3 Reviewer #3

We truly appreciate the reviewer for carefully reading the entire manuscript and making very constructive recommendations to improve the study as well as the manuscript. We have adopted all the recommendations in the revised version and have also provided point-to-point responses in the following.

This study uses microwave measurements from SAPHIR onboard the Megha- Tropiques satellite to investigate the diurnal variation of water vapor in the tropics. In this study, the limb effect-corrected observed radiances were transformed into layer- averaged relative humidity and then partitioned into 24 bins of local observation time. The authors determined the phase and amplitude of diurnal variation by fitting the Fourier series to the binned data, and showed a large inhomogeneity in the diurnal variation of tropospheric relative humidity in the tropics. Although there are some is- sues that the authors need to clarify, the results presented in this study appear to help improve our understanding of the diurnal variation of water vapor.

Many thanks for the positive feedback!

1. Motivation of the study: Although the authors argues that the diurnal variation of water vapor has not been investigated due to the lack of observations, the argument is not true given the previous studies addressed in the manuscript. Also, the diurnal variation presented in the manuscript is very weak over most regions of the tropics, raising a question on the need of investigation on the diurnal variation of water vapor. Moreover, it is unclear in what ways the diurnal variation of water vapor is important.

We appreciate the reviewer for this valid comment. We have revised the introduction to better clarify the novelty of the current study. We have also more properly cited the previous publications (mainly conducted using the IR measurements) in the revised version. However, since these changes are major and are introduced in several different places, we have directly incorporated them in the revised version and have not copied the revised text here.

2. The authors argue that their results are superior to previous studies based on IR measurements or multi-instrument microwave measurements. However, it doesn't seem that the differences from the previous studies are discussed comprehensively in the manuscript. Therefore, it is not certain whether this study advances our understanding of the diurnal variation of water vapor

in the tropics.

In the revised version, in addition to citing previous publications, we have also better compared our results with the previous studies.

3. The manuscript documents the peak time and amplitude of the diurnal variation in the tropics, but does not provide reasonable physical mechanisms responsible for the spatial and altitude discrepancies in the diurnal variation. Also, it is unclear why some parts of the tropics have an early morning maximum of the tropospheric relative humidity.

We have now provided more explanation on the physical mechanisms behind the diurnal cycle of RH. We have also provided explanation for the early morning peak time which is consistent with previous studies and is correlated with the early morning peak time of convergence zones (e.g., Haffke et al 2015).

4. Given the seasonal migration of ITCZ, the amplitude and peak time of the diurnal variation for a given season might be different from that for the annual mean. However, this aspect is not investigated in the manuscript.

We agree with the reviewer regarding the impact of ITCZ on the diurnal cycle of RH, therefore in addition to the mean annual values, we have also included the diurnal amplitude and peak time for the months of December/January as well as June/July in the revised version. As the reviewer pointed out, the results change with the season due to shift in ITCZ.

Specific points:

1. L1-2: The argument that the diurnal variations of water vapor have not been investigated in the past due to the lack of observation is not correct.

As we mentioned in response to the reviewer's general comments, we rewrote the introduction to better clarify the new aspects of our study.

2. L12-13: Is this a new finding?

We slightly changed the sentence to emphasize that this is not a new finding:

> The results showed that the wet regions are normally associated with convective regions, and the dry regions with the subsidence regions which are consistent with the previous studies.

3. L13: high (surface) pressure?

Changed to "the subsidence regions"

4. L26: The statement that water vapor in the free troposphere contributes to the water vapor feedback through latent heat processes is confusing, because the water vapor feedback is associated with radiative processes. Please clarify.

We changed it to "radiative processes" and also rewrote the introduction for a better clarification.

5. L30-34: These sentences describe the water vapor feedback. What is the difference from the lines 24-28? The first paragraph of introduction needs to be reorganized.

We have revised and reorganized the introduction as suggested by the reviewer.

6. L40: Do the authors mean Kottayil et al. (2013)?

Many thanks to the reviewer for noting the error. It is now corrected.

7. L39-44: The transition is not logical. It seems that sentences are missing between the two sentences.

We added a sentence to provide a logical transition as follows:

> One exception is Kottayil et al. (2013) that used multi-instrument microwave measurements from five polar-orbiting satellites to investigate the diurnal variation of brightness temperature (Tb) over the globe. However, other issues are involved when data from polar-orbiting instruments are utilized. First, polar-orbiting satellites only overpass each location twice a day, thus even a constellation of five satellites do not properly represent the diurnal variation of RH (e.g. see Figure 1 in Kottayil et al. (2013) for the temporal coverage in different years).

