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Interactive Comment

Interactive comment on "Diurnal variations of humidity and ice water content in the tropical upper troposphere" *by* P. Eriksson et al.

Anonymous Referee #1

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A review of 'Diurnal variations of humidity and ice water content in the tropical upper troposphere' by P. Eriksson et al., for publication in Atmos. Chem. Phys.

This is an interesting paper that combines ice water content (IWC) observations from the CloudSat and Odin-SMR platforms and relative humidity with respect to ice (RHI) from the Aura Microwave Limb Sounder (MLS) and Odin-SMR to investigate the diurnal cycle of IWC and RHI in the upper tropical troposphere. Since Odin-SMR orbits with a local crossing time around 0600 and 1800 and the others (in the A-train) sample at 0130 and 1330, a rough estimate of the diurnal cycle can be obtained. Offline retrievals of IWC for both instruments were performed for better consistency and to eliminate potential biases from different algorithm assumptions. The mean values, phase and amplitude of the diurnal cycle of IWC and RHI were compared to several

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climate and weather model climatologies for several regions of interest and differences and similarities were quantified with respect to modeling and observational limitations. Overall this is a useful paper that deserves publication after some important revisions are made in response to various points of clarifications listed below.

General comments:

Part of the introduction is lacking substance with regard to the sensitivity of IR sounders to sensing RHI in the upper troposphere. On p. 11714, lines 2-7, it is true that IR sounders saturate around and optical depth of 5 or so, but many (or most) clouds have values less than that. Values of RHI can be obtained in many tenuous clouds. Furthermore, IR sounders are very useful for sounding around edges of thick broken clouds, too. There have been several studies that have quantified the precision of AIRS by cloud amount (Susskind et al. 2006), in the presence of thin cirrus (Lamquin et al. 2008, and Kahn et al. 2008) and within slightly thicker cirrus clouds (Kahn et al. 2009), assessments of vertical information content (Maddy and Barnet, 2008), and precision and accuracy between AIRS and MLS in the upper troposphere (Fetzer et al. 2008). There are also many studies on microwave retrievals of RHI in the upper troposphere that in fact sound through even thicker clouds; the most notable paper of late is by Buehler et al. (2008). The present paper concerns thicker convective clouds as a whole, but it is clear from Fetzer et al. (2008) that MLS has many limitations within these types of clouds, along with AIRS and other instruments. A more nuanced discussion on this must be added to the Introduction and other parts of the manuscript where this subject is raised. Some of the appropriate references are listed below:

Buehler , S. A., M. Kuvatov, V. O. John, M. Milz, B. J. Soden, D. L. Jackson, and J. Notholt (2008), An upper tropospheric humidity data set from operational satellite microwave data, J. Geophys. Res., 113, D14110, doi:10.1029/2007JD009314.

Fetzer , E. J., et al. (2008), Comparison of upper tropospheric water vapor observations from the Microwave Limb Sounder and Atmospheric Infrared Sounder, J. Geophys.

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Res., 113, D22110, doi:10.1029/2008JD010000.

Kahn, B. H., C. K. Liang, A. Eldering, A. Gettelman, Q. Yue, and K. N. Liou (2008), Tropical thin cirrus and relative humidity observed by the Atmospheric Infrared Sounder, Atmos. Chem. Phys., 8, 1501 – 1518.

Kahn , B. H., A. Gettelman, E. J. Fetzer, A. Eldering, and C. K. Liang (2009), Cloudy and clearâĂŘsky relative humidity in the upper troposphere observed by the AâĂŘtrain, J. Geophys. Res., 114, D00H02, doi:10.1029/2009JD011738.

Lamquin, N., C. J. Stubenrauch, and J. Pelon (2008), Upper tropospheric humidity and cirrus geometrical and optical thickness: Relationships inferred from 1 year of collocated AIRS and CALIPSO data, J. Geophys. Res., 113, D00A08, doi:10.1029/2008JD010012.

Maddy, E. S., and C. D. Barnet (2008), Vertical resolution estimates in version 5 of AIRS operational retrievals, IEEE Trans. Geosci. Remote Sens., 46, 2375 – 2384, doi:10.1109/TGRS.2008.917498.

Susskind , J., C. Barnet, J. Blaisdell, L. Iredell, F. Keita, L. Kouvaris, G. Molnar, and M. Chahine (2006), Accuracy of geophysical parameters derived from Atmospheric Infrared Sounder/Advanced Microwave Sounding Unit as a function of fractional cloud cover, J. Geophys. Res., 111, D09S17, doi:10.1029/2005JD006272.

In Section 2.1, Odin-SMR is discussed. However, it is not clear how the RHI retrievals are obtained. Are temperature and water vapor retrieved individually and then combined to calculate RHI, or is RHI directly obtained from the observations? Although this explanation may be contained in other references, some basic description, at a minimum, is required here. How are the precision estimates of 17% and 65% for RHI and IWC, respectively, determined?

