**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Bulk microphysical properties of semi transparent cirrus from AIRS: a six years global climatology and statistical analysis in synergy with CALIPSO and CloudSat" by A. Guignard et al.

Anonymous Referee #1

Received and published: 30 September 2011

This manuscript presents results of a retrieval of transparent cirrus properties from the Atmospheric Infrared Sounder (AIRS) over a six-year period. It is a long and ambitious paper that discusses in detail the experimental set-up and assumptions, including the single-scattering ice crystal models, a sensitivity study of the retrieved parameters to these assumptions, the retrieval methodology, global statistics of the retrieval parameters (emissivity, effective diameter, ice water path (IWP), and ice crystal habit), makes comparisons with previously published TOVS retrievals, and lastly, presents a new effective diameter parameterization as a function of ice water content (IWC).

There is a good deal of interesting and publishable results contained within the paper, but they suffer from such a long paper that makes it difficult to understand what the most important and salient points are in the end. Furthermore, besides the abstract and conclusions, the clarity of the writing is not up to standards, which makes the paper an even more difficult slog. Although most of the figures are interesting and relevant to the paper, a few of them could be either condensed or removed altogether, and replaced with appropriate literature citations. Also, the parameterization presented at the end, in the reviewer's opinion, is past the 'breaking point' of too much information in the paper. There are many papers dedicated to De-IWC parameterizations that focus on this topic alone, and the authors are encouraged to remove this section, justify its applicability and perhaps test it in some climate model runs, and present this work on its own. In its current form, the validity of this parameterization cannot be described, justified, and appropriately referenced if only one subsection of the paper is dedicated to it (Section 5.4).

In summary, this paper is definitely publishable, but only with some mandatory and major revisions outlined below. These seem tenable in the normal revision period for ACP. The primary suggestions to improve the manuscript follow in a short list, and more specific comments follow below the primary suggestions.

Primary suggestions

The grammar, writing style, and conciseness need to be addressed to make the paper more readable. There are key spots in the paper that can be compressed and made shorter and more concise. The specific suggestions only cover a tiny part of the writing issues. The authors need to spend effort in making the manuscript clear.

The most important conclusions need to be drawn out and highlighted. For instance, the results on the types and distributions of ice crystal habits are completely new and important. More space in the paper should be devoted to addressing this set of results.

Some of the less critical figures should either be removed or modified.

The sub-section on De-IWC cloud parameterization does not stand on strong ground in its current form, and probably deserves its own paper down the road with a more advanced treatment of this topic.

Specific comments

Title: Perhaps it should be shortened and made clearer? Are CloudSat and CALIPSO really critical to the retrievals from AIRS, or were they only used to analyze cloud geometrical thickness? Also, 'a six years global…' is not grammatically correct.

Abstract: Even though it is on of the clearest parts of the paper, it still has some clarity issues. It should emphasize the most important results of the paper, not recap every sub-section in the paper.

p. 24673, l13: correlations between what and what?

L21: how about 'Infrared sounders have continuously observed our planet since 1979, with recent improvements in spectral resolution:'?

Some additional references that may be useful are the following:

Posselt, D. J., T. S. L'Ecuyer, and G. L. Stephens (2008), Exploring the error characteristics of thin ice cloud property retrievals using a Markov chain Monte Carlo algorithm, J. Geophys. Res., 113, D24206, doi:10.1029/2008JD010832.

Cooper, S. J., T. S. L'Ecuyer, P. Gabriel, A. J. Baran, and G. L. Stephens (2007), Performance assessment of a five-channel estimation-based ice cloud retrieval scheme for use over the global oceans, J. Geophys. Res., 112, D04207, doi:10.1029/2006JD007122.

Pavolonis, M. J. (2010), Advances in Extracting Cloud Composition Information from Spaceborne Infrared Radiances-A Robust Alternative to Brightness Temperatures. Part I: Theory, J. Appl. Meteor. Climatol., 49, 1992-2012.

