Anonymous Referee #2

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.

We thank the reviewer for the very helpful and positive comments about the paper. Our responses to the reviewer comments are detailed below.

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 (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.

This is correct. We have essentially paraphrased a portion of your comment and inserted it into the revised manuscript in the middle of Section 2.1 (p.5, line 19 in the ACPD version): '*The AIRS sampling includes nearly all ice clouds with $\tau_i > 0.1$, while the maximum values of τ_i asymptote to values near 6-8 (e.g., Kahn et al., 2015). The r_{ei} is retrieved for the same sample although retrievals with QC=2 are not included.*'

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?

We thank the reviewer for the insightful comments and careful reading of the manuscript. Given these comments and the ones that follow below, we have made some changes to the organization of the cloud categories. We now define three categories titled 'Opaque', 'Non-Opaque', and 'Multi-Layer' that are defined in a new Table 2. The Opaque category is restricted to an upper layer ECF >=0.98

(which implies that the lower layer ECF can be anywhere from 0.0 to 0.02). The Non-Opaque category is intended to capture approximately single layer clouds with an upper layer ECF < 0.98 and a lower layer ECF < 0.1. (We decided to allow for some small amounts of lower layer cloud as this value has no material impact on the results except for moving around some of the pixels from one category to another.) The Multi-Layer category is intended to capture two-layer clouds with a lower layer ECF >= 0.1 with any value of upper ECF included in this category. This category shows the impacts of a lower layer of cloud on the retrievals or on the geophysical relationships shown in the joint histograms.

Thus, to answer the reviewer comments about a lower layer impacting the fidelity of the retrieval, that is entirely possible and those effects would in theory be exhibited more strongly in the Multi-Layer category. Without going into a full sensitivity and retrieval impact study, however, we are unable to quantify the contributions of a second layer on CER and COT separately from the geophysical characteristics of this category, which may in fact be different than Opaque and Non-Opaque categories. However, with this additional category, we can to first order isolate the pixels that may be the most troublesome and are known to have somewhat lower values of information content in a multi-layer configuration (see Section 2). We have added the following text in Section 5.2 to clarify: '*Multi-layer clouds exhibit the largest changes with wind speed (Fig. 8). However, the reduced values of r_{ei} at higher wind speeds have low frequencies of occurrence (i.e., noted by the gray scale shading). The contribution of retrieval biases that arise from an additional lower layer(s) not accounted for in the forward model (Kahn et al., 2014) has not been quantified. A firm conclusion on the realism of changes in multi-layer cloud top r_{ei} to wind speed variability thus remains elusive and warrants further investigation*.'

Figure 7 now includes three panels, and Figures 8-11 now have six panels each, reflecting the Opaque, Non-Opaque, and Multi-Layer categories. The original figures 8 and 9 for CER are now Figures 8 and 10. The new Figures 9 and 11 are the new figures that contain the COT results. We point out that the Opaque variations seen in COT in the first response to reviewer #2 are in fact a result of clouds that included a significant magnitude of lower layer ECF. In removing these and placing them into the Multi-layer category, the dependence of COT is much reduced, but is still non-zero, with respect to surface winds, CWV, and Tsfc.

We have added significant new discussion taking into context these three new categories and the new COT joint histograms and have substantially revised Sections 5.1 to 5.3. Please refer to track change version for specific changes as they are too numerous to list in the response.

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?

With the results emerging from the revised categories Opaque, Non-Opaque, and Multi-Layer, we believe that the reviewer interpretation is correct to first order. Now that the Opaque category does not contain significant lower layers of cloud that may contain liquid, we see that the COT has a stronger relationship with ice Tcld rather than with surface wind speed, CWV, Tsfc, etc. The COT histograms that were a part of the first reviewer response, which exhibit large gradients with wind speed, CWV, etc., have been reduced substantially or nearly eliminated in the new diagrams with three categories. Basically, the take home message is that cloud vertical structure and cloud regime exhibit different signals, and if mixed together in the same diagram, can lead to the interpretation of gradients that do not show up in individual cloud regimes.

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.

