Response to the review (by Referee #1) of
"Investigating the role of typhoon-induced gravity waves and stratospheric hydration in the formation of tropopause cirrus clouds observed during the 2017 Asian monsoon"
by Amit Kumar Pandit et al.

1 General comments

This study reports on a subvisible cirrus cloud observed at the tropical tropopause in one of the BATAL soundings. The instrumentation carried by the balloon on this flight includes a backscatter sonde and a particle counter, besides a standard radiosonde. The study first presents how these instruments are used to precisely characterize the cirrus microphysical properties. In the following part, the authors try to identify the mechanisms that could have led to the formation of this cirrus. Combining backtrajectories with satellite brightness temperature maps and lidar soundings, they suggest that ovsershooting convection associated with typhoon Hato over the South China sea a few days before the cirrus observation may have injected water vapor in the lower stratosphere. The formation of the cirrus was then cause by gravity-wave induced cooling while the water-vapor enriched air parcels were advected toward India by the monsoon anticyclone.
The article is to my opinion a very nice observational study, which provides sound evidence for the formation mechanism that is advocated. The paper is well-written and the argumentation is easy to follow. I would thus recommend its publication with only minor revisions, which are described below.

Reply: We thank the referee for going through our manuscript and providing constructive feedback for its further improvement. We are glad to read the referee's views on our manuscript. In the following, our point-by-point response to the referee's comments (in black text) are presented in blue text.

2 Minor issues

• The presentation of Section 2.2.2 (Solair Boulder Counter) may be improved. From line 9 on page 8 to the end of the section, the text does not provide details specifically on the instrument, but rather describes how derived quantities (effective diameter or Ice Water Content) can be inferred from the raw counter or (more confusingly) from the backscatter sonde observations. It may be easier for the reader if an own separate subsection were devoted for this derived quantities.

Reply: We are thankful to the referee for pointing this out. As suggested, we have separated Section 2.2.2 into two subsections, one (Section 2.2.2.1) containing the details of Solair Boulder Counter while the other (Section 2.2.2.2) describing the estimation of cloud microphysical properties from COBALD and Boulder Counter data in the revised manuscript.

• Figure 2: It will help the reader to have an additional vertical pressure scale in this figure. Since pressure is very likely measured by the radiosonde, this should not be an issue to add this scale, and it would greatly ease the comparisons with figures 7b, 9 and 10b, which display water vapor on pressure levels.

Reply: We completely agree with the referee and thank him/her for this suggestion. We have added a vertical pressure scale in Fig. 2b, as suggested.

• Figure 3: I am uncertain about the relevance of the ERA5 cloud cover fraction on the top panel. Since the previous figure showed multiple cloud layers, it is probably quite speculative to make a link between the ERA cloud cover fraction and the CL5 cirrus cloud studied in this paper. The fraction numbers are themselves furthermore very low... On the lower panel of this figure, or on the previous figure, an ERA5 vertical profile of relative humidity over ice might on the other hand provide some additional information.

Reply: We agree with the referee and thank him/her for this suggestion. We have replaced ERA5 cloud fraction with ERA5 temperature at 100 hPa as suggested by another referee (Referee#3). We have also added the vertical profiles of relative humidity over ice from ERA5 during the ascent and decent of the balloon in Fig. 3c, as suggested.

• p17, l1-2: I am a bit skeptical about the quite optimistic statement that the effective diameter obtained with Eq. (5) is in good agreement with the observations. Indeed, in Table 3, one observes that the effective diameter is monotonically increasing as temperature decreases for the observations (Eq. 3), whereas it is continuously decreasing when estimated according Eq. 5. I have therefore the impression that the claimed agreement is somehow fortuitous here.

Reply: We agree with referee's argument and therefore, we have deleted that statement in the revised manuscript.

• p28, l18-19: actually I do not see the quasi-periodic feature in temperature in Figure 11, but rather in Figure 12.

Reply: We agree with the referee. The quasi-periodic feature in temperature is not clearly visible in Fig.11 due to the overlap of air parcels originating from different altitudes. In the revised manuscript, we have labelled the quasi-periodic features in Fig.11.

• p33, l17-18: Be careful though that the 8-10K decrease emphasized in Figure 14 is a Eulerian perturbation. In other words, it is different from the cooling that may have undergone air parcels coming above Hyderabad on that day. The temperature fluctuations felt by air parcels are those shown in Figure 12.

Reply: Thanks for this comment. We completely agree with the referee's comment.

3 Additional corrections

• p9, l15: the sentence that starts here should be rephrased.

Reply: The sentence is rephrased, as suggested.

• Table 1: please use De instead of Deff , as in the text.

Reply: We have replaced “Deff” with “D_e” in Table 1, as suggested.

• Table 2: Distance rather than displacement?

Reply: We have replaced “displacement” with “distance”, as suggested.

• p24, l22: a right) is lacking after TEJ.

Reply: We have added a right “)” after “TEJ”.

• p27, l5: a space is lacking before 19 km.

Reply: This has already been corrected.

• p29, l22: propagation rather than movement.

Reply: We have replaced “movement” with “propagation”.