8. L46: Kottayil et al. (2013)?

Changed the citation to Kottayil et al. (2013)

9. L67: the lack of ‘‘adequate’’ observations?

We amended the sentence and now it reads "lack of adequate observations.

10. L84: six instead several?

Done!

11. L89: Does ‘‘upper’’ mean channel 1?

Yes! The text was amended as follows to reflect this:

> Figure 1 shows the weighting functions for the SAPHIR channels which are roughly sensitive to upper (channel 1 peaking around 10 km) to lower troposphere (channel 6 peaking around 2 km).

12. L89-90: Please consider adding additional y-axis (altitude) to the right in Figure 1. Done!

13. L105: Does ‘‘i’’ denote the earth incidence angle?
The index "i" was unnecessary. We have removed it in the revised version.

14. L115-116: However, the radiative transfer calculations are used to derive the empirical coefficients in Eq. 1 and to determine the thresholds for excluding surface affected radiances. Therefore, the phrases ‘‘to avoid any possible errors due to the radiative transfer calculations’’ need to be changed.
We amended that as

> Since the SAPHIR data do not suffer from scan asymmetry, we preferably used the satellite data to develop the limb-correction technique.

15. L124: Do ‘‘upper’’ and ‘‘lower’’ here have the same meaning as in the line 89?
The upper and lower had been mistakenly switched in L124. Now it reads

> ... 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).

16. L158: Tian et al. (2004)
Done

17. L173-174: Please specify the channels.
Done

18. L197-198: redundant (lines 194-195)
Removed the redundancy. Now it reads as

> As shown, on average, 100 to 300 observations are retained for each bin per hour.

19. L205: high (surface) pressures?
Changed to "subsidence regions"

20. L212: water vapor or moisture instead of humidity?
Changed to water vapor.

21. L228: Figure 8 22. L229: Figure 9

Many thanks to the reviewer for noting the errors. Both errors are fixed.

23. L236-237: redundant (lines 234-236)
Removed 236-237.

24. L238-242: The errors are not significant because the accuracy of SAPHIR measurements is roughly 5% in RH space (line 73). In that case, polar-orbiting satellite observations are sufficient to determine the daily mean. Please discuss in the paper.

Yes, we also believe that polar-orbiting satellites may be used to derive the mean tropospheric humidity to some extent but not the amplitude and peak time. We also need to mention that there is probably a small systematic error in the SAPHIR data but when looking at the peak and amplitude the systematic error is not important because it is canceled out when we take the differences. The random error is also mostly canceled out so the actual error is really very small and negligible. Therefore, we added the following statement to clarify this:

> These results show that measurements from polar-orbiting satellites may be used to derive the mean tropospheric humidity in the tropical region to some extent. However, polar-orbiting satellites may not provide a good picture of peak time and amplitude, because these parameters show a large spatial inhomogeneity and obviously depend on the satellite overpass time.

25. L292: Consider replacing South East Asia by the maritime continent (also in Table 2).
Done

26. L317: Figure 13 does not indicate the early morning maximum/afternoon minimum of RH over the South Atlantic (cf. line 320).

Many thanks to the reviewer for carefully checking the results. We realized that it is difficult to directly interpret diurnal variations presented in Figure 13. We replotted the RH values by either removing the mean RH

from the diurnal cycle or scaling the RH values between a minimum and maximum. It turned out that the latter, Figure 2, better indicates the relative diurnal cycle of RH over all regions. Figure 2 is also included in the revised version of the manuscript. We agree with the comment and accordingly the text has been revised based on results presented in both Figure 13 and the new figure to better explain the diurnal variation of RH.

27. L319: Figure 13 shows the afternoon minimum of RH over Amazon and South East Asia.

We agree with the reviewer. As mentioned, we are providing a new figure, Figure 2, that better shows the diurnal cycle of RH over all the regions.

28. L334-335: Please clarify.

We amended the sentence as follows for a better clarification:

> As shown the distributions of RH with respect to ice and water are similar in the lower troposphere (channels 5 and 6). This is because the relative humidity values calculated using Equation 1 are nearly the same over ice and liquid for the expected range of brightness temperatures in lower troposphere. This is an indication that the transition from ice to liquid is performed smoothly.

29. L345: The distribution is different between the South Atlantic (right-skewed?) and South East Asia (left-skewed?) in Figure 14. Please clarify and reorganize.

