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Atmospheric
Chemistry
and Physics
Discussions

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

B. H. Kahn et al.

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Anonymous Referee #2:

Author response: The authors are grateful to the reviewer for his/her thoughtful and helpful comments on the manuscript. The authors believe the suggestions have led to significant improvements in the manuscript. The authors have responded to each reviewer comment and concern below. In the case that the manuscript was modified in response to a comment, the changes are highlighted.

Referee: 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

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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.

Author response: Thanks to the reviewer for the thoughtful review, and we hope that we have appropriately addressed all of the reviewer’s comments below.

Referee: 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 straightforward 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.

Author response: With regard to why AIRS FOVs with fA $<$ 0.02 are not considered, we cite the significant number of spurious cloud retrievals from validation studies (see Fig. 3 and Kahn et al., 2007c). This is mentioned in the original manuscript on p. 16193, lines 1-3.

With regard to the upper limit of fA = 0.4, as cloud amount increases within the AIRS FOV, scattering increases and renders the thin cirrus approximation of Yue et al. (2007) less accurate. Scatter plots of fA and retrieved OD show a fairly significant correlation (not shown) between the two quantities when OD $<$ 0.5-0.8 and fA $<$ 0.3-0.4. The

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decreased correlation at higher values is a consequence of the increased importance of scattering for more opaque clouds. The authors constrained cirrus retrievals to fA $<=$ 0.4 to limit the effects of scattering, although 0.4 is somewhat arbitrary and could have been lower or higher to be more or less stringent, respectively. Please see the author response to point (3) by the first reviewer for further discussion on the OD-scattering relationship. To clarify in the manuscript, the following text on p. 16191 was changed from:

‘… much less accurate above OD $>=$ 0.5-1.0 when scattering begins to dominate …’

to the following:

‘… much less accurate above OD $>=$ 0.5-1.0 or fA $>=$ 0.3-0.4 when scattering begins to dominate …’

With regard to the interpretation of fA (effective cloud fraction), this quantity is the product of cloud emissivity x cloud fraction within the AIRS FOV. It is discussed at length in Kahn et al. (2007a,c). In order to clarify the manuscript according to the reviewer’s comment, we have added the following text to p. 16191, line 24:

‘The quantity fA is a product of cloud emissivity and cloud fraction within an AIRS FOV. Values less than 1.0 may arise from the presence of transmissive cloud or partial cloud coverage within the FOV (Kahn et al. 2007a,c).’

Referee: 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?

Author response: Yes, this is entirely possible and consistent with the definition of fA described above. So, fA $<$ 1.0 can result from partial cloud coverage or transmissive cloud. As a result, there may be clear portions within the FOV that could impact the interpretation of the in-cloud RHI distributions.

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.

Author response: The authors have performed some additional analysis following the very good suggestion of the reviewer to quantify any potential dry bias introduced by heterogeneous clouds within the AIRS FOV. There is a revised Fig. 11 to demonstrate the results. The curve for the tropical oceans is split into three parts: (1) 1-4 CALIPSO lidar-detected cloud features nearest to the center of an AMSU FOV, (2) same as (1) except for 5-8 features, and (3) same as (1) except for 9–12 features.

In summary, the effects of heterogeneity are largely negligible as shown by Fig. 11. Both the mean and variability of RH_ic are essentially independent of CALIPSO-defined cloud heterogeneity. Although a relationship is somewhat observable within thicker clouds (higher fA and OD, but this is not shown in the paper), these clouds are not the focus of this work, there are quality limitations of derived temperature and specific humidity within thicker clouds that must be carefully considered, and this is the subject of future investigation. Thus, we have inserted new text into the manuscript to emphasize these latest results (p. 16203, line 3):

‘Furthermore, Fig. 11 demonstrates the negligible effects of horizontal cloud heterogeneity on the relationship between RH_ic and dZ_ci. For AMSU FOVs over the tropical oceans with varying degrees of horizontal cloud heterogeneity as defined by the 5 km CALIPSO cloud feature mask, the correspondence of RH_ic and dZ_ci is virtually independent of horizontal cloud heterogeneity when fA <= 0.4. Values of fA > 0.4 suggest that there is some dependence of RH_ic and dZ_ci on horizontal cloud heterogeneity (not shown), but it is not entirely clear if these are a result of biases in RH_ic retrievals within thicker clouds; this remains the subject of future work. These results are consistent with biases introduced by the geometrical thinness of cirrus relative to the vertical resolution of RH_ic that swamp any effects due to horizontal cloud heterogeneity.’

On p. 16202, lines 26-27, the sentence has been modified to the following: ‘…the mean RH_ic increases from 60% to 90% for dZ_ci increases of 0.5 to 4 km, a change of 30% over the range of dZ_ci.’

