Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-630-RC2, 2018

Atmospheric Chemistry and Physics Discussions

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 #2

Received and published: 28 September 2018

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 statisti-

Printer-friendly version

Discussion paper

cal analysis from backtrajectory 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 modeling 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.

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. 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.

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. P2 line 8: you mean direct injection of water vapour by

volcanic eruptions? 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 tratospheric 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.

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 $\sim$15 %, $\sim$0.1 ppmv and $\sim$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.

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.

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.

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.

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?

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.

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?

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?

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

Technical corrections

P2 line 8: Please write “the tropopause” P2 line 12: You should define the BDC acronym in the abstract P2 line 14: the LS acronym is already defined in the abstract P2 line27: please define the SST acronym (Sea Surface Temperature)

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-630, 2018.

Printer-friendly version

Discussion paper