We thank the reviewer for noting the error. We amended it as follows:

> The distribution is left-skewed (the left tail is longer) for Amazon and the Maritime Continent, and right-skewed over the rest of the selected regions.

30. L368: Do the authors argue that microwave radiances are significantly affected by thin clouds? If so, what is the advantage of microwave measurements over the infrared observations?

We meant "thin" from a microwave instrument perspective which is still generally a thick cloud. We removed the word thin to avoid any confusion.

31. L378-383: It is difficult to figure out how the diurnal variation of tropospheric humidity is related to global warming. Please discuss in the manuscript.

We completely rewrote that paragraph to better explain the relation between the observations and climate simulations.

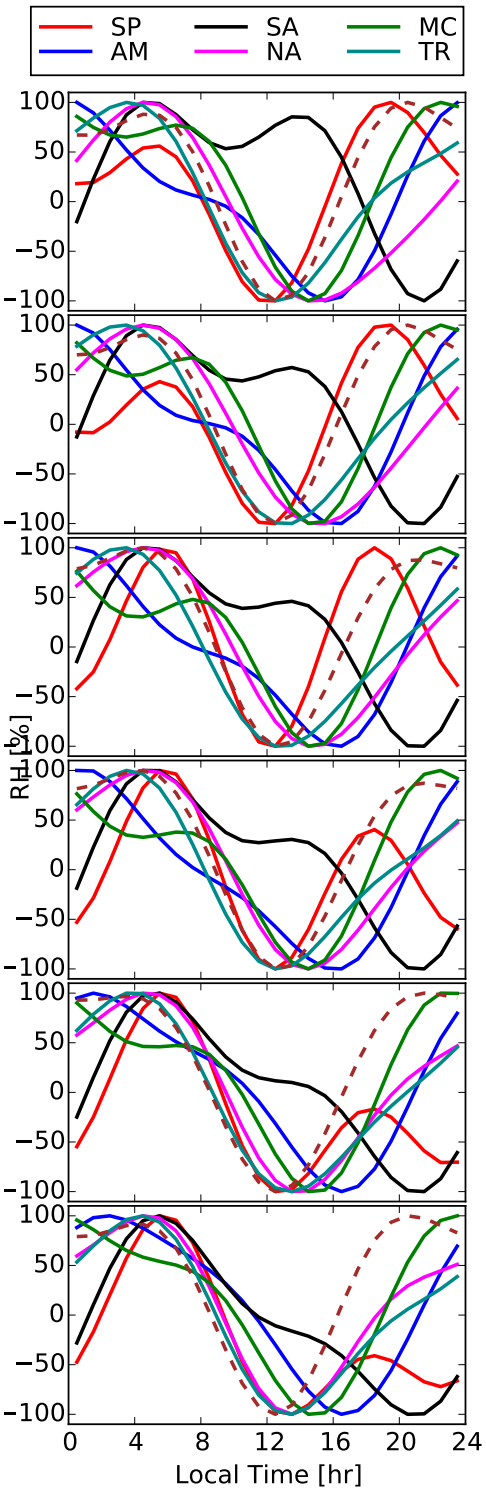

Figure 2: Diurnal variation of RH scaled between -100 and 100 individually for each region.

32. L395: high (surface) pressure?
We changed that to subsidence regions.

33. L396: Please clarify "significant" regions.
We changed it to several regions.

34. L488: Coauthors are missing.
Coauthors are now included.

35. Figure 2: Please correct typo (y-axis).
Done!

36. Figure 3: Are the histograms independent of latitudes and seasons?
Yes, they are independent of the season and latitude. We have used a large subset of data covering different atmospheric conditions.

37. Figure 4: The range of simulated Tb is 240-280 K in Figure 3. In contrast, the range is narrower here. Why are they different?
In Figure 3, only a small percentage of the data are between 270-280K, which are not shown in Figure 4 because Figure 4 is a density plot.

38. Figures 5-12: Please include tick labels for longitude and latitude in Figures 5-12.
Done!

39. Figures 6-7: Please specify the time period for the mean daily relative humidity.
We have used the data for the period January 2012 to September 2015. This is now clarified in the captions as well as in the text in Section Satellite Data.

40. Figure 13: It seems that the diurnal variation is only evident over regions of high elevation.
The diurnal variation is dominant over high elevations but also the tropical lands such as Africa. However, the diurnal variation, though exist, is very weak over oceans.