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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diurnal variation of tropospheric relative humidity in tropical region" *by* Isaac Moradi et al.

Anonymous Referee #1

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General comments

The authors used brightness temperature data from 6 channels in the 183 GHz water vapour absorption line, measured with the SAPHIR instrument on Megha-Tropiques, to infer the diurnal cycle of layer averaged relative humidity in the tropics, up to 25° from the equator. The 6 channels correspond to 6 thick layers in different altitudes from the bottom of the free troposphere to the upper troposphere. The use of microwave (MW) data from this instrument is a progress relative to earlier attempts using infrared (IR) data or data from polar orbiting satellites, because MW is much less sensitive to clouds and the low inclination angle of Megha-Tropiques allows to sample the tropics much more often than just twice daily.

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Discussion paper

It turns out that the diurnal cycle is weak over most regions, but with exceptions where the amplitude can be very large (as the pentagons curve in Figure 13 exemplifies). The daily peak occurs mostly in the early morning, but there are places where it occurs in the afternoon as well. Interestingly, there seems to be no correlation between the daily amplitude and the general level of relative humidity.

To my opinion, the paper is a useful and interesting contribution to ACP. The following lists minor points. It should be no problem to address them.

Minor comments

Lines 20-34: The motivation for the study is centred around the many roles of water vapour (WV) in the climate system. However, the study is about the diurnal cycle of free tropospheric WV. How are the subtleties of WV's diurnal cycle related to climate issues, is there a connection at all? To my opinion it is important to study the diurnal cycle since it exists. I believe, however, that the diurnal cycle is not important for the climate issues and thus the motivation may not be appropriate. Perhaps the authors can provide arguments for such a link.

Lines 74/75: Unclear sentence. Which T change is meant? Do you mean that the diurnal RH variation is correlated with the diurnal T-variation? A reference for this statemant would be fine.

Lines 85-87: On first reading I had the impression, motivated by the $x \pm y$ error bar style, that the channels get broader and broader from ch. 1 to ch. 6. But this is probably wrong. I believe now that 183 ± 11 GHz is not a 22 GHz wide channel, but a channel which detects radiation at 172 and 194 GHz. Probably this is meant with "double pass band". Could you please clarify this?

Line 89: check whether 6 km is correct; appears too low.

Lines 90/91: The variation of the Jacobian's peaks with moisture content of the atmosphere is a problem in the IR, too. Is the problem particularly strong in the MW region?

Eq. 1: Please specify the meaning of upper and lower indices (probably channel and swath position). Note that it is mathematically incorrect to have lower indices i on the rhs of the equation, but not on the lhs. Did you forget a sum sign?

Line 124: check use of "upper" and "lower". Do these words refer to the channel number or to the peak altitude of the respective Jacobian?

Lines 127-143: I do not really understand what you describe here. First, how the channels with high peak altitude can be influenced by the surface. Regarding figure 1, the Jacobians of chs. 1 and 2 should be close to zero at the ground. Or is this contamination from high mountains? Second, why there is a cut-off at both low and high temperatures? How does the surface feign a Tb below 230 K for instance? Additionally, it would help, if the figure would not only show data, that are NOT affected by the surface, but also those data, that are affected. Does the warning for mountainous terrain not imply that data over land are generally bad, since there is mountainous terrain everywhere on land in the tropics? Can we only trust the data over ocean?

Eq. 4: Where do the weights come from and how are they determined?

Line 173: Whether RH is expressed as RH_I or RH_L is independent of whether an ice phase exists or not. The justification for preferring RH_I is unnecessary.

Lines 223-242: The authors estimate errors due to insufficient temporal sampling of polar orbiting satellites here. It seems, that these errors are generally small, both at single locations (Figures 8 and 9) and on average. One should note that it is hardly possible to measure RH better than to about 10% in RH. Compared to this typical error margin, 2-4% difference is little. Also when I compare these differences with the diurnal amplitude (Figure 10) or with the difference of measurements vs. Fourier fit (Figure 12),

it seems that they can almost be neglected. Please comment on this.

Line 331: I suggest to write "distribution of layer averaged RH". It is important here to distinguish between the local RH (which is usually understood under the term RH) and the layer averaged, non-local, RH. Since these data are already layer averaged, extreme values are largely smoothed away. Distributions based on local data would be much broader then what is shown in Figure 14.

Lines 333-335: There is no saturation pressure with respect to ice above zero (Celsius). How did you calculate the RHI for these lower channels? And does the sentence "we use RH over ice ..." not contradict the line types in Figure 14 (dashed and solid)?

Figure 14: I understand ice supersaturation in the figure, but it looks as if there were water supersaturation as well. Please check and if there is water supersaturation try to explain.

Line 366: what is the transformation method?

Line 375: which change in air temperature is meant?

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