

Interactive comment on “Cloud climatologies from the InfraRed Sounders AIRS and IASI: Strengths, Weaknesses and Applications” by Claudia J. Stubenrauch et al.

Anonymous Referee #1

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Stubenrauch et al. present results for a unified retrieval approach of cloud top height and cloud amount for high spectral resolution infrared sounders called ‘Clouds from IR Sounders’ (CIRS). Results for the AIRS and IASI instruments are shown and compared to other cloud climatologies assembled as part of the GEWEX cloud assessment. Discussion on ancillary data sets as used for input in the radiative transfer calculations is included, such as the AIRS team T/q retrievals, ERA-Interim T/q profiles, and IASI surface emissivity. The previously developed sounder retrieval methodology, with modifications that resulted in the CIRS retrieval algorithm, is described in detail. Corrections accounting for increases in atmospheric CO₂ that has impacts on the interpretation of cloud trends is described and is a part of the CIRS retrieval package. An a posteri-

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ori cloud detection process is described and placed in context with the cloud property retrieval approach. The cloud properties are thoroughly compared to the active Cloud-Sat and CALIPSO profilers in terms of cloud detection and cloud height accuracy. The climatological values of CIRS clouds to other A-train and ISCCP data sets are shown and described. Lastly, the paper ends with a few examples of application of the data to hemispheric differences and ENSO variability.

The AIRS and IASI sounders are extremely valuable observational tools for atmospheric sounding, including cloud properties. The authors have described a single approach that is applied to multiple sounders, and that is a valuable contribution for establishing the temporal continuity of cloud properties from these observations. A very thorough effort in comparing the cloud properties to other sensors has been accomplished. The reviewer appreciates the amount of effort over the years that it has taken to provide a multi-sensor cloud property record and the authors should be commended for that.

With that said, the paper is a slog to get through. It is long and overly detailed. There are many instances of over explaining things that could be easily covered with a reference or assuming some prior knowledge on the part of the reader. There are too many plots and subpanels of plots and at times repetitive information is presented. The two applications that are included don't have a lot of focus and the novelty of the results isn't entirely clear as currently written. With the overwhelming (and ultimately unnecessary) detail, it is hard to narrow down to the most salient points to be made and figures (or panels within the figures) that should definitely be retained and definitely should be removed. Given that this paper does need major revisions, it is mostly from the perspective of tightening up the presentation and making it easier on the reader. The material is there and seems mostly complete – it only needs to be tightened up more than its present form.

Following are some specific suggestions and comments on various parts of the manuscript:

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Title: the authors should consider a better title that is punchier and emphasizes the great aspects of using sounders for cloud properties (and not have 'weaknesses' in the title)

Abstract: it is pretty long and not very specific. For instance, lines 24-28 has a single long sentence making multiple points about the apparent cloud top/base. Is the correction for co2 really that original and worth advertising in the abstract? On lines 23-24, the 'global cloud amount' is detected clouds, not effective emissivity?

p. 5, lines 20-21: did the authors try (or consider) using a SST data set independent of the IR sounders, say, RTG-SST or the optimal approach using microwave made available at www.remss.com?

p. 5, lines 23-24: 'quite different' is not quantitative and not useful in the context of this discussion. How different were they? Line 25: the IASI and AIRS sounders will not resolve the diurnal cycle but will capture aspects of it. Lines 26-27: if it is of any help, there is a paper that describes cloud type comparisons between AIRS and ECMWF T/q:

Yue, Q. et al. (2013), Cloud-state dependent sampling in AIRS observations based on CloudSat cloud classification, J. Climate, 26, 8357–8377.

p. 6, lines 9-12: the variables should be listed here (e.g., T, q, emissivity, sfc T, etc.)

p. 7, lines 11-12: here is a good example of over explaining. Why should 'for which temperature first increases with height before decreasing' be included? This is technically only true if ascending in the atmosphere. Line 12: 'moved to the inversion layer' is not clear. Is the cloud placed at the base of the inversion? Hopefully not the top because that would be impossible in reality. Line 14: '...about 7 to 15% of the time.'

p. 8, lines 20-23: this statement is unclear. How can 'clear sky' be 'not too cloudy'?

p. 9, line 7: there is a specific QC approach that filters based on a PBest or PGood pressure level. Was this done on a per profile basis? Or were the Level 3 gridded AIRS

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Team products used?

p. 9, lines 8-9: there is a paper that describes AIRS surface temperature biases with respect to ship observations:

Dong, S., S. T. Gille, J. Sprintall, and E. J. Fetzer (2010), Assessing the potential of the Atmospheric Infrared Sounder (AIRS) surface temperature and specific humidity in turbulent heat flux estimates in the Southern Ocean, *J. Geophys. Res.*, 115, C05013, doi:10.1029/2009JC005542.

