Anonymous Referee #2

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

General comments

Brunamonti et al. present results from the StratoClim balloon campaigns. They measured vertical distributions of temperature, ozone, water vapour and aerosol in the south Asian UTLS during one post-monsoon and two monsoon campaigns. They identify three significant thermodynamic levels and layers, which provide a framework to understand the UTLS structure within the Asian summer monsoon anticyclone. The paper is sound and clearly within the scope of ACP(D). It is based on a new and important data set that needs to be published.
Some arguments regarding the confinement effect of the ASMA are not yet clear to me, or at least do not sufficiently consider alternative explanations: Convective height might primarily control H2O, O3, and confinement. The effect of confinement on H2O and O3 needs to be clarified. Details are given in the specific comments.
It's hard to tell whether addressing those requires minor or major revisions. Alternatively, the paper would be worth publishing even without discussing the relative importance of confinement and other processes in the ASMA. The outlook at the end of the paper shows its importance for ongoing other studies.

We are grateful to Anonymous Referee #2 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 concerning the relative importance of confinement and convective altitude are valid, as well as the considerations made in specific comments regarding age of air and convection. We improved the discussion of the H_2O, O_3 and aerosol vertical distributions by taking into account the suggested alternative explanations (mainly Sections 5.3 and 6.2), and our conclusions were revised accordingly.
As suggested by the reviewer, the use of acronyms was reduced for easier reading (including the abstract, which was substantially simplified) and redundancy in figures was avoided. A new table was introduced to summarize the main not well established acronyms (Table 1). The formulation of the UTLS structure that we propose was made more fluent by rearranging Sections 4.3-5.2 and by introducing the schematics figure (now Figure 6) earlier in the paper.
The meteorological overview section was also improved, and particularly Figure 2, now displaying time-averaged cross sections and geopotential height fields along with the trajectories.
Despite several additions (requested by the reviewers) were made, the revised manuscript is more concise than the previous version. This was achieved by avoiding repetitions (e.g. the campaign details) 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.

Presentation

(1) Too many acronyms make the paper hard to read. I suggest to count the number of occurrences of each (not well established) acronym, then write out those 50 % that occur least.

The use of acronyms was reduced in the manuscript accordingly, namely by eliminating FLS (free lower stratosphere) and avoiding the use of UT and LS as individual acronyms (e.g. CLS is now used consistently throughout the whole paper, avoiding the use of "Confined LS"). In addition, a new table was introduced to summarize the main not-well established acronyms used in the paper

(Table 1), and the schematics in former Figure 13 was introduced earlier in the paper (now Figure 6) to help the reader familiarize with the acronyms.
In order to make the manuscript more reader-friendly and the formulation of the UTLS structure that we propose more fluent, we have also moved former section 4.3 ("Horizontal confinement in ASMA") to section 5.3 ("Confined lower stratosphere"), where the acronyms TOC and CLS are defined and the schematics in Figure 6 is discussed.
Finally, the abstract was also simplified by strongly reducing the use of acronyms in it.

(2) Consider to reduce redundancy in the figures (e.g. T vs p for DK17 and NT16AUG is shown in Figs. 1, 4, 6a, 13).

Redundancy in figures was reduced accordingly:
- Figure 4 (comparison with ECMWF) was removed, as it was not directly necessary to the objectives of the paper
- Figure 6 reduced: panel a (T vs p) was removed due to redundancy, and panel b (θ vs p) moved to supplementary material (Figure S4).

(3) Consider to annotate curves etc. in the figures only, rather than the captions. For instance in Fig.13, the campaigns and the meaning of the colours impede reading of the caption, but are already obvious from the panels.

In Figure 13 the curves are identified by the color of their axis label, and since the figure is already quite "full", we believe there is no need to add an extra legend to it (we tried different options but figure becomes too hard to read). In the revised version, Figure 13 was improved with respect to the previous version (θ dashed lines changed from green to grey, labels rearranged).

Specific comments
Line numbers in the following are approximate, sometimes referring to the arguments of an entire paragraph.

P3L7: What about aircraft measurements? CARIBIC provides a lot of species in high resolution. Dedicated campaigns (ESMVal/HALO, OMO/HALO, StratoClim/Geophysica) sampled higher altitudes and also did some profiles. There are a few aircraft in-situ monsoon papers, at least from CARIBIC and ESMVal.

Reference to aircraft measurements in ASMA, namely from ESMVal (Gottschald et al., 2018) and CARIBIC (Raute-Schöch et al., 2016) added to the introduction accordingly (page 3 lines 9-11).

P7L2: Fig. 2 shows snapshots of individual days. Are those days chosen to be representative in some respect? Please consider to provide time averages for the respective measurement periods (or for the sounding days).

Change made accordingly: individual days replaced by time average of each measurement period (Figure 2, panels a, c, e).

P7L9: There are different PV thresholds for the dynamical tropopause. Please provide a reference or justify your choice.