In Section 2.2.2, the alternative CloudSat retrieval is briefly mentioned. There are absolutely no details here. Is this the same retrieval algorithm for Odin-SMR? Please

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describe the approach taken with CloudSat IWC. Furthermore, no mention of a rudimentary comparison to the standard CWC product is discussed. Why not? Wouldn't it be illuminating for the purposes of this research to have some type of basic comparison?

At the start of Section 3, it is mentioned that IWC obtained from CloudSat is vertically averaged to the same 'gridding' as IWC from Odin-SMR. How is this done? Are there averaging kernels or weighting functions associated with Odin-SMR, or is each point in all of the CloudSat bins weighted the same? This is an important point as any vertical heterogeneity in cloud layering could end up becoming a source of error if the vertical weighting of Odin-SMR is anything other than a top-hat.

I found that some of the discussion in Section 3.2 to be unclear and misleading. For instance, lines 12-14 on p. 11725 seem to imply that ice particles are the primary source of moistening in the UT and that is definitely not the case. Most of it comes from detrainment of moist air in convection. Refer to the following paper:

John, V. O., and B. J. Soden (2006), Does convectively-detrained cloud ice enhance water vapor feedback?, Geophys. Res. Lett., 33, L20701, doi:10.1029/2006GL027260.

On line 15, I am not sure what the authors mean by 'strength of relatively local convection.' Is there a particular set of references the authors can clarify this statement with? This entire paragraph should be reworked (or expanded greatly, or eliminated) as the last two sentences (lines 20-22) are not clear from the figure. What are we supposed to see in Fig. 1, and how is that related to the points being made above?

On lines 26 and 27, how were the CloudSat data averaged? Were the reflectivities averaged then the IWC retrieval was performed, or was the original resolution of reflectivity retained then IWC retrieved? The same question applies to both the vertical and horizontal dimensions. These are points probably better made in Section 2.2.

On p. 11726 at line 6, it is not clear what the authors are trying to say. The 'stages' or

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magnitude of 'strength' of convection may have many definitions. Which one is being used here as a standard? This issue arises again at p. 11730, lines 7-8.

Same page, line 16, need a definition for 'thin cloud'. The CALIPSO lidar is the standard for thin cirrus, but it appears the authors are referring to slightly thicker cirrus, for instance the kind discussed in Kahn et al. (2009).

How precisely is the temporal matching of the model data to the observational data? Is it done for the exact same time periods? That may be mentioned in the paper but it is hard to find it. Since model output isn't necessarily continuous and is often done in 3 or 6 hour intervals, did the authors interpolate in time? Did they sample the same ground track pattern as the satellite observations? More detail is needed here.

In the conclusions, should mention the sampling limitations of MLS RHI in the presence of thicker convective clouds (Fetzer et al. 2008). Also, in the last paragraph, the shortcomings of the model output are stressed, but to be fair, the limitations of the satellite data themselves are significant and relevant to the conclusions. For instance, models will produce values of IWC and RHI everywhere, but the satellite observations will only report certain ranges of those values, and sub-ranges may contain biases of various magnitudes, changes in sign, and variable precision. How does this impact the conclusions reached in this work?

Editing, grammatical and suggested figure changes:

p. 11712, l10: 'is on the order'

p. 11714, l27: 'scope of these studies'

p. 11716, l22: elaborate on what is meant by 'weather information'

p. 11717, l4: What is 2x45 km^2? Doesn't make sense.

p. 11723, l23: 'that the humidity distribution'

p. 11724, l16: 'That is say,' needs to be rephrased.

p. 11726, l20: ‘of tho’ should be ‘of two’?

p. 11726, l21: ‘show realistic’ and ‘what is expected’

p. 11727, l11: ‘meaningful’

p. 11727, l24: ‘respective’

p. 11728, l4: ‘more accurately.’

p. 11728, l18: ‘combinations’

p. 11730, l1: ‘relatively high’

p. 11731, l9: ‘than that modeled.’

p. 11732, l13: ‘The models also show’

p. 11732, l17: how about ‘Although most mean IWC values from models are within. . .’

p. 11732, l22: not sure what is meant by ‘with the measured.’

p. 11733, l1: ‘the diurnal’

p. 11733, l12: ‘ECHAM is poorest’

p. 11733, l13: quantify ‘some’

p. 11733, l14: ‘the also too early phase’ is unclear

Figs. 2 and 5: Are the y-axes ‘cumulative distributions’?

Fig. 6: Hard to see the two different widths of the vertical bars. What about using two different colors?

Fig. 7: Not sure why there is no mean IWC for the N. Pac region (was explained well in the text) but there are values for its amplitude and phase. This isn’t entirely clear. Please explain.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 11711, 2010.

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