is here zero, reflecting that most cases are either clear sky or have a pIWP be-

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low the detection limit. It would neither make sense to take the median of the "cloudy cases" as the result would then be very sensitive to selection of detection limit (in contrast to the overall mean value). We gladly welcome suggestions on this point, but believe that mean values are acceptable as long as complete PDFs also are given. For example, if the data should be used to validate some climate model, step 1 would be to compare mean values, and step 2 would be to compare PDFs between the model and the measurements.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 12035, 2007.