Anonymous Referee #1

*Below are the comments from the referee in **black** and replies from the authors in **blue***

General Comments

This manuscript presents the data and analyses of balloon-borne measurements from Northern India and on the southern slope of the Tibetan plateau during two Asian summer monsoon (ASM) seasons. The high vertical resolution profiles of temperature, water vapor, ozone, as well as the cirrus clouds and aerosol information are analyzed together to characterize the region's UTLS thermal and dynamical structure, transport characteristics, in particular the transport of water vapor into the stratosphere and the presence of the Asian tropopause aerosol layer (ATAL). This work is part of a larger project involving the airborne campaign StratoClim. The data and the analyses are well documented in the manuscript. The result contributes important new information to the larger picture of ASM UTLS transport. The work is high quality and fits the scope of ACP well. I have a number of suggestions for improving the manuscript, mostly related to presentations and discussions of the results.

We are grateful to Anonymous Referee #1 for the careful reading and for providing many valuable suggestions, which contribute to improving the manuscript significantly.
We recognize that the points raised by the reviewer about the 3D structure of ASMA are valid, and particularly relevant for assessing the moistening mechanisms of the CLS. This issue is now addressed in the conclusions, with considerations about adiabatic transport from the Tibetan plateau to the southern slopes of the Himalayas and CPT variability based on existing literature. We also recognize that the assessment of the ASM role in moistening the stratosphere is only partly supported by the analysis in this paper, which does not take into account horizontal motion of the air, and consequently this statement was revised in the conclusions.
The abstract was revised in order to better highlight the objectives of this paper, namely to provide an overview of all the balloon measurements performed during our campaigns and to address the broad relevance of this dataset. More targeted studies addressing the question of stratospheric moistening are still ongoing, and the results will be discussed in future publications.
As suggested by the reviewer, the meteorological overview was improved, in particular the seasonal variability section and Figure 2, now showing time-averaged cross sections (including the easterly jet position) and geopotential height fields along with the trajectories. We made the formulation of the proposed UTLS structure more handy by rearranging Sections 4.3-5.2 and introducing the schematics (now Figure 6) earlier in the paper. This includes reducing the use of not well established acronyms (now also summarized in Table 1) and avoiding redundancy in figures (comparison with ECMWF figure removed). The revised manuscript is more concise than the previous version, which was achieved by avoiding repetitions (e.g. campaign specifics) and making the overall discussion more targeted to the objectives of this paper.
In the following, we reply point-by-point to the reviewer's comments, and highlight the corresponding changes made to the manuscript. Note that page and line numbers given in the replies refer to the revised version of the manuscript without tracked-changes.

Major comments and suggestions:

1) Balloon-borne measurements have their strengths and weaknesses. When making interpretation, it is important to recognize the main weakness that the data is approximately one dimensional while the atmosphere in general is described in 3 (spatial) + 1 (time) dimensions. In this specific study, the location of measurements is uniquely situated in the region of steep elevation change.

Associated with the terrain variation, the upper level anticyclone also creates a significant tropopause height variation. How the measurement location is relative to the horizontal structure of the tropopause height, especially the region of the highest tropopause, is very important for the conclusions. This consideration is largely missing in the discussion.

Suggestion: Discuss your results in contrast to the results from previously published work using data from balloon-borne measurements with similar payloads but launched from the Tibetan plateau (Bian et al., 2012). Identify the key differences and their implications to your conclusions in relation to the UTLS structure.

We agree with the reviewer that considering the 3D structure of ASMA, and particularly the "bulging" CPT above the Tibetan plateau, is important for assessing the mechanisms moistening of the CLS. The fact that isentropic transport from Tibetan plateau / below CPT to southern slopes of Himalayas / above CPT might be responsible for (part of) the enhanced H_2O observed in the CLS is an important feature that was missing in our previous discussion. This issue is now discussed in the conclusions section, based on comparison of the average CPT isentropic levels from our datasets with Tibetan plateau soundings from Bian et al. (2012) as well as with considerations about CPT variability based on previous literature (page 14, lines 3-9).
Comparison of our southern-slopes measurements with the simultaneous Lhasa 2016 and Kunming 2017 campaigns of SWOP in the Tibetan plateau region are ongoing (see page 15 lines 1-2) and the results, which will address explicitly the issue of adiabatic transport vs slow ascent and overshooting convection (i.e. "chimney vs blower"), will be discussed in a future publication.

