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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Atmospheric Infrared Sounder Version 6 cloud products" *by* B. H. Kahn et al.

Anonymous Referee #1

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The manuscript describes a new version of AIRS/AMSU cloud products obtained with the help of improved retrieval algorithm. Compared to a previous version, cloud temperatures are determined on a ∼3 times finer spatial grid (13.5 km vs 45 km), accounting for variability within complex cloud scenes. Version 6 data set (v6) also contains new products: cloud thermodynamic phase, ice cloud optical thickness, ice cloud effective diameter, and ice cloud top temperature. This, combined with a broad spatial coverage of the AIRS sounder makes v6 a high demand product for the community.

Overall, the paper is well organized, well written, and is detailed enough to understand the approach and the results. I had difficulties only with two parts, which I address in "General comments" section. I believe that these parts are essential for the quality of the paper and they both require additional calculations, so I have chosen "major

revision" from the list of options. I am looking forward to see an updated version of the manuscript.

General comments

1) The manuscript describes improvements to existing methodology of the cloud parameter retrieval from the AIRS observations, in comparison to an earlier NASA retrieval. However, the most interesting and most difficult methodological part is underrepresented (the case studies described in page 7 and 8 are already dealing with measured radiance). Section 2.1 "What is new in Version 6" qualitatively describes the new algorithm, but it does not provide a self-consistent study, which would be more convincing than 75 lines of text. Without this study, it is difficult to estimate the quality of the retrieval algorithm itself, especially since the authors note in lines 26-27 of page 6 that "there is the potential to create a non-existent cloud layer that in practice only fits noise". If the retrieval algorithm can take noise signal for a real cloud instead of filtering it out based on a large r.m.s. of the deviation of radiances, this raises certain questions regarding the approach. I would suggest supplementing Section 2.1 with the following exercise: two cloud layers are considered, full pixel coverage is assumed, four test situations are modeled – two layers, lower cloud, upper cloud, no clouds. Forward radiance is calculated in the corresponding channels; realistic noise is added to each channel; standard retrieval procedure is performed. Radiance deviations (one can sort them in an ascending mode for the presentation purposes) should be shown for each case, and the solution search described in lines 29-31 of page 6 should be presented graphically. This exercise can be repeated for two or three values of cloud optical depth and shown in one plot.

2) I believe that the manuscript would benefit from extending the demonstrated dataset for at least one month in boreal summer (or better yet for a whole year). Otherwise, it creates a feeling that the retrieval is so slow and complicated that only one month of data could be produced during the time of the manuscript preparation. Having four seasons in two hemispheres will give a representative picture of the updated dataset

and will create a better image of the work as a whole.

The authors refer to other cloud datasets, as well as to an internationally coordinated assessment activity. The latter has led to a database which is available since the beginning of this year. To show how the results of the presented dataset compare with the other existing datasets, especially with the one derived from the same AIRS observations (on a statistical basis), it would be important to include a short comparison, for example in the form of a table.

The assessment activity has shown that distributions of cloud properties like tau and De depend strongly on retrieval filtering: the authors introduce a quality flag for the retrieval of these quantities and show the statistics of retrieved data for different quality flags (Table 4). One has to note here that using only good or best quality retrievals introduces a bias in the statistics towards, for example, optically thicker or optically thinner clouds (please, see the specific comments to page 46).

Specific comments

Page 1, line 1 – I don't know an internal policy of naming the datasets in this case. Perhaps, the authors have to specify that this is a "Science Team Version 6" to distinguish it from other retrievals?

Page 2, line 18 – IPCC AR4 is not defined. I would suggest putting all the abbreviations to the Annex for the sake of readability. In this case, one can skip some definitions in the text and just add a note in the beginning.

Page 3, line 18 – perhaps, one has to complete the sentence: "climate sensitivity to radiative forcing" since this is the first time when climate sensitivity is mentioned.

Page 4, line 27 – The AIRS cloud retrieval algorithm described in [Kahn, et al., ACP, 2008] required far less channels. Please, explain.

Page 5, line 2 – how does fixing the surface temperature affect the retrieval of low clouds? What is meand under "fixing" atmospheric parameter here? Please, explain.

Page 6, lines 24-31 – please, see the general comments.

Page 7, lines 13-15 – "more accurate determination" should be supplemented with the actual numbers.

Page 8, line 3 – the "summary" is "summarized". Please, re-phrase.

Page 8, line 4 – are the values in Table 1 area weighted? If not, this can bias the statistics. Please, specify.

Page 8, lines 6 and 19 – what is meant under the term "cloud signal" here?

Page 9, lines 2-16 – this is only an indirect proof of the improved methodology. No change is required here, I just draw the authors' attention to a lack of direct demonstrations of the quality of the retrieval approach using single retrievals.

Page 10, lines 16, 21 and below (i.e. page 14, line 6) – the names of the fields alone can be used in the description of a data product. However, if they enter the formulas (like formulas 3-5), it creates a strange mixture of scientific and programming styles. Please, consider some simplification/unification of the notation.

Page 11, line 29 – "60% of all liquid clouds are identified by AIRS as unknown" – this is a large value. Is it related to a previous version? How is this issue addressed in a new version? Please, specify the name of the algorithm in the text.

Page 13, line 9 – "are retrieved in log space to prevent negative values". I did not understand this explanation since the log space and a "normal" one are in one-to-one correspondence. Is it related to some kind of extrapolation? Please, explain.

Page 16, lines 17 and 21 – these sentences contradict with each other. If the computational expense is high then it would be logical to optimize the selection of the channels.

Page 17, line 8 – please, see the general comments. I would suggest showing more examples to balance the methodological part of the manuscript.

Page 46, Table 4 – why does De retrieval never produce “Best” quality retrievals? Please, supplement this table with averaged tau values for each column.

Page 47, line 2 – color bar titles are barely readable (the same is true for Fig. 5,6, and 8)

Page 52, Fig. 6 and lines 4-7 – the “phases” here are not “natural” phases like “liquid/ice”, so it can mislead the reader. The numbers should be explained in the legend or, perhaps, one can also introduce some names in the text.

Page 55, Fig. 9 – is the binning always linear or the “Tau” histogram has a log scale binning?

Page 56, Fig. 10 – same as above.

Page 59, Fig. 13 – Please, add the latitude and longitude values to the axis since the contours are not always visible.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 14477, 2013.