Reference to Kunz et al. (2011) for the choice of the PV threshold for the dynamical tropopause was added, and the discussion was revised accordingly: we now use PV = 3-4 PVU which is more appropriate for the considered latitude and season (page 7, lines 5-6).

P7L17: Given the structural differences of the tropopause region between summer and autumn: Why do you choose the same pressure altitude to compare the two seasons? You might consider to show trajectories started over some altitude range, or from a specific distance to the respective tropopause altitudes.

Figure 2 is meant to illustrate the differences between the UTLS structure and dynamical features of the monsoon vs. post-monsoon season in a qualitative manner. The contrast between monsoon and post-monsoon season is already evident by comparing panels b-f vs. panel d (furthermore now that geopotential height fields are also shown in addition to trajectories). Therefore, we believe that there is no need to further refine the trajectory comparison.

P8L23: What is the spacing between trajectory starting points?

Trajectories are initialized at 5 hPa intervals. Manuscript modified to include this information (page 9 line 5).

P8L26: The ASMA box seems to be rather big. Please justify or provide a reference.

Our ASMA box definition (10-50°N, 0-140°E) is based on the average geopotential height fields during our campaign periods, now shown in Figure 2 (page 9 line 11 rephrased accordingly). Similarly large domains were used to approximate the ASMA area in previous literature, e.g.:
- 15-45°N, 5-105°E (Vernier et al., 2011)
- 10-60°N, 10-160°E (Ploeger et al., 2015)
- 0-60°N, 0-140°E (Pan et al., 2016)

Therefore we believe our boundaries provide a reasonable approximation of the ASMA area.

P9L19: Formulation for O3 is ambiguous. Please revise.

The whole sentence was removed from the revised version (for the sake of brevity).

P10L25: What do you mean by “feature” here: H2O max, O3 min, or the combination of both? Anyway, neither the H2O feature, nor the O3 feature is necessarily related to differences in the strength of convection alone. The time since the last convective influence on the air mass might also be important. If NT16Aug by chance sampled older air on average, the H2O feature would have been smoothed out. Also, convection increases the availability of O3 precursors, leading to enhanced photochemical O3 production. The absence of an O3 minimum just above the LRM in NT16Aug might be due to longer confinement or to higher O3 production. Please discuss.

We agree with the reviewer that considering the age of air (meant as time elapsed since the last convective influence) is important due to enhanced photochemical O_3 production in ASMA, and that the fact of missing O_3 minimum above the LRM in the mean profile of $NT16_{AUG}$ is consistent with older air sampled on average during this campaign vs. fresh convective outflow sampled more frequently during DK17. We also recognize that the same argument applies to the H_2O maximum above the CPT (see comment below).
Assuming the balloon soundings are frequent enough to be statistically representative of the respective measurement periods, we still would argue that this evidence does suggests that more frequent deep convection occurred during DK17 compared to $NT16_{AUG}$.
The manuscript was revised according to this consideration (page 10 lines 21-26).

P11L5: This is consistent to older samples in NT16Aug.

Same arguments as in the comment above applies to the H_2O maximum above the CPT in DK17: we agree with the reviewer and the manuscript was modified accordingly (page 10 lines 24-25).

P11L8: This argument is not quite clear to me. H2O in the CLS is compared to H2O in higher altitudes. The difference is attributed to the horizontal confinement effect of the ASMA. However, first order this might just reflect the decreasing frequency of convective tops with altitude. To quantify horizontal (isentropic) confinement, you might consider comparing back-trajectories according to their respective lengths in the ASMA. Please discuss.

We agree with the reviewer that, to some extent, decreasing “convective signature” with altitude might simply reflect the decreasing frequency of overshooting convective tops with altitude. Indeed, the current status of our analysis does not explicitly disentangles the effects of convective vs. confinement top height in controlling the vertical distributions of H_2O, O_3 and aerosols above the CPT. Furthermore, the anticyclonic confinement is caused by the high pressure built up by the deep convection, hence the two processes are intrinsecally connected.
However we also consider that, to our knowledge, convective updrafts overshooting the CPT by 1.5-2 km were never observed (also not by the Geopyhsica campaign during StratoClim in Nepal), and that the gradients in the vertical distributions of H_2O, O_3 and aerosols are in good agreement with the TOC inferred from backward trajectories.
Therefore, we still would argue that these evidences suggest that the horizontal confinement effect of ASMA plays an important role in shaping the vertical distributions of H_2O and O_3 above the CPT (and in particular the H_2O enhancement in the CLS). Nevertheless, we recognize that further analysis would be required to fully disentangle the relevance of the different transport processed, and we revised the manuscript accordingly (page 10 line 32 to page 11 line 5).

P11L14: Not necessarily, see previous comments on convective strength versus age. Age is related to confinement. Please disentangle.

Same as above: we agree with the reviewer and the manuscript was modified accordingly (page 10 lines 25-26).