The figure caption now reads as: ‘Fig. 11. Relationship of RHic to geometrical cirrus cloud thickness derived from collocated AIRS–CALIPSO observations (Kahn et al. 2007c). Only oceanic AIRS FOVs containing single-layered clouds with 0.02 <= fA <= 0.4 for ś 70° lat (green) and ś 20° lat (gray, red, and black) are used. For the tropics, three bins are defined that are based on the number of coincident 5 km CALIPSO lidar-detected cloud features (1–4, 5–8, 9–12) nearest to an AMSU FOV. For latitudes ś 70°, one bin with 6–12 coincident cloud features is shown. Horizontal bars indicate 1σ-variability for each 0.5 km cloud geometrical thickness bin. Cloud thickness bins containing less than 40 data points are not included.’

Referee: 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.

Author response: The lower sensitivity limit of specific humidity (q), and its possible dependence on various conditions (e.g., cloud amount), is not exactly known. Estimates of a lower limit are only known from validation studies (e.g., Gettelman et al. 2004). As long as there is sufficient cloud variability within the AIRS 3x3 array for cloud clearing to be effective (hence, a more accurate cloud-cleared radiance spectrum leads to a more accurate T and q profiles), there should be little dependence a lower limit of q on cloud amount. However, if the cloud somewhat uniformly occupies the AIRS 3x3 array, this may affect the lower sensitivity limit. In AIRS V5, averaging kernels are being derived from AIRS radiances for q and trace gas species. At this time, the averaging kernels are being used to compare more carefully and systematically q with coincident radiosondes in the presence of variable cloud amounts, and the reviewer’s question will be quantitatively addressed by these efforts.

Referee: 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.

Author response: The authors completely agree with the reviewer. We inadvertently included a version that only has Gaussian noise with 1-sigma = +/- 12 K. We have modified Figs. 4a-c, 4e, 4g, and 4i to include the effects of a constant Tc bias along with the random noise. The constant Tc bias is set to -12 K (to account for the low height bias/high Tc bias) and the Gaussian noise of +/- 12 K is retained as well. The selection of these values is consistent with the results of Fig. 3, approximately equivalent to 2.0-2.5 km of bias and variability in the upper troposphere.

Overall, the shapes and magnitudes of the PDFs in Fig. 4 change little. The biggest change is a slight reduction in OD for Figs 4a-c. A smaller change is that the mode near OD = 1.0 in Fig. 4c is now gone. This is a result of the small reduction in OD.

The Fig. 4 caption has been modified to explicitly mention the Tc bias, where the following has been added after the 2nd line: ‘Furthermore, Tc is adjusted by a

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Discussion Paper

bias of -12 K to be consistent with Fig. 3.’

The following are textual modifications and additions to the manuscript in response to changes in Fig. 4:

Added sentence to p. 16193, line 11: ‘The bias of 2.0-2.5 km in cloud top height is corrected by subtracting a constant -12 K from Tc.’

Removed sentence from p. 16194, line 11, to remove discussion of spurious mode that is gone after bias correction: ‘A spurious mode at OD = 1.0 arises from imposing a limit on OD at 1.0 in the retrieval minimization.’

Referee: 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).

Author response: The authors are in complete agreement with the reviewer. There are at least two (related) reasons why adding a bias correction to Tc may be problematic for calculating in-cloud RHI. First, adding a constant bias to Tc to obtain a more representative in-cloud RHI may introduce additional biases and variability because of the significant scatter (see Fig. 3). Second, adding a constant bias to obtain RHI at cloud top isn’t desirable because it is likely to be too dry because the weighting function extends above cloud top. A more reasonable correction is near 1.0 km to maximize the signal within the cloud layer. The authors conclude that the uncertainties that result from bias corrections necessitate a different, more rigorous approach to address the reviewer’s point.

To quantify whether a systematic dry bias is introduced when calculating RHI at the al-

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titude/Tc of IR-derived cloud tops, we calculated an in-cloud RHI PDF using CALIPSO-derived cloud profiles (L2 5 km CLay cloud product). RHI is interpolated (log-linear) to all altitudes where CALIPSO observes a cloud (i.e., at every altitude bin between the cloud tops and bases). Thus, the RHI PDF represents all in-cloud RHI values, not just those at the IR-derived cloud top.

The authors have added a new Fig. 12 to the manuscript. It comprises two RHI distributions: (1) a mean RHI PDF from Fig. 7a (across all OD bins), and (2) in-cloud RHI using CALIPSO (as discussed above), but limited to AIRS FOVs with 0.02 $<=$ upper layer fA $<=$ 0.4. The new PDF is essentially identical to Fig. 7 except that it contains RHI throughout the entire cloud. The differences between the two PDFs are not large. First, the peak RHI is very similar. If anything, the CALIPSO-derived PDF is up to 5% drier at peak frequency. Second, the frequency of the driest and wettest values for the CALIPSO-derived PDF increases significantly over those shown in Fig. 7a. Thus, it is apparent that the IR-derived cloud top does not necessarily introduce a dry bias in in-cloud RHI. Rather, a narrower PDF results because a smaller number of extreme RHI values exist at the IR cloud top height.