2) When concluding the role of ASM in moistening the stratosphere, it is important to recognize that the time scale changes at the level around the CPT. While the vertical transport up to the CPT is in general within the season, it becomes much slower above. The significant difference between the "confined layer" and the "background stratosphere (FLS)", defined to be above the level ~ 65 hPa, is part of the "tape recorder" structure, i.e. the summer and winter difference. How does the ASM enhance this difference is the relevant question.

Suggestions: For the structure of the water vapor tape recorder, a good recent figure could be the Fig.2 of Glanville and Birner (2017). To estimate how much ASM is more effective in moistening the stratosphere compared to the tropical equatorial entry point in summer, you could possibly use the published result in Bian et al., 2012 (Fig 5) where soundings from Costa Rica (TC4) are used as a contrast to the ASM.

We agree with the reviewer that, for assessing the role of ASMA in moistening the stratosphere, comparing different stacks of altitudes is of limited use due to the tape recorder structure, and that the fate of the enhanced H_2O observed in the CLS needs to be addressed by explicitely taking into account the horizontal motion of the air.
Consequently, the comparison of water vapor PDFs in the CLS and free stratosphere region was removed from the manuscript, and replaced with a discussion based on results from recent literature (namely, Pan et al., 2016) (page 14 lines 10-17). In this paragraph, the statement of ASMA moistening the stratosphere is flagged as "potential" (page 14 line 10).
As mentioned in the manuscript (page 2 lines 5-6 and page 14 lines 32-33), further investigations aimed to assess the relevance of our measurements in the context of stratospheric moistening and related processes are currently ongoing. However, we refrain from performing additional analyses here, as we intend to discuss this issue in a future dedicated publication.

Additional comments:

3) This manuscript is desired to be more concise. For example, it is not clear how section 3 is contributing to the goal of the paper, since the discussion there are not related to particular scientific questions. The points made in that section may be better received when addressing particular questions in the later sections. Also suggest that you work to reduce the repetition of figures.

Suggestions: Be clear on the key objectives of the paper and focus on what serves these objectives. For example, is the comparison of the mean profiles with ECMWF necessary for the objective? Also note that some campaign specifics appeared three times (abstract, intro and campaign description).

Several minor and major changes were made in order to make the manuscript more concise and more targeted to its objectives, and to avoid redundancy in figures. In particular:
- Abstract simplified (use of acronyms reduced)
- Figure 4 (comparison with ECMWF) removed
- Figure 6 reduced (panel a eliminated, panel b given as Figure S4 in supplementary material)
- Use of acronyms reduced (FLS eliminated, use of UT and LS as individual acronyms avoided)
- Main acronyms summarized in Table 1
- Sections 4.3-5.2 rearranged and schematics figure introduced earlier in the paper (Figure 6)
- Campaign specifics removed from abstract and introduction

As a consequence, the revised manuscript is shorter than the previous version (despite several other additions requested by the reviewers were made), and we believe the presentation of the UTLS structure that we define is more fluent and reader-friendly.

4) When describing the dynamical settings and seasonal changes, it is important to connect to the seasonal change of the ITCZ. See schematic in Lawrence and Lelieveld (2010) and Pan et al 2016 for related discussions. This will put the change from August to November into the right context. It is also more desirable to show the cold point tropopause in ECMWF in addition to PV, since CPT is what you use with the observation.

