Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-1216-RC2, 2018

Atmospheric Chemistry and Physics Discussions

ACPD

Interactive comment

Interactive comment on "Ice cloud microphysical trends observed by the Atmospheric Infrared Sounder" *by* Brian H. Kahn et al.

Anonymous Referee #2

Received and published: 6 March 2018

This is a review of the paper titled "Ice cloud microphysical trends observed by the Atmospheric Infrared Sounder" submitted to ACP by Kahn et al.

The paper presents retrievals of ice cloud properties using the AIRS instrument and discusses systematic variation of the ice cloud properties and trends seen in the record. The current paper mainly focuses on effective radius trends and variations. The paper is well written. The results are interesting and the techniques used are sound. In my opinion the results need to be related to past publications somewhat more. Also, as explained in my earlier online comment, the sampling of clouds is somewhat confusing, and makes interpretation difficult at times.

Firstly, I would like to thank the authors for replying to my first comments promptly. Those clarifications were helpful. I now understand that essentially all ice clouds

Printer-friendly version

Discussion paper

(tau>0.1) are included in the sample, the optical thickness values asymptotes at around 5, but for those thick clouds effective radius is still retrieved and included in the sample. Thank you in advance for making that clearer in the revised version of the paper.

I do have some more major comments or questions about the sampling of clouds in the paper. In additional, I have some minor comments. If the comments below are addressed in a revised version, I would recommend publication of this paper work in ACP.

1) In response to my question about what sample of clouds are included in the 'opaque' cloud selection, the authors included some more analysis in their reply. The included figures of the variation of optical thickness of opaque clouds appear to be interesting, although I'm still a bit confused about how to interpret them. The opaque clouds include clouds with optical thicknesses around 1 "as lower layer clouds may exist and this drives the effective cloud fraction to near 1.0 even though some of the upper level ice cloud may in fact be transparent." Thus, in such cases AIRS is able to retrieve the optical thickness of the upper ice cloud without interference of the lower clouds? If so, is the retrieved effective radius for these situations also not affected by the lower cloud?

Following this rationale, should I interpret retrievals of large optical thickness for opaque clouds as situations of thick ice clouds without any lower liquid clouds present to which AIRS is sensitive to. In turn, are retrievals of decreasing optical thickness for opaque clouds then associated with increasing interference of thick liquid clouds under the ice layers?

Please explain this further in the text. I suggest that the figures of the variation of optical thickness for opaque clouds are included in the paper and an interpretation of the systematic variation of ice cloud optical thickness with wind and surface temperature is provided.

2) The saturation of optical thickness at around 5 leads to a low bias of mean optical thickness for much of the globe, especially in the convectively active regions. Please

compare the global optical thickness distribution plots to those shown in King et al. (2013; reference below). King et al. (2013) report mean values generally exceeding 10, although the distribution is highly skewed towards thin clouds. Since the mean MODIS ice cloud optical thickness is dominated by the occurrence of thick clouds, the global distribution of MODIS optical thickness might be correlating better with the distribution of fraction of opaque clouds identified by AIRS. I suggest to include such a plot in the revised paper. Also, please note in section 4 of the paper that the saturation of optical thickness at around 5 also means that there is no sensitivity to any possible trends of ice cloud optical thickness of thicker clouds. The trend shown in Figures 5 and 6 are only reflecting trends in the optical thickness range for which AIRS is sensitive to. Finally, since trends in optical thickness may also lead to trends in the relative occurrence of opaque clouds, I suggest to also look at this and to describe the findings in the paper. Possibly a figure of the trends can be included in Figure 6.

3) Clouds closer than 6K to the cold point tropopause are removed from the sample for most part of the analysis. I wonder what potential influence this may have on the trends. In a warming world clouds may increase in height towards the tropopause over time. With this filtering in place, more cloudy pixels would be removed over time in that case. This may lead to an unrealistic positive bias in temperature trend and may also lead to biases on the mean effective radius and optical thickness trends. Since you are looking at rather small (but not unimportant!) trends, such sampling issues may impose relative large biases. Please investigate any possible trends in the filtered clouds and discuss it in the paper. Possibly a figure of the trends can be included in Figure 6.

Minor comments:

1) There seem to be issues with saturated colors in the global distribution plots. For example, in the Tcld plot in Figure 1, there is a white spot off the African coast that is surrounded by dark red colors. It seems that the white should be dark red. A similar thing happens in the trend plots, where regions that are off the scale on the low end are not dark blue, but white. Please inspect the plots for such occurrences and correct

the color scale.

2) In addition, please make the labels of the plots consistent with what is used in the rest of the papers (T_cld should be T_ci, etc.). Also, please check the text for consistency. At page 11, line 17, T_cld is used in the text, while otherwise T_ci is used, but there could be more of these inconsistent labelings.

3) At page 5, line 20, please provide reference(s) for the "well-documented spatial distributions of ice clouds". I suggest at least King et al. 2013. Also discuss the cloud fraction, height, optical thickness and effective radius distributions shown in Figure 1 in relation to those shown by King et al. 2013 and other relevant papers.

4) In section 4.2, trends on ice water path are introduced. Please write out "ice water path" before using the acronym. Also, please explain how IWP is determined. I suspect that is derived from the product of effective radius and optical thickness. That would mean that the absolute value is very much biased low for thick clouds because of the insensitivity to thick cloud optical thickness. Is the assumption here that the trends are not similarly affected?

5) Page 10, line 22: Van Diedenhoven et al. (2016) used airborne remote sensing data instead of in situ data.

6) Section 5.1: One of the three categories is where there is no cloud and no rain (CWP=0). This is confusing, since you are presenting cloud properties. Is "no cloud" really meaning no liquid part of the cloud? Please explain and change the nomenclature.

7) Figure 10: Please add "effective radius" to the y-axis label.

Reference: M. D. King, S. Platnick, W. P. Menzel, S. A. Ackerman and P. A. Hubanks, "Spatial and Temporal Distribution of Clouds Observed by MODIS Onboard the Terra and Aqua Satellites," in IEEE Transactions on Geoscience and Remote Sensing, vol. 51, no. 7, pp. 3826-3852, July 2013. doi: 10.1109/TGRS.2012.2227333

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-1216, 2018.

Printer-friendly version

Discussion paper