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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.

Anonymous Referee #2

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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 field, 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 figures. Additionally, while the liquid water sensitivity 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.

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 reflectivity, 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.

2) The section on modifying the microbase inputs one by one to use the UU inputs to look at the effect on the LWC profiles 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) significantly 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.

3) The phase partitioning algorithms were seen to be the major source of difference between the microbase and UU profiles, and likely lead to differences in the particle size profiles as well. However, there is no discussion of how to assess which of the

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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 first author (Huang et al. 2009) to these single wavelength retrievals to assess their accuracy.

Specific 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.

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

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

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

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

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?].

7) In my opinion the autocorrelation figure (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 sec-

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tion.

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

9) P. 7134, line 5-6. Authors state "there are also a considerable number of cases where the UU radar reflectivity 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 reflectivity larger?

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.

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

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.

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

14) P. 7137, it is not clear why none of the microbase retrievals (even using N = 50 cmˆ-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 cmˆ-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).

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

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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.

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)

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