Along with the addition of the new Fig. 12, the following additions and changes have been made to the manuscript to describe these results:

The title of Sect. 3.3 is changed to ‘Vertical structure of thin cirrus and dry biases in RH_ic’

The new Fig. 12 caption reads as follows: ‘Fig. 12. Mean RH_ic distribution from Fig. 7a averaged over all OD bins (dotted gray), and the RH_ic distribution for clouds that are observed by CALIPSO, constrained to AIRS-observations with 0.02 $<$ fA $<$ 0.4, for July 2006 (solid black). RH_ic is interpolated (log-linear) to all height bins located between cloud tops and bases identified in the CALIPSO L2 CLay 5 km product.’

The following text has been added to p. 16203, starting at line 7 as a new paragraph:

‘An additional candidate that may cause a dry bias in RH_ic is the high bias in AIRS-derived Tc (or low bias in cloud top height), illustrated in Fig. 3. Rather than applying an ad hoc correction to Tc to compensate for the bias, we employ a more rigorous approach using the L2 5 km CALIPSO cloud feature mask to determine the altitudes at which RH_ic is to be calculated throughout the depth of all cloud layers. In Fig. 12, results for July 2006 are shown and compared to the average RH_ic distribution derived from Fig. 7a. The value of RH_ic that contains the peak frequency is at most 5% drier using the CALIPSO cloud profiles. Although the days used in Figs. 7 and 12 are not identical, the temporal sampling induces little sensitivity in RH_ic (not shown). The RH_ic distribution using the AIRS-derived Tc contains fewer of the moist and dry observations compared to the CALIPSO-derived distribution. In summary, when using the entire cloud profile to calculate RH_ic distributions, there is little to no change in the mean bias compared to the Tc-derived distributions. Rather, this approach shows that more of the dry and moist variability is captured by sampling the entire cloud layer.’

Referee: 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.

Author response: Indeed, the authors agree that this point should be highlighted in the paper. Thus, we have added the following text to p. 16192, line 24 (just after the first mention of diurnal differences): ‘The slightly higher differences found at nighttime are consistent with the increased sensitivity of the lidar in the absence of scattering from solar radiation.’

Referee: 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.

Author response: Thanks to the reviewer for raising the issue of ice crystal habit impacts on De and OD. In Cooper et al. (2006), the effects of ice crystal habit are much larger in the near IR and VIS wavelengths (e.g., their Fig. 5) than the IR. However, in Wendisch et al. (2007) (J. Geophys. Res.), ice crystal habit distribution uncertainties can change thermal IR radiances up to 70% for thin cirrus (OD ˜ 1.0). The sources of the discrepancies between these papers are not entirely clear, but both demonstrate some degree of sensitivity. Thus, we have added the Cooper et al. (2006) and Wendisch et al. (2007) references and have modified the manuscript with the following text on p. 16191 on line 16:

‘Although this simplifies the interpretation of De and more readily facilitates a comparison to MODIS, other studies have demonstrated varying but significant impacts of ice crystal habit and size distributions on thermal IR radiances (Cooper et al. 2006; Wendisch et al. 2007; Yue et al. 2007) and is suggestive that the habit distribution may be a retrievable physical quantity in future retrieval efforts (Baran and Francis 2004).’ [The last reference has been added to the modified manuscript, too.]

Referee: Second, I think it is worth noting that co-variances 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).

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Discussion Paper

Author response: The authors fully agree with the reviewer that the various quantities considered in this study may have non-zero co-variances under some conditions. This topic is worthy of future research efforts and will be actively pursued by the lead author (B. H. Kahn) in regards to cirrus retrievals using combinations of MODIS and AIRS radiances within an optimal estimation framework. To specifically address the reviewer’s point in the manuscript, the following text has been added to p. 16193, line 13:

‘Furthermore, the error perturbations assumed in Fig. 4 are not correlated between the different physical quantities, although non-zero co-variances may exist in the observed atmosphere. Thus, the calculated errors in De and OD shown in Fig. 4 may be an overestimate since correlated errors will effectively reduce the width of the PDFs.’

Referee: 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.

Author response: The first author has collaborated with several other researchers to evaluate the AIRS-derived cirrus retrievals against the operational MODIS and 1.38 micron approaches. Please see the author reply to the first reviewer’s last comment. These efforts are ongoing and will be published elsewhere.

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

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