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Atmospheric Measurement Techniques Discussions

Interactive comment on "An intercomparison of radar-based liquid cloud microphysics retrievals and implication for model evaluation studies" *by* D. Huang et al.

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Comment: The authors compare 3 standard liquid water cloud retrieval algorithms for liquid water content (LWC) and effective radius (reff) over a time period of 9 years using data from the ARM SGP site. The long-term comparisons are intended to give an uncertainty estimate of current liquid water cloud microphysical products essential for the evaluation and further development of numerical weather prediction models. My main criticism lies in the following points:

Response: We thank the reviewer for the insightful comments that help to improve this

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manuscript. Here are our detailed responses to the review comments.

Comments: 1. Introduction: The authors should also mention the following approaches to quantify the accuracy of liquid water cloud retrieval algorithms a.) SW and LW radiative closure studies (e.g. McFarlane and Evans 2004 JAS, Ebell et al. 2010 QJRMS) b.) Model bases approaches, i.e. model output is used to generate remote sensing measurements and then the retrieval is applied and one may quantify the error directly by comparison with original model output (e.g. Löhnert et al., 2007 JGR)

Response: A brief discussion of several other approaches (e.g., radiative closure, and observation system simulation) to quantify cloud retrieval accuracy will be added in the revised manuscript. Relevant references will also be included.

Comment: 2. Introduction: What is the justiïňĄcation for the assertion that the spread of the three existing retrievals is useful for model evaluation? Can the three retrievals be regarded as independent estimates? Or are they based on similar assumptions? Please make this essential point more clear.

Response: To quantify the uncertainty in cloud retrievals is highly challenging due to our limited knowledge on cloud processes, and the scarcity as well as the huge scale mismatch of co-located in-situ and remote sensing measurements. This work is a first step toward understanding the uncertainty of cloud microphysical retrievals. The spread of the three retrievals could provide a rough uncertainty of cloud properties, which can be used for model evaluation. The DOE Atmospheric Radiation Measurement (ARM) program has established a focus group called "Quantification of Uncertainty in Cloud Retrievals (QUICR)" to address the cloud retrieval uncertainty problem.

The three retrievals cannot be simply regarded as independent estimates or simply thought to be based on similar assumptions. The MICROBASE and UU retrievals are based on similar inputs while the UND retrieval uses additional observations (shortwave transmittance). Some of their underlying assumptions are also similar. Therefore, they cannot be regarded as completely independent. Intercomparison studies will

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provide some hints on the magnitude of the spread in different retrieval. Stratified intercomparisons will also help to pinpoint the assumptions responsible for the spread and help to identify conditions when the retrievals are most reliable. These points will be clarified in the revised manuscript.

Comment: 3. Section3: The authors should generally state the expected/documented accuracies of the retrievals algorithms they present. These have been most certainly estimated in the past. Also it would be of high value if the assumptions made in the different algorithms were commented concerning their physical justiïňĄcation/background in a paragraph at the end of each sub-section 3.1, 3.2, 3.3. E.g. the assumption made by Eq. 8 seems non-physical.

Response: We will include a discussion about the retrieval uncertainty reported in literature in the revision. The accuracies of the retrieval algorithms have not been documented systematically yet, although radiative closure studies were performed to indirectly examine cloud retrievals (McFarlane and Evans 2004, JAS, etc.). This is one of the motivations of this study.

We agree with the reviewer that the validity of each underlying assumption is critical. The conditions where the assumptions used in each algorithm are valid will be discussed in more details in the revised manuscript.

Comments: 4. Section4: The authors should describe their data sets in more detail. a.) How many cases were evaluated for each retrieval algorithm? b.) Were exactly the same cases compared with each other – only this would make sense for and direct comparison. c.) How many cases were clear/cloudy/precipitating? d.) Could all retrievals algorithms be applied in all cases? Or does the one or the other have difïňĄculties is certain situations? d.) How many case were excluded due to precipitation? I would recommend to include this information in a table.

Response: Thanks for these suggestions. These information will be included in a table in the revised manuscript.

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Comment: 5. Section5: Could generally be shortened signiïňĄcantly. E.g. I do not see any reason for Fig. 1 and Fig. 2. There is no information here from which the reader could beneïňĄt. Instead, the authors could analyse the year-to-year variability for the different algorithms or discuss how the different algorithms represent the seasonal cycle. The fact that the mean LWC proïňĄles look similar and the discrepancies for reff are rather large could be described much shorter by means of one ïňĄgure and details could be summarized in a table. This also holds true for the low-level-stratus comparisons, which could also be summarized in this table. In this sense Fig. 4 is not really necessary –the essential information is shown in Fig. 3; also showing relative difference in LWC is a bit problematic because this can grow very large in case of very small absolute values. Fig. 7 could also be omitted – the statement about auto-correlation could be restricted to one or two sentences.

Response: We appreciate these valuable comments. Figures 2,3,4, 6, 7, and 8 will be removed . We will use Contour Frequency by Altitude Diagrams (CFADs) instead of PDFs to better show the vertical distribution of cloud retrievals.

Comment: 6. Section5: Before describing the mean properties of the microphysical properties (Fig. 3 and following), the authors should compare the cloud macro-physical properties, i.e. cloud occurrence at each level (yes or no), cloud base, cloud top and cloud depth. This means that Fig. 10 should be positioned at the very beginning of the paper and be extended towards cloud occurrence. Mandatory in this sense is a pdf of cloud occurrence at the different height levels for each of the different retrievals algorithms.

In contrast, the pdfs shown in Fig. 5 and Fig. 6 do not contribute to understanding the retrieval algorithm differences because the height dependent information is missing.

Response: We will add two figures describing the vertical distribution of cloud macrophysical properties. We agree that single-variable PDFs are unable to reveal vertical structure of the retrievals. Fig 5&6 will be replaced by CFADs in the revision.

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Comment: 7. Only after these properties have been analysed and discussed in detail, is it reasonable to begin discussing differences in LWC and reff. If modellers shall use this paper to ïňĄnd out how good remote sensing observations are for model evaluation, they should ïňĄrst be informed about the accuracies of the macro-physical-properties. Once this has been carried out, I also think the authors should evaluate LWC and reff as a function of normalized height above cloud based (i.e. differentiating by cloud depth classes) instead of plotting mean values throughout the troposphere. Plots like Fig. 3 give no information whether differences are occurring more at cloud base, cloud top or in the middle of the cloud (see e.g. Löhnert et al. 2003 JGR). This is essential for characterizing the algorithm deïňĄciencies.

Response: We agree with the reviewer. We will use normalized height above cloud base instead of altitude in the revised manuscript. Figure 3 will be replaced by CFADs of cloud properties.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 7109, 2011.