Delanoë, J., and R. J. Hogan (2010), Combined CloudSat-CALIPSO-MODIS re-

trievals of the properties of ice clouds, J. Geophys. Res., 115, D00H29, doi:10.1029/2009JD012346.

Huang, H. L., et al. (2004), Inference of ice cloud properties from high spectral resolution infrared observations, IEEE TGARS, 42, 842-853.

p. 24674, l7: 'describes our AIRS'

l25: why a new paragraph?

p. 24676, l27: 'low-level'

p. 24678, when 'cirrus emissivities' are being discussed, are the authors using 'emissivity ratios' as described in Pavolonis (2010), or a different approach? This is a bit unclear.

L19, Is fig. 3 really necessary? There are so many papers that have this diagram, it could be easily covered by references.

L26-29, What AIRS channel numbers are being used? What are their noise characteristics? Have they been reliable over the entire AIRS mission? Are they in windows, on weak absorption lines, etc.? Just a thought: an improved one-to-one comparison with TOVS could be made if the HIRS spectral response functions are applied to AIRS, then the retrieval is performed, and then compared to TOVS. Not that one would want to do this here because it defeats the purpose of the AIRS retrievals, but it could help to explain the loss of information in the TOVS retrievals (e.g., reduced dynamic range in Fig. 15) compared to AIRS and tie it to the spectral resolution differences.

p. 24679, l2: 'does not improve by'

Section 2.2.2: It is not clear how p_cld is obtained if none of the CO2-slicing channels are used in this retrieval. Are these derived separately (i.e., previous Stubenrauch et al. work), and then used as inputs to this approach?

L9: what is the 'De-IWP couple'?

p. 24680, l19: no need to capitalize look-up table

l9-10: 'several assumptions about...the cloud and the ice crystals.'

L22: 'assume'

L24: 'corresponding to'

p. 24681, Sect 3.1: The small changes in the retrieved parameters when the input variables are adjusted (e.g., Table 1) seem low to me. Are these consistent with Posselt at al. (2008), Kahn et al. (2008), Yue et al. (2007), etc. and so forth? Are these bias or random values? These results need to be more closely tied to previous results, because they seem presented in a vacuum without any context. Lastly, are the values smaller than previous works because of the use of emissivity ratios and/or differences, rather than BTDs as in the other papers?

L19-21: The discussion on horizontal heterogeneity is really confusing. What is the point of this, and what are the take-home points? Is a golf ball still homogeneous even though two cloud layers exist?

p. 24682, l1-3: De only changes by 10% in the presence of horizontal inhomogeneities? Did the authors show this in the paper? This is difficult to believe. Are there references? Furthermore, where is the evidence for partially covered golfballs being smaller in population then completely covered golfballs by an order of magnitude? This seems counterintuitive to me given the global population of low-level broken clouds. Are the authors only referring to ice clouds? If so, where is the supporting evidence for this?

L18: 'Differences are small, around. . .'

L21: 'event' means what?

p. 24683, l1: What do the authors mean by 'uncertainty'? Are these 'error bars'? If so, I don't see them presented in the paper.

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L10: What is meant by a 'stable solution'? Unique? A single solution? A solution that converges well with low chi-squared values?

L13: 'accorded' should be 'paid', or something else

L14: the authors use 'cuts' many times, but it isn't entirely clear what is meant. 'Boundaries?'

L14-16: What is the total % of AIRS FOVs being retrieved for this paper compared to the total number? Reporting a sampling % is highly recommended.

Section 4.1: Are their any studies published on CALIOP-related information on ice crystal habit that could be used for comparison and context in this Section?

L25: It is not clear at all what the uncertainty in the habit type is near zero for the thickest clouds. Shouldn't the spectral shape be more similar for clouds with much different De and habits? Some clarification is warranted.

p. 24684, l1: 'slope of increase' should be modified to something like 'rate of change', also occurs elsewhere. Do aggregates + columns + uncertain sum to 100% of all cases?

