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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cloud type comparisons of AIRS, CloudSat, and CALIPSO cloud height and amount" *by* B. H. Kahn et al.

Anonymous Referee #1

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This manuscript presents comparisons of cloud height and cloud amount with respect to cloud classification derived using both active and passive satellite based sensors. These kinds of intercomparisons are critical for understanding the strengths of the new generation satellite sensors that are now in orbit, which will aid scientists in determining global distributions of cloud properties.

Overall, this manuscript is organized and well written. The authors systematically analyze the cloud heights/amounts derived from each remote sensors. The primary focus is to quantify the skill at which AIRS can measure cloud height and amount relative to the active sensors. One original aspect to this comparison is the categorization of cloud properties with cloud type, which provides further insight into the behaviour of the instruments. I recommend that this manuscript be accepted with minor revisions.

Primary Weakness:

The primary weakness in this manuscript is in the use of CloudSat and CALIPSO data products that have not yet been thoroughly evaluated. While this reviewer understands that there is excitement over availability of these products, the authors should use caution in using these resources too soon. This reviewer also notes that the primary author does include many of the developers of the CloudSat and CALIPSO products as coauthors, who presumably are aware of the issues associated with these products. At the very least, the authors should spend some time addressing this issue.

Specific Comments:

1) Section 2. There is insufficient discussion concerning the field-of-view overlap of the three instruments. You have listed the specifications for each instruments in Sec. 2.4, and discussed FOV in Sec. 3.1, but need to provide a value that indicates the actual size of the footprint shared by the three instruments. Since this shared footprint should vary with altitude, you might provide a value for typical low/middle/high cloud heights. This will help to put the comparison into perspective. This detail would be appropriate for Section 2.4 or Section 3.1.

2) P. 13922, paragraph near line 25. Please provide more detail concerning the validity of the cloud classification scheme. How has this scheme been evaluated and verified? What skill does it have in comparison to a radar-lidar scheme? If there are any details concerning these questions in the referenced Wang and Sassen 2007 work, please provide some brief statistics.

3) Section 2.4. It would be useful for you to provide an explanation of the factor f_A in this discussion of Fig. 2. You define the term f_A in Section 2.1, but it would be useful to put it in context with the measurements in Fig. 2b.

4) Section 3.1. It seems that you really gloss over the fact that CloudSat cloud frequency is remarkably lower than either CALIPSO or AIRS, as shown in your Table 2.

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Interactive Comment

This point should be emphasized in Sec. 3.1. Also, please move the statistics concerning the typical number of lidar/radar profiles in a typical AIRS footprint to the text in Sec. 3.1 (rather than in Fig. 3 caption). I kept looking for it in the text.

5) Fig. 3 caption. Are the percentages for each (A)-(F) scenario the relative frequency that AIRS agreed with CloudSat (CALIPSO) for that particular scenario? You should consider making this a little clearer in the caption. It seems like you are in a good position to really understand the issue of inhomogeneous cloud fields within the AIRS FOV (within the limitations of the lidar/radar resolutions). You touch on this issue but the values are stuck in Table 2 and Fig. 3. You should summarize better in the text the main points about inhomgenous cloud fields and how often AIRS pixels are considered homogeneous, when the active sensors show that they are not.

6) Sec. 3.1, p. 13925, line 25. You say that Scenario C is explained by the insensitivity of CloudSat to thin Ci. However, the CloudSat frequency is 9.5% and the CALIPSO frequency is less than 1% smaller. It seems like the difference between the two numbers should be larger if this really is the explanation. Your conclusion does seem substantiated.

7) Conclusions, p. 13936, lines 10-20. Given the inherent limitations of CloudSat, your conclusions in relation to cloud type should be made in reference to CloudSat's sensitivity, and also in the validity of the cloud classification scheme, which is not currently addressed in the manuscript. You should also clearly state in the conclusions the cloud types that AIRS retrievals excel, and those where AIRS does poorly (you do identify stratocumulus as challenging) and state the statistics to back up your claims. It should not be seen as a flaw, but rather a characteristic of the measurement.

8) Conclusions, p. 13937, lines 1-10. Again, I think that you are tiptoeing around the issue of the CloudSat sensitivity. Instead of stating that the biases in cloud top height are increased for high clouds when comparing with CALIPSO, you need to state clearly that both CloudSat and AIRS have difficulty with the thin cirrus (or boundary

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layer clouds), and underestimate thin cirrus cloud tops by X%, and that AIRS misses thin cirrus X% of the time. Do the same for layered clouds, state AIRS/CALIPSO do not detect lower cloud layers X% of the time when CloudSat does detect lower layers (here I mean lower cloud layers below higher clouds; i.e. middle or boundary layer clouds below thick anvils). The final conclusions will be much clearer if stated this way.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 13915, 2007.