We have included the optical thickness histograms for all of the panels depicted in Figures 8 and 9, which are now Figures 8 and 10. The COT versions of the histograms are now Figures 9 and 11. All edits are included in the track change version and are found in Sections 5.1 to 5.3.

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.

The suggestion of an additional plot showing correlations between optical thickness among AIRS and MODIS for optically thicker clouds is well taken. In Kahn et al., (2015), J. Geophys. Res., MODIS and AIRS are compared for one month of data using pixel-level comparisons sorted by scene complexity. We quote from Kahn et al (2015): '*For four positive (ice phase) tests, an approximately zonal symmetric pattern emerges and is similar to MODIS ice cloud τ distributions described in King et al. [2013]. This result is encouraging as it demonstrates the overlapping sensitivity of subsets of AIRS and MODIS ice cloud properties for thicker ice clouds*.' Instead of reproducing material from a previous paper, we decided to point the reader to Kahn et al. (2015) paper where a very detailed comparison between MODIS and AIRS is described. We have added text following that added above describing AIRS sampling: '*Kahn et al. (2015) describe pixel-level comparisons between AIRS and MODIS ice cloud properties and show that overlapping sensitivity for both τ_i and r_{ei}*

are maximized for optically thicker pixels containing four positive ice phase tests with spatial maps resembling those described in King et al. (2013).'

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.

We have added the following text in the beginning of Section 4.1 to clarify: '*We reiterate that the AIRS sensitivity to ice clouds is limited between 0.1 < τ_i < ~6-8, thus the τ_i trends do not include potential trends outside of this sensitivity range*.'

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.

This is a good question and is something we are currently working towards answering. We intend on submitting a separate manuscript that addresses trends in ECF, Tcld, thermodynamic phase partitioning in the extratropics, and tie these results together to the present work, which is focused on ice microphysics. The trends in opaque clouds are somewhat ambiguous. We are still trying to determine the best approach to calculate these trends, which variables to use, and how to filter and/or classify the data. We have calculated the trends for ECF with regard to all ice clouds and are included as Fig. 3 of this response (Figs. 1 and 2 are in the response to reviewer #1). The trends are not statistically significant at the 95% level.

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.

The reviewer's comments above are well taken and we did not discuss this in the manuscript. We have repeated the trend analysis for no 6 K filtering within the tropopause and the results are included as Figs. 4 and 5 in this response for τ_i and r_{ei}, respectively. The results are virtually identical to those shown in Fig. 6 in the submitted manuscript. The tropopause filtering has no material impact on the sign, magnitude, and statistical significance of the zonal band trends. All other variables shown in Fig. 6 in the submitted manuscript yield very similar results upon manual inspection but we did not include them here because of prioritization of the reviewer response subject matter and author time constraints. To address this point in the revised manuscript, we have included the following statement in Section 4.2: '*The*

***results in Fig. 6 have clouds within 6 K of the tropopause filtered out; very similar results are obtained with no filtering, and no material changes in sign, magnitude, and statistical significance are found.*’**

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.

In these cases the values went beyond the color scale. These have been fixed. We point out in the figure caption that areas at either end of the color scale may contain values lower or greater than the color scale indicates. Instead of stretching the color scale to minimum and maximum values, we prefer not focusing on the outliers and instead show the more interesting structure in a more narrow range.

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.

This is a good catch that requires some additional explanation. Figures 7-11 use the upper level cloud top temperature Tcld from the standard cloud clearing retrieval on the y-axis. Tcld is used as the prior guess to Tci (additional text to clarify on p. 4). We have made identical plots with Tci and they are generally very similar but there can be a few changes especially in the 230-250 K range. Recall that Tci is derived assuming a single layer ice cloud while Tcld is derived for up to two layers; thus the upper layer Tcld is judged to be somewhat more precise in multi-layer clouds (see Yue et al., 2017b, AIRS Version 6 Test Report). We are currently working on a follow-on manuscript that addresses these and other cloud variables other than ice microphysics. We will discuss and reconcile the different cloud top temperature estimates and their trends, as this topic deserves a separate venue for discussion.