L11-12: What thresholds are being referred to? Not clear.

L17-19: Previous work has showed that the retrieval sensitivity is focused in the upper few optical depth units of an ice cloud.

Sect. 4.2: Is the habit-temperature dependence consistent with previous research, especially in situ aircraft data? Lots of papers and books contain information on the temperature dependence of ice crystal habit.

p. 24685, How do the authors deduce that ice-only clouds exist for T_{cld} < 230 K? This is counter to numerous in situ observations that show clear ice crystal populations at warmer temperatures. The authors need to clarify and justify this claim. It is not consistent with in situ observations of ice clouds. Also, the cited studies (Yang et al.,

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2002; Hu et al., 2009; Riedi et al., 2010; Martins et al., 2011) are proving this point that pure ice clouds only exist at these really cold temperatures? This is not what I deduced from them or read in their abstracts.

L6-7: The 'roundness' of an ice particle needs to be defined. They are never really round, rather they have aspect ratios that vary from 1.0 to several factors higher than that, in the case of columns and plates.

L17: Figure 9 is referenced before Figure 8. The order should probably be switched, if this is correct and the reviewer didn't miss something.

L16-17: Its hard to understand what is being said, but do the authors mean that the magnitude of the seasonal cycle in cloud frequency is unaffected by the emissivity thresholds for which the clouds are kept? Is this true? The difference in the SH mid-latitudes is 74% and 75% from DJF to JJA, but is 42% to 73% for emis_cld between 0.2 to 0.85. Or am I misreading Table 2?

p. 24686, l6: How is this linked to the general circulation?

L16-17: Over all areas globally, the ocean, land?

L23-24: This is a gross oversimplification and reads more like a book for a Meteorology 101 class.

p. 24687, l1-2: what are the 'middle range values'? Is the 'dominating' shape the one that is in the majority, or the one with the largest %? Also, it is somewhat disturbing to see the majority habit type follow the land-ocean contour so clearly. Why is this the case? Is there a possible retrieval issue with the surface that was not accounted for? Why would one expect more columns over land than aggregates, and appear tightly correlated with the coastline?

L19: the ice particles aren't spherical. Refer to earlier comment about aspect ratio.

p. 24690, l3: The authors need to be specific about the channel selection. There are

dozens, if not more, AIRS channels that fall within the HIRS spectral response function

l11-13: This sentence contradicts Fig. 15. Does TOVS or AIRS have a larger dynamic range of De? According to Fig. 15, it looks like AIRS does.

Section 5.4: This is where the authors really lost me. There is a huge literature on ice cloud parameterizations for climate modeling, and in the opinion of the reviewer, it can't be addressed as an afterthought in one sub-section of the paper. Also, a vertically constant IWC is assumed from the IWP given the cloud boundaries of 2B-GEOPROF-LIDAR, which assuredly is not true. The authors could spend some time exploring this assumption more rigorously with the lidar and radar profiles that coincide with AIRS, and do a deeper analysis to obtain a publishable parameterization. As of now, it is not sufficiently justified and way too much is presented in the paper.

Table 2: Appears to be labeled a bit strange. Which sets of rows are for which ranges of emis_cld? Also, need to come up with something better than 'cuts', of which there are many instances.

Table 4: It looks like either only the mean or the median is presented. Or is it the mean for some variables and the median for others?

Fig. 1. legend: What is 'eps'?

Fig. 2. The dashed line in the legend does not look like the dashed lines in the figure

Fig. 3. Should toss this one. Can simply reference previous work. This is an obvious plot and follows from the previous ones.

Fig. 4. How critical are the vertical cloud thickness distributions for display? Can't previous papers be cited instead? Why so many panels?

Fig. 7. Labels for y-axes missing. Need labels for 'ocean' and 'land' for left and right columns, respectively.

Fig. 8 and onwards. The outline of the continents should be bolder and more visible.

Figs. 16 and 17 should go with the removal of Section 5.4.

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