Interactive comment on "Annual cycle of watervapour in the lower stratosphere over the IndianPeninsula derived from Cryogenic Frost-point Hygrometer observations" by Maria Emmanuel etal.

Anonymous Referee #2

This paper investigates the processes which control the water vapour budget at the seasonal scale in the tropical UTLS region above two Indian sites through the use of balloon-borne profiles of water vapour. The study is completed by space-borne observations of temperature and water vapour.
I do not think the manuscript adds much to the general knowledge of the processes explaining the seasonal and interannual control of the UTLS water vapour variations (connection with the tropopause temperature, tape recorder). It is not also a case study of dehydration or hydration effects. Although to me more investigations using statistical analysis from back trajectory calculations (from the position of the balloon profiles) matching some locations of convective systems (through OLR) would have completed the study, the paper is nice and interestingly addresses the effects of dynamical processes (convection, horizontal transport in the UTLS and BDC in the stratosphere) in the specific Indian subcontinent impacted by strong seasonal dynamical, microphysical and chemical variations associated with the monsoon in summer. Also, one of the most positive point of the presented work is that it definitely promotes the precious need for regular in situ water vapour observations in the tropical UTLS and stratosphere for all seasons in Asia, with India as a highly valuable location for such investigations. The balloon records presented here would be helpful to decrease the uncertainty in model convective parameterization and in currently assimilated in the reanalysis modelling systems which typically show some errors in UTLS water vapour due to the limitations of observational data in the tropics, especially for the Asian monsoon season.
The idea to use water vapour observations at stratospheric altitudes from both Indian sites as a proxy for the ascent rate of BDC is interesting given the possibility of regular balloon launches from these locations that is to be supported by the scientific community.
Also the paper is very nice to read and well concise. I would recommend publication in ACP after the following comments have been addressed.

Response: First of all, we thank the anonymous referee for his appreciation and valuable comments. We have taken into account all the comments and suggestions in preparing the revised manuscript. Three-dimensional concept of water vapour transport in the UTLS regions has been discussed using the occurrence frequency of deep convection from thermal Infrared data, horizontal and vertical transport from wind field and potential path ways of air mass (back trajectory analysis) for different seasons and is shown as new figures (Figures 6 and 7 in the revised version).

The response to each specific comment is given below.

General comments

One point would be to investigate if the features observed on the balloon-borne profiles over both stations are typical or repetitive in this region on a seasonal (and perhaps interannual) scale. The authors interpret the vertical dependence of WVMR (especially in fig.2 and chapter 3.1) through general concepts of transport. Have they attempted to verify this statements using some satellite data (MLS/Aura mainly)? For instance, the explanation dealing with synoptic variabilities for the minimum of WVMR in summer around 21 km could be checked from seasonal variations inferred from MLS over the same period (MLS are anyway used in this study to investigate differences between both sites in

Fig.9). In other words, one would have appreciated a better inclusion of satellite observations in the interpretation of the vertical profiles on Fig. 1 and 2.

Response: The features observed on the balloon-borne profiles in Figure 1 over both the stations are typical in the region. The seasonal mean water vapour profiles using MLS data also shows almost similar seasonal patterns to that obtained using CFH observations. However, MLS profiles appear smoother than the CFH profiles partly due to lesser vertical resolution and partly due to greater number of profiles. The MLS derived seasonal mean profiles for the year 2015 is added in Figure 2 of the revised version.

