1 Reviewer #1

We sincerely appreciate the reviewer for providing very helpful and constructive comments on the paper. We basically agree with all the comments and will consider them when revising the manuscript. In order to make benefit of the peer-review interactive discussion, we are providing our answers to the reviewer's comments as well as the changes that we will make in the revised version of the manuscript.

`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 issu23es, 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.`

As pointed out by the reviewer, we realized that we have not explicitly discussed the importance of diurnal cycle of humidity in climate. We added the following sentences to clarify this:

> Diurnal cycles in temperature and moisture drive diurnal variations in temperature, precipitation, and convective activities (Chung et al., 2007), therefore, are expected to interact significantly with, for example, changes in global mean humidity or temperature. However, current climate and numerical weather prediction models do not adequately simulate the diurnal variation of tropospheric humidity (Chung et al., 2013), a failing that is very likely to lead to inaccuracies in their simulations. As models are improved, accurate observations of diurnal cycles of humidity will be crucial in verifying the validity of simulations.

`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 statement would be fine.`

We amended the text as follows to better reflect the meaning:

> As stated before, relative humidity is the ratio of water vapor pressure to the saturated vapor pressure. The water vapor pressure depends mainly on the water vapor content of the atmosphere, but the saturated vapor pressure depends on the air temperature. Therefore, the diurnal variation of RH does not necessary indicate change in the water vapor content of the atmosphere, because it is affected by both diurnal variation of water vapor and air temperature. For instance, change in the amount of lower tropospheric water vapor over deserts is very small during day, but RH can significantly change because of change in air temperature. Therefore, it is more desired to analyze the diurnal variation of absolute humidity parameters.

`Lines 85-87: On first reading I had the impression, motivated by the x±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?`

Yes, as the reviewer stated, the x±y is conventionally used to specify the double passbands not the error bars

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

We thanks the reviewer for noting this. The correct number is about 10 km. We have amended the text accordingly.

`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?`

Yes that is correct. The peak altitude of both IR and MW Jacobians change with the humidity. We have amended the text to reflect this.

`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?`

We removed the index i as it was not necessary and have explained that ch stands for the channels. The a and b coefficients are calculated separately for each channel so no summation is required (we may have misunderstood the comment on adding 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?

Many thanks to the reviewer for noting the error. We have reworded the sentence as follows:

> We used the same thresholds proposed by Moradi et al., 2015, to screen-out the clouds using the differences between Tb's of an upper channel (Tb2, channel 2 operating at 183±1.10 GHz) and a lower channel (Tb5, channel 5 operating at 183±6.8 GHz).

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?

The reviewer is correct and the measurements for high peaking channels are normally not affected by the surface. However, it is very likely that over high mountains the measurements for those channels are also affected by the surface. We have already explained in the text that the results over mountains should be used with caution. The Jacobians are plotted for all the profiles regardless of whether there is any surface effect or not. Overall, the surface emissivity for water vapor channels is about 1 over land and 0.5-0.7 over ocean. Thus, the brightness temperatures affected by the surface are high over land and low over ocean. Therefore, we need a filter that removes both high and low brightness temperatures. In the case of mountains, the surface emissivity is close to one but since the land surface temperature is low, the affected brightness temperatures may not be rejected by the surface filter.

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

We amended the text as follows to explain how the weights are calculated:

> The wights are calculated as $\frac{1}{\sigma}$, where σ is the standard deviation of all the measurements within each individual bin.

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

We rewrote the whole paragraph as follows to explicitly state that we mean saturated vapor pressure above and below freezing points:

> We used a subset of the ARM radiosonde data to calculate the empirical coefficients (a and b) for Equation 1. Since the saturated vapor pressure can be calculated with respect to either liquid (temperatures above the freezing point of water) or ice phase (temperatures below the freezing point of water), the empirical coefficients can be defined the same way with respect to saturated vapor pressure over either liquid or ice. We use RH_I to refer to RH with respect to ice and RH_L for RH over liquid. It is expected that at least in the middle and upper troposphere, the air temperature is generally below the freezing point. Additionally, for the lower channels the saturated vapor pressure expressions for ice and liquid approach each other. Therefore, in most cases we only present the results for the ice phase (RH_I) and the results for the liquid phase (RH_L) are provided in supplementary materials.

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.

We agree with the reviewer that over most regions these differences can be counted as negligible. We added the following sentence to clarify this:

> Overall, over most regions, these differences are small and can be considered negligible compared to the methodological errors.

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.

Done!

`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)?`

There are in fact two independent equations for saturated vapor pressure over ice and liquid that require air temperature as input. The equation for the saturation over liquid (ice) is just more accurate for temperatures above (below) the freezing point. We already amended the text to state that we mean saturation vapor pressure for the temperatures above/below freezing point (because of the super cold water) rather than the actual liquid/ice phase. We also amended the text as follow to avoid any confusion:

When necessary, we use RH over ice for channels 1-4, and over water for channels 5-6 to discuss the results

`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.`

Thanks to the reviewer for reminding this. Yes, a small percentage of data show supersaturation even over liquid (up to 110%) that can be explained by the methodological uncertainty as well as some possible errors in the data (all together probably around 15% error). Please see the section on "error estimates" for more information on the possible sources of errors. We also added the following sentence in the revised version to clarify this:

A small percentage of the data show supersaturation over liquid (up to 110 %) for Channel 6. This can be explained by the methodological error as well as error in the satellite observations. We estimate that the error introduced by difference sources (see Section 4.6) can be up to 15%.

`Line 366: what is the transformation method?`

We amended the text as

the Tb to RH transformation method

`Line 375: which change in air temperature is meant?`

We amended the text as

the diurnal variation of air temperature