Interactive comment on "Annual cycle of water vapour in the lower stratosphere over the Indian Peninsula derived from Cryogenic Frost-point Hygrometer observations" by Maria Emmanuel et al.

Anonymous Referee #1

General comments:
This study presents the water vapor profiles measured by the balloon-borne Cryogenic Frost point Hygrometer (CFH) in the upper troposphere and lower stratosphere (UTLS) over two stations in India during the period from February 2015 to January 2016. Their figures show that the CFH measurements have sufficient quality to discuss the nature of water vapor in the UTLS, in particular, the tape recorder signals observed at the two launching stations are very impressive. However, I think that the current manuscript lacks some essential and key points to understand and interpret the observational results. In my opinion, the required components are 1) employment of the saturation water vapor mixing ratio, 2) understanding a concept of three-dimensional transport in the UTLS, 3) presentation of the value to use the column integrated water vapor amount, and 4) presentation of the value to focus on the upward propagating signal in the water vapor mixing ratio difference between the two launching stations. The specific comments, including above four points, are described below.

Response: First of all, we thank the anonymous referee for the appreciation and valuable comments. We have taken into account all the comments and suggestions in preparing the revised manuscript. The response to each specific comment is given below.

Specific major comments:
1) The atmospheric pressure logarithmically changes with altitude. This is one of the reasons why we usually use the "mixing ratio" for our analysis because of its conservative property in vertical movement of the atmosphere. If one air parcel moves to upward, its air pressure, water vapor pressure, absolute humidity [mg/m^3] which the authors employ in the manuscript, must change, however, the water vapor mixing ratio never change without the occurrence of dehydration or hydration or mixing it with surrounding air mass. Therefore, when we want to discuss the water vapor and the dehydration, in particular in the tropical UTLS, we usually employ the minimum saturation water vapor mixing near the cold point tropopause (CPT), but not temperature at the CPT, to compare the observed water vapor mixing ratio. For example, here we consider two air parcels (parcel_1 and parcel_2), one has the temperature (T_1) and pressure (p_1) at altitude (z_1), and another has (T_2) and (p_2) at (z_2), and we assume parcel_1 locates higher altitude than parcel_2 (z_1 > z_2). If T_1 and T_2 are the same value, the two produce the same saturation water vapor mixing ratios (p_wv1 and p_wv2). However, the two situations produce different saturation water vapor mixing ratios (SMR_1 and SMR_2) because they are obtained from SMR_1 = p_wv1/p_1 and SMR_2 = p_wv2/p_2 under the condition of p_1 < p_2. This fact imposes the employment of the minimum SMR (SMRmin) near the CPT (the altitude where produces the SMRmin does not always agree with the CPT) on the current manuscript to discuss dehydration or hydration, in particular, in the following parts. Figure 1, Figure 3 (Could you include symbols showing the mean SMRmin at the altitude where they produce in the same color scale to water vapor?), Figure 5b, Figure 6a, Discussions in Page5 Line27-Page6 Line2, Page7 Line20-30, the first paragraph in Page8, and Page11 Line21-24.

Response: The suggestion is well taken. The SMRmin values are estimated and is included in Figure 1. The mean annual variation of SMRmin altitude is shown in Figure 3 along with the monthly variation of CPT altitude. The annual variation of SMRmin altitude and SMRmin from CFH observations are added in Figure 5. The mean annual cycle of SMRmin value is added in Figure 6a along with the CPT temperature. The SMRmin altitude occurs within 500 m below the CPT altitude and the difference between these two altitudes is maximum in the winter months. The discussions are made in the revised manuscript accordingly.

2) Though the authors cite some articles (e.g., Randel and Park, 2006; Park et al., 2007) addressing the Asian summer monsoon (ASM), a modern concept of the ASM is not sufficiently reflected in the interpretation of the results obtained from the current study. To grasp the concept, I think Figure 14 of Park et al. (2009) and Ploeger et al. (2017) may be helpful. They present the pictures involved in the ASM that consists rapid vertical transport by convections, horizontal transport by anticyclonic circulation at the UTLS, and slow ascent in the tropical stratosphere by the BDC. After considering those transport mechanisms involved in the ASM, I basically agree the interpretation that the water-rich air mass at higher altitude than that of the CPT observed over Hyderabad during ASM season, which might be transported from the region over Bay of Bengal (BoB) after it is hydrated by convections. It likely occurs, I think. But, if so, I think the infrared data around BoB (as well as other upstream regions of the anticyclonic circulation) should be additionally shown together with the horizontal wind field at just above the CPT altitude.