We have added the following text to Section 3.3 to explain the choice of the y-dimension: ‘*As discussed in Kahn et al. (2014), the T_{ci} variable is included in the retrieval state vector to improve the chi-square radiance fits and the success rate of retrieval convergence. While there are strong similarities between T_{ci} and the upper level T_{cld}, some differences arise within multi-layer clouds as expected since T_{ci} is based on the assumption of a single-layer cloud (Kahn et al., 2014). Further discussion on the reconciliation of the two cloud top temperatures is in progress and will be presented in a separate manuscript.*’

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.

We added King et al. (2013), Wylie and Menzel (2005), and Stubenrauch et al. (2013) to point the reader to standard references for ice cloud property distributions from a variety of satellite remote sensing data sets.

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?

We now spell out ice water path (IWP) at first usage in section 4.2. The IWP is calculated using a standard relationship between τ_i and r_{ei} and have included a brief description of the calculation: '*The IWP is calculated using the relation IWP = (2/3) $\rho_i \tau_i r_{ei}$, where ρ_I is the density of ice (0.92 g cm^{-3}).*' Indeed, the trends for the optically thick clouds are likely biased low. We refer reviewer #2 to the response to reviewer #1 regarding using other data sets. We showed trends for MODIS in the response, and in fact, the COT trends are larger in the convective areas compared to AIRS, but the patterns are very similar. This topic deserves further examination but is well beyond the scope of the present work as we discussed in the reply to reviewer #1.

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

Fixed.

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.

Since passive microwave radiometry is sensitive to LWP and not IWP, we describe in the text that this is due to liquid condensate and not ice. We have changed all uses of CWP to LWP in the text, figures, and tables.

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

Fixed.

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

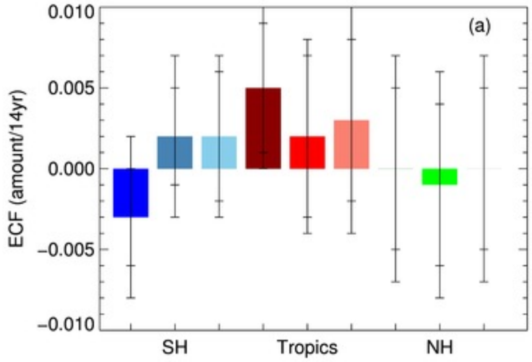

Figure 3. AIRS ECF trends for all ice clouds for the three latitude bands and three sampling/algorithm categorizations shown in Fig. 6 of the manuscript.

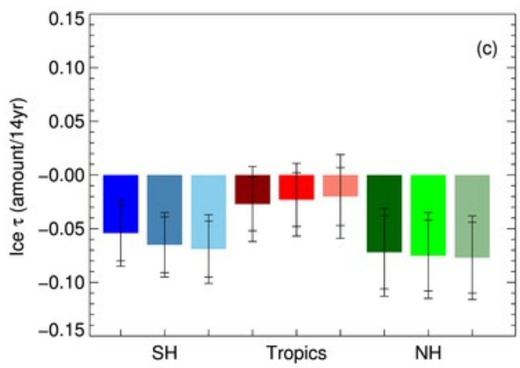

Figure 4. AIRS ice cloud COT trends for the three latitude bands and three sampling/algorithm categorizations shown in Fig. 6 of the manuscript. No 6 K filtering is applied. The results are nearly identical to the 6 K filtering version.

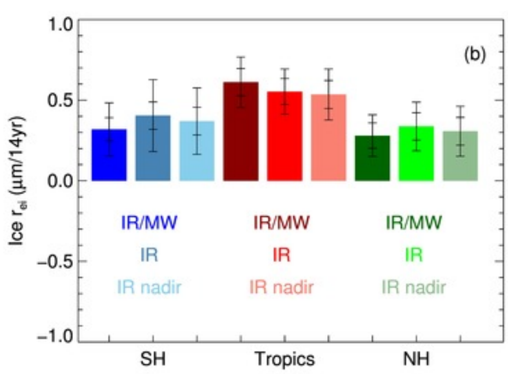

Figure 5. AIRS ice cloud CER trends for the three latitude bands and three sampling/algorithm categorizations shown in Fig. 6 of the manuscript. No 6 K filtering is applied. The results are nearly identical to the 6 K filtering version.