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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 paper presents an inter-comparison of three radar-based liquid cloud retrieval algorithms and performs several sensitivity studies to understand the factors that impact the retrieval differences. The study is a useful contribution to the ïňĄeld, and the sensitivity experiments, in particular, provide a more systematic way of assessing retrieval differences than I have seen in previous intercomparisons. However, the paper is too long and repetitive, and needs to be rewritten for conciseness, removing extraneous and duplicative text and ïňĄgures. Additionally, while the liquid water sensi-

tivity tests were extremely useful, I found the effective radius tests less so because the confounding factors of phase partitioning and precipitation contamination were not separated from the general examination of differences in the effective radius formulation. I feel the paper needs major revision, and the comments below need to be addressed before it can be considered for publication.

Response: We thank the reviewer for the constructive comments that help improve this manuscript. We will follow these comments to make a major revision. Particularly, we will make the paper more concise. Here are our detailed responses to the review comments.

Comment: Major comments: 1) Sections 2 – 4 were too long, confusingly organized, and repetitive. The theory of radar retrievals of LWC and the use of LWP constraints is repeated in both sections 2 and 3; discussion of input dataset details are in both Sections 3 and 4; discussion of millimeter-wave radar in Sections 2 and 4, etc. Then further discussions of the details of the algorithms are again given in Section 6. These sections need to be rewritten. One approach would be to combine (and shorten) Sections 2-4 into a single section, which discuss the utility and theory of radar measurements, the key inputs needed (radar reïňĆectivity, cloud boundaries, LWP constraint), the basic types of assumptions used in the current retrievals, and the limitations therein. Then a table listing the key inputs/assumptions, dates available, for each algorithm could be included. Then this discussion and table could be referred to (rather than rewritten) in Section 6.

Response: We appreciate this valuable comment. We will re-organize sections 2-4 of this manuscript. Sections 2, 3, and 4 will be merged and a table will be included to illustrate key inputs/assumptions for each retrieval algorithm.

Comment: 2) The section on modifying the microbase inputs one by one to use the UU inputs to look at the effect on the LWC proïňĄles is quite useful. However, the comparison of the particle size is much less satisfying because the conclusion from the previous section (that the phase partitioning is the main difference) signiïňĄcantly impacts the particle size comparisons as does the likely impact of precipitation. I think it would be extremely useful to do a comparison of the particle for cases in which both algorithms agree it is a liquid cloud with no precipitation to truly examine the effect of the effective radius formulations on the retrievals. Once that was established for the most basic case, the complicating factors of phase and precipitation could be added into the comparison.

Response: We agree with the reviewer that it is important to separate other complicating factors such as phase and precipitation when examining the effective radius retrievals. We will perform comparisons and experiments for only non-precipitating liquid clouds to better characterize the difference between the retrieval algorithms. We will make it clear that some underlying assumptions of the retrieval algorithms are in general not valid for precipitating clouds. Therefore, we will suggest that users should use the retrievals for precipitating clouds with caution.

Comment: 3) The phase partitioning algorithms were seen to be the major source of difference between the microbase and UU proïňĄles, and likely lead to differences in the particle size proïňĄles as well. However, there is no discussion of how to assess which of th methods used is more accurate, which would be useful. Are there any ways that phase partitioning algorithms for single wavelength radars could be assessed and/or improved using either in situ data or other remote sensing measurements? Or do we just have to give up on single wavelength radar retrievals? If possible, it would have been useful to have comparisons of the dual-wavelength LWC retrievals performed by the ïňĄrst author (Huang et al. 2009) to these single wavelength retrievals to assess their accuracy.

Response: The phase partitioning algorithms could be improved if more information is used. Shupe (2007) showed that hydrometers can be more accurately classified using temperature, and three radar Doppler moments. However, there is still no acceptable method to specify the radar reflectivity due to cloud liquid if only temperature

and Doppler moments are used. When the full Doppler spectrum is available, it is possible to identify a "cloud mode" from the spectrum in some cases (Luke et al., 2010; Kollias et al., 2011).

We will include some discussions on how to improve the phase-partitioning problem in the revised manuscript.

Comment: SpeciïňĄc comments: 1) p. 7114, the authors state that “the choice of the functional form has only minimum impact on radar retrieval algorithms”. A reference or example for this statement would be useful.

Response: Relevant references will be added in the revision.

Comment: 2) P. 7120, why are supercooled droplets expected to be larger?

Response: In mixed phase clouds, supercooled liquid droplets co-exist with large ice/snow particles. Therefore, the observed radar reflectivity will be dominated by ice particles and the UU algorithm will provide a large droplet effective radius retrieval. It is clear that cloud microphysical retrievals under such conditions are unreliable. 15 μm is just an arbitrary upper limit for supercooled liquid droplets set by the UU algorithm.

Comment: 3) I don’t really see the utility of the yearly-mean comparisons (Figure 2).

Response: Thanks for pointing this out. Figure 2 as well as relevant text will be removed. We will only keep the monthly-mean comparisons and add comparisons of Contour Frequency Altitude Diagram (CFAD).

Comment: 4) To reduce the number of plots/panels, it seems that some ïňĄgures could be combined. For example, in several of the plots (Figs 3, 5-6), the non-precip proïňĄles could easily be plotted on the same panel with a different color or line type.

Response: These figures will be replaced by Contour Frequency Altitude Diagrams (CFADs).

Comment: 5) P. 7125, line 25 incorrectly states that MICROBASE has more liquid water at heights < 4.0 km than UU

Response: MICROBASE has more liquid water at heights > 4.0 km than UU. This typo will be corrected in the revision.