I think the manuscript should better address the possibility of local effects to explain differences observed from the CFH balloon records over the Hyderabad and Trivandrum locations. The authors describe the seasonal difference on the WVMR profiles between Hyderabad and Trivandrum (through Fig.8). The problem is that I do not see the features described in chapter 3.3 (especially, I cannot verify the statement that the WVMR just above the CPT altitude, around 17-18 km, is relatively high over Hyderabad during summer monsoon (JJAS) and winter seasons (DJF) and high over Trivandrum during pre-monsoon (MAM) and post-monsoon (ON) seasons, also from Fig.3). The propagation of the water vapour amount difference similar to a tape recorder shape is also not obvious at all to me. However, features are more apparent when MLS WVMR is used to highlight the differences (Fig.9). To me, the different features observed from CFH and MLS in terms of WVMR may reflect significant local effects controlling the water vapour budget on both sites whereas the use of a 5_x10_ grid tends to smooth out the effects. I do not think the differences to be caused by the measurement quality because as pointed out by the authors their amplitude is higher than instrumental uncertainties. Could local effects be due to local convection impacting the water vapour budget? However, something striking is that the "noisy" differences are visible up to 25 km and not only near CPT whatever the season (not only in convective seasons). Could this be due to long-range transport of hydrated or dehydrated air masses? What do the authors think about this? I would recommend the authors to clarify, simplify or remove this part (P9 lines 16-30 and/or Fig.8). Same remark for the Summary/conclusion part.

Response: As pointed out, the difference in water vapour amount between the two stations and its propagation is not clearly visible in 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). In the MLS WVMR (Figure 9) the difference is clearly seen. Figure 8 could be improved if profiles are 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 to the water vapour difference profiles for better representation. The upward propagation is clearer now and marked with an arrow mark. The discussion in the manuscript is also modified.

In order to address the local effects on water vapour distribution in LS, 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, CPT- 21 km region which contributes about 60-70 % of the IWV_{LS} indicates the direct effect of local/regional dynamics. But, the integrated water vapour in the LS2 region does not show much variability and have no direct association with regional/local dynamics. Hence, we feel that the noisy differences upto 25 km could not be attributed completely to the deep convection. The direct influence of deep convection can be seen up to a maximum altitude of 20-21 km; that is the LS1 region. Above that region, the large-scale dynamics (BDC) mainly controls the transport of water vapour. The influence of methane oxidation and long-range advection also may play a role in this altitude region.

Specific comments:

P2 Line 3: "Due to the large residence time (of more than a year) stratospheric water vapour contributes significantly to the climate forcing instead of a simple response (Wang et al., 2009)" What do the authors mean by "simple response"? Please clarify the end of the sentence.

Response: Simple response refer to instantaneous direct effect But, as it seems to be confusing we have deleted 'instead of a simple response' in the revised manuscript.

P2 line 8: you mean direct injection of water vapour by volcanic eruptions?

Response: Yes.

P3 line 6: you can add 2 other references to span other balloon borne FP hygrometers than the typical NOAA CFH:

Vömel, H., V. Yushkov, S. Khaykin, L. Korshunov, E. Kyrö, and R. Kivi, Intercomparisons of stratospheric Water Vapor Sensors: FLASH-B and NOAA/CMDL Frost-Point Hygrometer, Journal of Atmospheric and Oceanic Technology, Vol.24, 941-952, 2007.

Mélanie Ghysels, Emmanuel D. Riviere, Sergey Khaykin, Clara Stoeffler, Nadir Amarouche, Jean-Pierre Pommereau, Gerhard Held, and Georges Durry, Intercomparison of in situ water vapor balloon-borne measurements from Pico-SDLA H2O and FLASH-B in the tropical UTLS, Atmos. Meas. Tech., 9, 1207-1219, https://doi.org/10.5194/amt-9-1207-2016, 2016.
Berthet, G., J.-B. Renard, M. Ghysels, G. Durry, B. Gaubicher and N. Amarouche, Balloon-borne observations of mid-latitude stratospheric water vapour: comparisons with HALOE and MLS satellite data, J. Atmos. Chem., 70:197-219, doi: 10.1007/10874-013-9264-7, 2013.

Response: Suggested references added in the revised version.

P4 line 7: in the sentence "MLS provides water vapour profiles with a vertical resolution of 2-3 km, 4-6 km and 8 km at 316 hPa to tropopause, tropopause to 1 hPa and at 0.1 hPa with precisions of _15 %, _0.1 ppmv and _0.3 ppmv respectively" it is not clear to which altitude range the "respectively" term corresponds. 316 hPa to tropopause? Tropopause to 1 hPa? At 0.1 hPa? Please clarify.

Response: The sentence is split into two sentences for more clarity.

