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Interactive Comment

Interactive comment on "Tropical thin cirrus and relative humidity observed by the Atmospheric Infrared Sounder" *by* B. H. Kahn et al.

Anonymous Referee #2

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The authors present early results from analysis of a unique combination of in cloud relative humidity and ice cloud microphysical property information from the AIRS instrument aboard Aqua. Considering the current high level of interest in improving our understanding of processes relating the occurrence and radiative properties of high thin clouds and the moistening/dehydration of the upper troposphere and lower stratosphere, this work is very relevant and contains suitable subject matter for Atmospheric Chemistry and Physics Discussions. Overall the paper is well written and the physical basis for the datasets analyzed has been shown to be sound in prior publications. The analysis is reasonably thorough, all relevant assumptions are clearly stated, and a thoughtful treatment of resulting uncertainties is offered along with frequent discussion of their impact on the interpretation of the results. As a result, the paper is suitable for publication subject to the authors addressing the following concerns. I don't believe they should require major modification of the manuscript but do feel that it is important they are addressed to clarify the interpretation of the results.

As noted above, the authors are very rigorous in stating all assumptions made in the analysis but the interpretation of the requirement that effective cloud fractions be between 0.02 and 0.4 is not entirely clear to me. The text seems to suggest that there is a fairly straight forward connection between fA and optical depth but it is not at all clear why this should be the case and, more generally, how one should interpret the effective cloud fraction from AIRS? The significance of this parameter is really never fully explained anywhere in the text and is not even mentioned in the conclusions.

Even more fundamentally, there is little mention of the impact of partial cloudiness within the AIRS FOV on the interpretation of the results. I realize that there is not a direct connection between fA and physical cloud fraction but is it not possible that when fA is less than unity there could be cloud free areas within the FOV? If so, then why do the authors focus exclusively on cloud vertical thickness as the most likely cause for the apparent dry bias in the current analysis relative to in situ observations? I would think that the same argument regarding the presence of drier air in the cloud free regions that was used in support of the cloud thickness argument should apply to horizontal inhomogeneity in the cloud field as well. It seems that the analysis in Figure 11 could be repeated to examine the effect of partial cloudiness by plotting the mean in cloud RH as a function of physical cloud fraction (even if CALIOP provides only a 2D measure) for clouds of varying thickness.

A smaller point, but I was also wondering if the specific humidity limits discussed on page 16198 might themselves depend on fA? It seems to me that the more cloud present the more water vapor one would require to obtain a measurable signal.

I also feel that more discussion is warranted concerning how the low bias in AIRS high cloud heights is dealt with. It was unclear from the paper whether or not anything was done to remove this bias, i.e. by systematically increasing the cloud heights for the analysis. If not then there are a number of consequences that may have important impacts on the results. First, the uncertainties in Tc cannot possibly be assumed to be normally distributed with a sigma of 12 K in the error analysis. The bias must be removed prior to these perturbation analyses. Second, and more importantly, in calculating IN CLOUD relative humidity, the temperature and humidity of the layer that directly corresponds to where the cloud physically resides must be used. If, on average, the T and q from 2.5 km below cloud are used to compute RH then one would again expect a significant low bias relative to more precisely matched cloud/RH observations (note that according to the authors, AIRS T and q have a vertical resolution of between 2-3 km so this error in cloud height could lead to an offset large enough to be resolved by the instrument). It might also be worth noting that the nighttime differences between AIRS and CALIPSO cloud top height estimates are probably more representative of the true AIRS cloud height biases than those obtained during the daytime. Scattering of solar radiation during the daytime causes noise in the lidar observations that effectively raises the noise floor and reduces its sensitivity during daytime hours. As a result, CALIPSO detects cloud top more accurately at night.

The treatment of cloud height notwithstanding, I applaud the authors for their effort in conducting the error analysis presented in section 2.3. I feel, however, that there are two important clarifications that should be made here. The first is a simple reiteration of the fact that errors in the assumed ice crystal habit model are not addressed here but Cooper et al. (2006) demonstrate that errors in crystal habit can lead to large uncertainties in retrieved optical depth and effective diameter from MODIS observations. Second, I think it is worth noting that covariances between the various error sources themselves have not been considered (at least it is not clear that they have). It is, however, quite reasonable to expect uncertainties in AIRS T, q, and even cloud height to be correlated with one another. In fact, the method of perturbing all variables randomly to represent distinct uncorrelated Gaussian error distributions likely provides an upper bound on the errors expected in the retrieved optical depth and effective diameter (worth noting). Finally, given the vast amount of cloud property information provided by MODIS along the same track, I wonder if the authors have tried "evaluating" the results of these sensitivity studies by directly comparing their retrieved optical depth and effective diameter retrievals against those from MODIS that derive from a somewhat more sophisticated radiative transfer model and use different wavelengths than those applied here.

References:

Cooper, S. J., T. S. L'Ecuyer, P. Gabriel, G. L. Stephens, A. J. Baran, and P. Yang, 2006. "Objective Assessment of the Information Content of Visible and Infrared Radiance Measurements for Cloud Microphysical Property Retrievals over the Global Oceans. Part II: Ice Clouds", J. Appl. Met. 45, 42-62.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 16185, 2007.