P11L16: Confinement tends to increase O3 via photochemical production. Please discuss horizontal confinement versus the altitude profile of convective influence. The argument regarding the quality of the TOC definition could go the other way round i.e. (simplified): ASMA is driven by convection -> convection reaches to a certain altitude -> no confinement above convective influence.

Same as above: we agree (at least to some extent) with the reviewer, and the manuscript was modified accordingly (page 11 lines 3-5).

P11L20. Ditto.

Same as above: the statement that TOC controls the vertical distributions of H_2O and O_3 was removed and replaced with a more detailed discussion (page 10 line 32 to page 11 line 5).

P11L28: Could different temperatures or different ages (time since last convective influence) be alternative explanations for the difference between DK17 and NT16Aug?

We believe that different temperatures are the main driver of the different ice saturations measured during $NT16_{AUG}$ and DK17, as it is stated already in the manuscript (page 11 line 9). At the current status of our analysis, it is not obvious to infer a correlation between of age of air and ice saturation. The statement that higher ice saturations in DK17 are related to stronger convective activity was removed from the manuscript.

P12L19: Comparing NT16Aug to NT16Nov per se generally reflects seasonal variation. Air mass origins might be totally different in August and November, even if there was no ASMA confinement in August. Please reformulate or elaborate, why NT16Aug without ASMA would be like NT16Nov.

We fully agree with the reviewer that, in addition to confinement (or lack of thereof), air mass origin and potential direct exposure to deep convection is important, as it provides the supply of aerosols and precursor gases to the UTLS. The manuscript was revised accordingly (page 12 lines 1-3).

P12L29: The parameters affecting the threshold depend on region and season. Is the threshold of Vernier et al. applicable to your measurements without adjustments?

The parameters affecting the cloud-filtering thresholds are independent of region and season. These thresholds are just based on optical considerations and the typical size ranges of atmospheric aerosols and ice crystals (see page 12 lines 10-12). Furthermore, it happens that the thresholds by Vernier et al. (2015) have been developed based on measurements from the same region and season as our measurements (Lhasa, China during the ASM season), and comply with CALIPSO depolarization criteria. Therefore, we are confident that these thresholds are applicable to our measurements without adjustments.

P13L21: Could the thermodynamic conditions at the CPT enhance aerosol formation from gaseous precursors? In that case convective outflow or confinement might not be as important.

We agree with the reviewer that thermodynamic conditions (namely colder temperatures) at the CPT can enhance the partitioning of condensable material (e.g. nitrate) to the aerosol phase, and therefore the fact that ATAL shows maximum BSR at the CPT is likely also influenced by temperature, in addition to confinement. The manuscript was revised accordingly (page 13 lines 5-7).

P13L22: “its level”: Does this refer to confinement or aerosol enhancement? Please reformulate.

Maximum strength refers to confinement. Sentence rephrased accordingly (page 13 lines 5-7).

P13L25: There is no convective supply of aerosols/precursors in November. Additionally there is no confinement. The ASMA might to some degree enhance ATAL. Consider revising to clarify causes and effects.

Same as discussed above: we agree with the reviewer that supply/lack of aerosols and precursors via deep convection needs to be considered in addition to confinement. Sentence revised accordingly (page 13 line 12).

P14L33: Please also discuss alternatives to confinement.

Alternatives to confinement (supply/lack of aerosols and precursors via deep convection) already discussed in Section 6.2 (see comments above). Conclusions revised accordingly (page 14 lines 18-20).

P15L3: Please discuss convective height versus confinement.

Convective height vs. confinement already discussed in Section 5.3 (see comments above). Conclusions revised accordingly (page 13 line 30 to page 14 line 2).

P15L6: Comparing different altitudes is of limited use for estimating the effects of confinement.

As discussed above, we agree with the reviewer that comparing different altitudes is of limited use for estimating the effects of confinement. This statement was removed from the conclusions.

P15L7: Please also consider convective height and possible vertical variations of aerosol formation.

Same as above: alternatives to confinement (convective height, supply/lack of aerosols and precursors via deep convection) now discussed throughout Sections 5.3 and 6.2. This statement was removed from the conclusions.

P24, Fig. 2: White contours for water vapour are not discernible. Please revise, e.g. consider omitting or doing an extra plot for them. Insets in panels a, c, e are too small. Consider to include those lines in the right column's panels.

White contours for IWC eliminated in the revised version of Figure 2.

P25, caption of Fig. 2: "Note that in panel c (NT16Nov), trajectories started : : :"
- Panel c is not about trajectories.
- stared -> started
- ppmm -> ppm

Caption of Figure 2 revised and typos corrected.

P27, Fig. 4: Consider to use ECMWF data only from the times of the respective soundings.

Figure 4 was removed for the sake of reducing redundancy in figures.

P30, caption of Fig. 6: Consider to give a short explanation of "GPS geometric altitude"

Former Figure 6 (now Figure 4) was revised and the caption rephrased to avoid mentioning "GPS" geometric altitude, but just "geometric altitude" as it is done previously in the main text (see page 5 line 12).