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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "A 6-year global cloud climatology from the Atmospheric InfraRed Sounder AIRS and a statistical analysis in synergy with CALIPSO and CloudSat" *by* C. J. Stubenrauch et al.

Anonymous Referee #2

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This paper is a good solid addition to the literature on Earth's clouds, mostly providing confirmation of previously found cloud distribution and properties. There are no major objections to its being published as is, although there are some minor points of clarity listed below. However, the "synergy" material is not well motivated: most readers will not understand the interest in these "radiative" differences between instruments nor the value of these results – as presented this material is very technical, oriented to the details of remote sensing of clouds. I recommend that the authors add some text to motivate interest in these details from a cloud physics perspective and summarize the conclusions by contrasting them with "simpler" models of cloud properties. In other words, these details provide important information about the characteristics of clouds that contrast with simple ideas like considering a cloud to be a uniform slab of material as they are in GCMs today. A second important matter is that the authors match CALIPSO and CloudSat to AIRS by sampling but do not present any test results to show what effects this has on their comparisons, even though they use this as an explanation of disagreements. Since they can use the full resolution CALIPSO/CloudSat data to test the effects of their sampling, I think that they should present some test results in the paper.

Page 2, 1st paragraph: Two sentences are repeated, starting "To resolve..." and "Time sampling..."

Page 4, Section 2.1: It should be highlighted here that although the datasets are spatially colocated, they are not always or ever coincident in time. This is especially true of the AIRS cloud results and temperature-humidity profile results. How large can the time mis-match be?

Page 5: The discussion of the CALIPSO and, especially CloudSat, products suggests that the author is unaware of the problems these instruments have with clouds near the surface, a location that is also very difficult for AIRS-based retrievals. Looking at Figure 8 suggests to me that there may some distortions of low cloud occurrence near the surface that the authors should discuss.

Page 6, near the top: A lot of sub-sampling of the CALIPSO results is being done in the matching to AIRS results – later in the paper, this is blamed for some of the disagreements. Since the CALIPSO (and CloudSat) data have many more samples in principle for each AIRS domain, I believe the authors are obligated to demonstrate that the sampling either does or does not cause the disagreements by testing some data with full resolution CALIPSO/CloudSat in comparison with sampled results. This seems lazy.

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Page 6, at the bottom: Here is some text that may be left over from the previous study because it implies that the TIGR dataset and radiance calculations are for the tropics only – this contradicted in later text.

Page 10, referring to Figure 2 (and 3): The selected threshold for the brightness temperature difference used to better discriminate clouds over ice and snow seem badly biased towards over-detecting clouds. It seems obvious from the figures that a threshold of – 2K would be much better than – 5. They never show the equivalent figures for snow-free land, so the same comment might apply. The authors are obligated to explain their choice.

Page 11, beginning of Section 2.5: Here is one place where the authors blame the sampling for an aspect of their comparison results – they can test this but did not. They should test this – maybe it is not actually true.

Page 12, beginning of Section 3: The authors introduce an ad hoc assumption of counting clear sky pixels as partially cloudy. Based on what tests? What is the basis for this? Are they just trying to make the results look better? This should be dropped from the paper unless some evidence in support is presented.

Page 13, referring to Table 2: It would be easier for the reader to understand what the results are if DIFFERENCES were shown instead of whole bunch of numbers that we are expected to compare.

Figure 6 and the one-paragraph discussion of it can be dropped in favor of a comment that all the major features of global cloudiness that have been known since the beginning of the satellite era (in fact, even before that) are also found in this dataset. It is silly to tell us there is an ITCZ and midlatitude storm zone.

Page 14 (but also before and after): I'm not sure everyone will understand what is meant by "once for the uppermost cloud layer and once for all clouds detected" - I don't really know what this means. Does the second case mean that all layers are counted?

Page 15, Section 4: The remark that the more frequent optically thinner high clouds have MORE influence on the Earth's radiation budget is not obvious, since their effect depends on their optical thickness as well as their coverage. As there is no evidence presented here, this comment should be dropped – there are other studies in the literature that have quantified this point that the authors could cite.

Sections 4.1 and 4.2: Why would anybody care about these results?

Page 18, beginning of Section 4.3: Why is the location of the "thermal" tropopause (this isn't standard terminology) taken from GMAO instead of AIRS products? This seems odd.

Page 19, top of page: Do you really mean to say that the penetrating convective cloud tops are LOWER than the associated cirrus??? Or are you referring to cloud top pressure? If you are actually saying that the convection top is lower than the cirrus, this contradicts all previous studies and HAS to be explained.

The authors should try alternate formatting for Tables 1 and 2 to improve clarity; the current version takes a lot of work to figure out which numbers are which.

The caption to Figure 14 is wrong; it mentions lines when only symbols are used.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 8247, 2010.

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