Response: As suggested, spatial distribution of occurrence of deep convection (using thermal infrared data), horizontal wind field (using ERA-Interim reanalysis data) and potential path ways of air mass (using HYSPLIT transport model back trajectories) are generated for different seasons and is shown as new figures (Figures 6 and 7) in the revised version). Vertical wind already in the manuscript. As suggested, three-dimensional transport and hydration of LS during ASM period are discussed.

3) I could not find the reasonable reason why the authors employ the column integrated water vapor in the LS (IWV_LS) in the current manuscript. The IWV_LS is mainly discussed in the text in Page7 Line7-20 and the discussion about its difference between the two launching site is connected to local processes. I think it could not provide scientific discussions unless the concept of three-dimensional transport associated with the ASM is accurately introduced as described in the previous comment. On the other hand, in my opinion, if the authors successfully determine some indicator to quantify the hydration amount above the CPT altitude (strictly the SMRmin altitude) caused by local convection and/or ASM (for example, to calculate the vertical integration of the water "increment" from the local SMRmin, etc.) and if the observed water vapor profiles can be quantitatively interpreted in connection with hydration processes using the indicator (for example, to show the relationship between the amount of the indicator and the ice water content in the convective overshooting clouds, etc.), such study may provide an new insight to understand the role of ASM on the stratospheric water vapor.

Response: The concept of three-dimensional transport is discussed in the revised manuscript. The spatial variation of deep convection, horizontal and vertical transport and their effect on the Lower Stratosphere (LS) water vapour are discussed in the revised manuscript. The seasonal variability of LS water vapour (Figure 2) shows large variability in the altitude region CPT-21 km compared to the altitude region 21-25 km. Hence, Figure 4 (annual variation of IWV_{LS}) is modified in the revised manuscript. The CPT to 25 km region (LS) was separated into two regimes, viz CPT-21 km (LS1) & 21-25 km (LS2). IWV in lower regime, LS1 is influenced directly by local/regional tropospheric dynamics and contributes about 50-70 % of the IWV_{LS}. Hence, the variability of IWV (from the annual mean) in this regime can be used as an indicator for quantifying/understanding the amount of water vapour entered in to the lower stratosphere from convective disturbances/monsoon dynamics. The integrated water vapour in the altitude region 21-25 km, is approximately 30-40 % of the total IWV_{LS} shows similar variability over both the stations are controlled mainly by large scale dynamics.

4) The authors focus on the upward propagating signal in the water vapor mixing ratio difference between the two launching stations in Figure 8. But I could not identify such propagating signal in the

figure. On the other hand, Figure 9, indeed, clearly shows such upward propagating signal. This signal, however, can be simply produced by larger and smaller amplitudes of the tape recorder over Trivandrum and Hyderabad, respectively. Such interpretation is likely reasonable to me because Trivandrum locates nearer the centre of the tropical pipe in the stratosphere than Hyderabad. How do you think about this opinion? You can check it by making some figures which show the meridional (latitude-altitude cross-section) distribution of water vapor mixing ratio over a meridian line across India (for example 80degE) for every month by using MLS data (like as Figure 1 in Ploeger et al., 2017).

Response: As pointed by the reviewer, the difference between the two stations and its propagation is not clearly visible from the CFH observations; This could be mainly due to the local effects such the day-to-day variability in CPT temperature and convection and/or due to the usage of lesser number of profiles in each month (1or 2 profiles). Also note that CFH provides better vertical resolution. The signature of upward propagation in Figure 8 could be improved if smoothed for atleast 1 km. But, it is equal to degrading the vertical resolution of CFH observations. In the revised version we have applied a 3-point smoothing for better representation and the upward propagation is marked with an arrow mark.
As rightly pointed out, the higher ascent rate at Trivandrum is expected as it is an equatorial station. Hyderabad being an off-equatorial station, the vertical ascent is relatively small compared Trivandrum. As suggested, meridional (latitude-altitude cross-section) distribution of water vapor mixing ratio over a meridian line across India (75- 80 °E) is generated for every month using MLS data, for examining the latitudinal differences in amplitude of signals and is included as a Figure in the revised manuscript (Figure 12 in revised version). The latitudinal differences in the water vapour signal is discussed in the revised text in section 3.3.

References Park, M., W. J. Randel, L. K. Emmons, and N. J. Livesey (2009), Transport pathways of carbon monoxide in the Asian summer monsoon diagnosed from Model of Ozone and Related Tracers (MOZART), J. Geophys. Res., 114, D08303, doi: 10.1029/2008JD010621.

Ploeger et al., (2017), Quantifying pollution transport from the Asian monsoon anticyclone into the lower stratosphere, Atmos. Chem. Phys., 17, 7055–7066, https://doi.org/10.5194/acp-17-7055-2017.

Response: The references are noted and cited in the revised manuscript.

Once again, we thank the reviewer for the constructive comments

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