Comment: 6) P. 7126, lines 3-7, authors state that effective radius retrievals larger than 10 um in UU algorithm are considered contaminated by drizzle, therefore it can be inferred that a large portion of the UU are contaminated by precipitation. However, on p. 7120 it is stated that droplet effective radius greater than 10 um in the UU algorithm are set to 10 um (except for supercooled droplets). Please clarify. [Note this also impacts Fig 9d, how can the UU algorithm have any values > 10 um for non-precipitating clouds?].

Response: We use a very simple method to flag precipitation profiles: if there is at least one point in the profile satisfying Z>-20 dBZ and T>273.1 K, the profile is considered to be precipitating. We examined the original UU retrievals and it seems that effective radii diagnosed to be greater than 10 μm are NOT set to 10.0 μm. This is not consistent with the original statement in Mace et al. (2006). We will remove the relevant sentences in the revision.

Comment: 7) In my opinion the autocorrelation ïňĄgure (Fig 7) did not add much to the paper. It should be removed or more information on potential uses of the autocorrelation (for model evaluation, parameterization, or other reasons) are needed to motivate this section.

Response: Figure 7 will be removed, as also suggested by other two reviewers.

Comment: 8) p.7134, I am not entirely clear how the scatter plot of radar reïňĆectivity in Fig 10 a is made since the UU and microbase algorithms do not necessarily have the same boundaries – are these comparisons of the reïňĆectivity for layers that the two retrievals have in common? Or do they include points that one retrieval ïňĆags as cloudy and the other doesn't? Please clarify in the text.

Response: Further clarification will be added in the revision. The comparison is for layers that both retrieval products provide valid cloud retrievals. So it does not include points that one product ïňĆags as cloudy and the other does not.

Comment: 9) P. 7134, line 5-6. Authors state "there are also a considerable number of cases where the UU radar reïňĆectivity is larger than that of microbase". Does this refer to the points under the 1-1 line in Fig 10a? In that case, isn't microbase reïňĆectivity larger?

Response: Further clarification will be added in the revision. This refers to the points above the 1-1 line in Figure 10a.

Comment: 10) P. 7134, Figure 10c. There are a large number of cases where microbase base height > 0 km while UU base height = 0 km. I would have expected the opposite given that UU explicitly uses the ceilometer to determine cloud boundaries.

Response: The MICROBASE algorithm does not explicitly use cloud base height information from lidar. The ARSCL best-estimate radar reflectivity is used as hydrometer mask rather than cloud mask. The reason why there are some cases where microbase base is well above the surface while UU base is close to the surface could be the averaging of 10-s microbase data to 5-m resolution. We will further examine this in the revision.

Comment: 11) P.7134, A table describing the details of the sensitivity experiments (and which inputs were used in which run) would be useful.

Response: We will summarize the details of each sensitivity experiment in a table.

Comment: 12) P. 7134, It would be useful to clarify explicitly that Experiment 3 uses the UU LWP, but the microbase distribution of LWC with height.

Response: Clarification of experiment 3 will be made in the revision.

Comment: 13) P. 7135, lines 13-20. The ïňĄrst two sentences in this paragraph are very repetitive, please make more concise.

Response: These sentences will be removed.

Comment: 14) P. 7137, it is not clear why none of the microbase retrievals (even using N = 50 cmËĘ-3) can come close to matching the UU particle sizes, given that it is stated on p.7136 that the UU assumption is similar to using N = 200 cmËĘ-3. I am guessing that the mixed phase partitioning and/or precipitation is coming into play here. This needs to be discussed (although perhaps this will fall out of my general comments above).

Response: As suggested by the reviewer, these sensitivity experiments will be redone for only non-precipitating liquid clouds to avoid complication by phase partition and precipitation. We agree with the reviewer that phase/precipitation is responsible for the fact that MICROBASE and UU effective radius retrievals do not agree even when a wide range of N is used in the MICROBASE algorithm. Our new analysis shows that when only non-precipitating liquid cases are used, setting N=160 in MICROBASE algorithm will result in similar effective radius retrievals as the UU algorithm.

Comment: 15) P. 7138, line 26. Please explain how cloud radius is determined in GCMs if it is not predicted.

Response: Liquid cloud drop effective radius is often parameterized using cloud LWC in climate models.

Comment: 16) P. 7138-7139. Your discussion here is somewhat contradictory. First you say that given the large spread in LWC in GCMs these datasets are still useful for evaluating climate models. Then you say that more accuracy is needed to constrain climate sensitivity. It would be useful to somehow bring these two thoughts together.

Response: The spread in cloud properties simulated by different climate models is larger than that in cloud retrievals. In this sense, cloud microphysical retrievals are useful to evaluate simulated clouds. When it comes to climate sensitivity, cloud radiative

properties are depended on cloud microphysical characteristics and the requirement of retrieval accuracy is much higher. We will add more discussions on this in the revision.

Comment: Technical comments: 1) P. 7116, line 23, remove “uses a” from sentence. 2) P. 7124, “Figure 1e-h is similar” should be “Figures 1e-h are similar” 3) P. 7134, line 3, “scattering plots” should be “scatter plots” 4) P. 7138, line 8, change “selected ten” to “ten selected” 5) P. 7137, line 12, change “captures” to “capture” 6) Fig 2. Figure captions are incorrect in panels (d) and (f)

Response: These technical comments will be addressed in the revision.

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