According to this comment, the discussion of dynamical settings and seasonal variability was improved with references to Lawrence and Lelieveld (2010) and Munchak and Pan (2014), relating the observed features to the seasonal variations of the ITCZ and the jet streams (page 5 lines 22-23, page 5 lines 30-32 and page 7 lines 14-16). In addition, a sketch of the ITCZ is now shown in schematics of the UTLS structure (Figure 6). Figure 2 was also improved to facilitate the discussion of the seasonal variability (see details in specific comment below).

Specific comments and suggestions:

- References:

P2L13: consider replacing Park 2007 by Hoskins and Rodwell 1995.

Done (page 2 line 10).

P2l23: add Ungermann et al., 2013 before Fadnavis

Done (however, note that it is Ungerman et al., 2016, added after Fadnavis et al., 2013 to maintain chronological order) (page 2 line 19).

- Wordings:

P1L22: “It is known to be enriched” -> “It is observed from satellite to contain enhanced”

Done (“be enriched” -> “contain enhanced”) (page 1 line 22).

P1L24: remove “very”

Done (page 1 line 25).

P2L4: reconsider “depletion” – the low ozone is not due to depletion but lofted low ozone air.

The whole sentence was removed in new abstract.

P3L11: rephrase “notoriously hardly accessible”

Done (page 3 line 9).

P5L2: rephrase “too high” F. P. temperature and “too high” w.v. mixing ratio. Perhaps “w.v. mr derived by the f. p. t. are too high to be physical”?

Done (page 4 lines 32-33).

P14L26-28: A more accurate statement here should be “it is interesting to contrast the result from Pan et al. 2014, where a smaller variability of water vapor is found above the CPT ..." Comparing the water vapor range of variability in Pan et al. 2014 (figs. 6&7, _ 3-5 ppmv) with results from this work (DK17 is _ 3.5-6.5 ppm, and NT16 is similar to the Kunming), the variabilities are qualitatively the same. Not sure where you found the "no variability above CPT" as a common concept.

The whole sentence was removed in new conclusions section.

- Figures:

Fig.2: (a) Consider adding easterly jet, which will show where the sounding location was in relation to the anticyclone. (b) Also consider adding simple dynamical field, GPH or tropopause 100 hPa contour to the maps on the right to indicate the anticyclone and possibly the region of highest tropopause. (c) it may be more insightful to color the trajectories by potential temperature.

Figure 2 was subject to a number of improvements accordingly:
- Wind contours at 20 m/s and 30 m/s added, showing the easterly jet position (panels a-c-e)
- X-axis range enlarged to show latitudes 10-50°N (panels a-c-e)
- Individual days replaced with time-average of the respective campaign periods (panels a-c-e)
- Geopotential height at 100 hPa (time average of each measurement period) added to the trajectory panels, showing the region of highest tropopause (panels b-d-f)

As a consequence, Figure 2 now provides a significantly higher amount of information than the previous version, and the discussion of seasonal variability was improved accordingly (page 6 line 26 to page 7 line 6).

Figs. 6-7: there is a strong discontinuity between the two figures when you changed from pressure to altitude. Suggest you consider using pressure altitude when you can label the profiles using both pressure and altitude. This can be consistently done throughout the paper.

Unfortunately we do not understand this comment properly, and in particular it is not clear to us what the reviewer means by “discontinuity”. Figure 7 shows altitude relative to CPT as y-axis (not just altitude), therefore adding an altitude scale to all plots would not make the profiles look more similar. In addition, all figures in the paper show more than one dataset at the same time, hence adding an altitude axis along with the pressure profiles is strictly speaking not possible. Therefore, we refrain from applying any changes.

Fig. 8. There is an error in the caption: FLS should not be CPT to CPT+5 km. By definition, FLS is above the TOC. I hope this is only an error in description, not in the actual calculation.

The reviewer is right and the description was corrected accordingly (the actual calculation was correct). Note that the definitions of the free stratosphere and troposphere regions for the PDFs calculation are now given in Section 5.3 (page 10 lines 2-4) instead of in the caption of Figure 8.