P3 line 24: It would be appreciable if the authors could discuss the choice of ERA-I reanalysis system keeping in mind the reported differences between reanalysis (ERA-I, MERRA, MERRA2, JRA) in tropical UTLS dynamics or at least provide relevant references quantifying these differences.

Response: Though there are differences between the different reanalysis, all the datasets shows almost similar feature/pattern.There are several Intercomparison between different reanalysis data. SPARC Intercomparison of Middle Atmosphere Climatologies (SPARC, 2002; Randel et al., 2004) have inter compared reanalyses and related data sets in the middle atmosphere. Recently, the Stratosphere–troposphere Processes And their Role in Climate (SPARC) Reanalysis Intercomparison Project (S-RIP) compared the reanalysis data sets using a variety of key diagnostics (Fujiwara et al. 2017). In the revised manuscript, the choice of ERA-I reanalysis is discussed by citing appropriate references.

Figure 1: the minimum and maximum temperature values on the abscissa axis should be 180 K and 240 K respectively so that the reader can better distinguish differences between profiles.

Response: Suggestion incorporated

P5 line 22: I do not see why the authors discuss the peak of _20 ppmv at 15 km in the manuscript which is focused on processes for levels above. Either the origin of the peak is discussed here (link to convection outflow for instance) or please remove it.

Response: Text modified.

P7 lines 22-30: the authors do not comment the reason for the minimum in CPT in summer (July) both in Hyderabad and Trivandrum which values are comparable to the ones in winter. Is it a local effect?

Response: The minimum in CPT in summer (July) was due to the effect local/regional deep convection. This is discussed in the revised text.

P8 lines 1-15: Similar question here: I agree that in general the mean annual pattern in CPT temperature is consistent with the annual pattern of WVMR obtained from CFH observations at CPT (as seen from Fig. 5) but why the CPT seasonal variation derived from COSMIC (Fig. 6) differs from that observed from the balloons (Fig. 5) with no minimum in July in the COSMIC time series? Is it due to local effects or different periods used? Please explain this in the text.

Response: The difference in CPT in July (from balloon flights) is due to the effect of local deep convection. The dehydration effect is seen in the MLS also. This is not evident in the COSMIC profile as it is the mean for five years (2011-2015). The convective effect on CPT and water vapour has inter-annual variability also. Clarity is made in the revised text.

P9 lines 6-7: the authors state: "Though the magnitude of vertical wind in any reanalysis may not be very accurate, the direction (updraft/ downdraft) would be quite reliable". Could you cite some references or studies having addressed this issue?

Response: The vertical wind in ERA-interim reanalysis is calculated from the convergence/ divergence of the archived u and v fields on interpolated pressure levels (eg: Sanz et al., 2007; Wohltmann and Rex, 2008). And, hence the direction of the wind will be quite reliable even though the magnitude may be erroneous. The ERA-interim reanalysis uses a w-equation balance operator in the background constraint (Fisher, 2003). Ploeger et al 2010 from studies of transport in the TTL has found that though the transport characteristics are depending on the vertical velocity, robust patterns of transport in the TTL have greater reliability than in exact numbers. The text in revised version is modified and cited the above said references.

P9 line 27: I think the sentence "difference in WVMR could be due to the difference in pressure also since it is the ratio of vapour pressure to the atmospheric pressure" could be checked easily from pressure profiles or fields above both sites. Why do the authors did not investigate the (not very probable with respect to hydration or dehydration effects impact water vapour absolute concentration) pressure variations?

Response: The difference in WVMR between the stations (Figure 8) is due to difference in actual amount of water vapour itself. Otherwise, it wouldn't have seen in the difference in absolute humidity between the stations (Absolute humidity doesn't have pressure dependence).

Legend of Fig.9: please specify to which months "J" and "D" correspond.

Response: J corresponds to June and D corresponds to December. It is added in the revised Figure caption of the mentioned Figure.

Technical corrections P2 line 8: Please write "the tropopause"

Response: Corrected

P2 line 12: You should define the BDC acronym in the abstract P2 line 14: the LS acronym is already defined in the abstract

Response: Corrected

P2 line27: please define the SST acronym (Sea Surface Temperature)

Response: Corrected

Once again, we thank the reviewer for the constructive comments

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