p. 9, line 11: with respect to what is the land more complex?

p. 10, line 23: is the artifact in cloud amount causing more clouds? Less clouds? Higher clouds? Lower clouds?

p. 11, line 3: base of the inversion?

p. 11, line 25: is 'not cloudy' the same as 'clear'? or something else?

p. 12, line 10: 'explainable' should be 'explained'. The paper could use a good thorough editing for clarity of English.

p. 12, lines 14-16: are there three different sigmas for the three different emissivity_i values? It appears that some of the clear will be selected as cloudy, and vice-versa. Is this correct?

p. 12, lines 20-21: the emis < 0.1 threshold is very conservative. The IR sounders will capture a lot of optically thinner clouds than that. Are the authors arguing the point that below that threshold some clear values could leak in? The paper by Kahn et al. (2008) seems to argue that the emis threshold could be lower than that:

Kahn, B. H. et al. (2008), Cloud-type comparisons of AIRS, CloudSat, and CALIPSO cloud height and amount, *Atmos. Chem. Phys.*, 8, 1231–1248.

p. 12, section 3.1: this is where the paper starts to be a real grind. Wasn't the method-

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ology of the AIRS and C/C comparison described in a previous paper(s) by the lead author? There must be a way to tighten this up and make it more concise, but I am lacking any good suggestions for that.

p. 14, start of Section 3.2: it is really nice to see that the level of agreement is very similar to the AIRS Team cloud retrievals in Kahn et al. (2008) with a finer breakdown of surface type and ancillary data. Is the fact that the percentage is slightly higher over ice/snow indicative of a loss of skill at sounding T/q over these surfaces, and Era-Interim is superior? What is different about these profiles over ice/snow? Better detection of inversions and isothermal layers in ERA-Interim?

p. 14-16: section 3.3: this section is extremely long and detailed. A lot of it seems consistent with previous paper by the first author. Around lines 31-32 on p. 15 there is one quite interesting point about opaque clouds and a reduced geometrical thickness. Could this be because the IWC is larger in these clouds and thus leads to a smaller difference between the sounders and CALIOP? This reminds me of a paper by Sherwood et al. discussing these types of discrepancies:

Sherwood, S. C., J.-H. Chae, P. Minnis, and M. McGill (2004), Underestimation of deep convective cloud tops by thermal imagery, *Geophys. Res. Lett.*, 31, L11102, doi:10.1029/2004GL019699.

p. 17-21, Section 4: another really long section with figures 8-14 that have a combined total of over 80 sub-panels. A lot of these figures are known from previous papers or are common knowledge. Some of these panels appear to show some redundant information. I would suggest trying to trim this down as much as possible and try and keep the information to the most interesting and novel bits.

p. 19, lines 25-27: I don't see why 'which might have important consequences on radiative feedbacks' should be there. Since the SW and LW budgets are not shown with respect to the different cloud types described in the paper, this is speculative. I would further emphasize that there are many other interesting things about these

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particular clouds, including the hydrological cycle, not just radiation and its feedbacks.

p. 20, lines 27-28: Are the authors suggesting that the global cloud amount should be related to the global surface temperature? Is there a previous reference that argues for this? Most studies show a relationship of the patterns of global cloud distributions, height, types, etc. can change with respect to global averaged surface temperature, but I've never seen an argument for an average global cloud amount. Also, another point here regarding surface temperature that it did not increase much. If the authors are referring to the alleged 'hiatus', I think that is basically proven that there was no hiatus (a recent paper by T. Karl at NOAA).

<http://science.sciencemag.org/content/348/6242/1469>

p. 21, lines 28-29: what is the justification to relate infrared derived cloud amount to SW reflected radiation? Are there any previous papers that have shown a correlation? The infrared derived cloud amount saturates around an optical depth of 5 or so, but the SW does not. How can the infrared derived cloud products be used to infer consistency with SW results?

p. 22, lines 1-3: how can the CAH be used as a proxy for precipitation rate? Because the ITCZ is narrower in the CIRS data, one can infer a more intense precipitation rate? I'm not sure I understand the logic used here.

p. 22, first paragraph of Section 5.2: there is no reason to have a basic tutorial on ENSO in the paper. The authors should just get to the results and describe what is novel and delete that part.

Figure 3: numbers are too small and blurry for reading

Figure 4: why bother with the right column? Weren't these differences previously described by the lead author?

Figure 6: three figures in a row describing apparent cloud top and biases with CALIOP. Need to emphasize the novel results and parts of figures that support them. The

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numbers are overlapping on the x-axis at the edges of the subpanels too.

Figure 13: can't tell the difference between open and closed red circle, red square, and red dashed line

Figure 14: the seasonal variability in latitude bands is well understood. What is new in this figure? Are there new insights between different instruments and inferences of the seasonal cycle?

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