

## Author Comment to Referee #3

We thank referee#3 for his helpful comments on the manuscript. The referee's comments are presented in *italics* and our point-by-point answers are highlighted in [blue](#).

*The paper presents an analysis of the location of the South Asian High (SAH) at 100 hPa for the time period from 1979 until 2014. In previous studies a bimodality in the longitudinal location of the South Asian High was found in two preferred regions classified into the Tibetan Mode (TM) and the Iranian Mode (IM), respectively. In this study, the existence of a bimodality of the SAH is analyzed in six different reanalyses: (1) The NCEP/NCAR Reanalysis 1 (NCEP1) from the National Centers of Environmental Prediction (NCEP) and the National Center of Atmospheric Research (NCAR), (2) NCEP/DOE Reanalysis 2 (NCEP2) from NCEP and the Department of Energy (DOE), (3) the Japanese 25-year reanalysis (JRA25) from the Japan Meteorological Agency (JMA) and the Central Research Institute of Electric Power Industry (CRIEPI), (4) the 55-year reanalysis (JRA55) from JMA, (5) the ERA-Interim reanalysis (ERA-I) from the European Centre of Medium-range Weather Forecast (ECMWF) and (6) the Modern Era Retrospective-Analysis (MERRA) from the National Aeronautics and Space Administration (NASA) on interannual, seasonal and synoptic time scales.*

*The main conclusion of the study is that only the NCEP1 reanalysis shows are clear bimodal structure of the longitudinal location of the SAH for daily and 5-day mean data. Moreover, the study demonstrate a connection between westward/eastward movement and northward/southward movement of the SAH based on ERA-Interim reanalysis data. The connection between the location of the SAH, OLR, and rainfall over India and the West Pacific in analyzed.*

*General comments:*

- The bimodality of the SAH, its drivers and its connection to precipitation is very important and interesting. I recommend that the paper is suitable for publication after revision to address the comments listed below.*

[We thank referee#3 for this very positive and encouraging comment and the apt summary of our study.](#)

- The paper is well-structured and significant for publication by ACP. However, section 4 and 5 are not well included within the abstract, introduction and the title (see comments below) and look like an appendix to section 3. For both topics (Sect. 3) and (Sect 4-5), the authors could go deeper into the details (see comments below). The authors might think of splitting this paper into two parts, however this is fully at the decision of the authors.*

[Again we thank the reviewer for this positive feedback and acknowledge, that the title did not incorporate the whole manuscript. Hence we changed the title to: "Movement, drivers and bimodality of the South](#)

**Asian High**". We think that the different sections of the manuscript belong together and hope that in the revised version this connection is presented more clearly. Thus we refrain from splitting the paper into two parts. Of course, the suggested extensions of the analyses have been considered though and are addressed below.

- *I believe that the paper will benefit from a bit more discussion of its results in context with previous studies to better demonstrate what is new in this study. I recommend to do that in an separate discussion section. Further, I believe that the paper will benefit from shorter conclusion section highlighting the main results. Some of the discussion could be shifted in the separate discussion section.*

We followed the referee's suggestion to split the former Section 6 (Summary and Discussion) into two sections. Section 6 now contains the discussion of our results and section 7 is the Conclusion section. As recommended we shifted additional parts containing discussion elements to the Discussion section. We hope that the additional references given (see comments below) and the restructuring of the manuscript (especially the new Discussion and Conclusion section) will help to contextualise our work.

*Specific comments:*

- *I recommend that authors think about a more comprehensive title of the paper. The current title *Is there bimodality of the South Asian High?* includes only the first part of the work (question 1). Section 4 and 5 (question 2 and 3) are not considered in the current title.*

Agreed. As mentioned before, we updated the title to better incorporate the whole manuscript.

- *p. 1, line 22-25: It would be helpful for the reader to add here in the introduction briefly how the SAH center location is defined/calculated by Zhang et al. (2002) (e.g. estimated as geopotential height maximum). The method by Zhang et al. (2002) is described in detail in Sec. 2.2, nevertheless I think it is helpful to also describe the basic idea within the introduction.*

Done. We changed the sentence referring to Zhang et al. (2002), so the method is stated. It now reads: **"Apart from the highly variable synoptic behaviour of the SAH, Zhang et al. (2002) have found that the longitudinal distribution of the SAH centre location - as identified by the geopotential height maximum along the ridge line (see green line in Fig. 1a) - is bimodal."**

- *p. 2, line 18-19: Tackling these questions is also subject of past and upcoming measurement campaigns such as ESMVal (2012), OMO (2015) and StratoClim (2016). This sentence seems a bit out of context here. Perhaps the authors could emphasize here that only very sparse aircraft*

*measurements in altitudes of the SAH were available in the region of the Asian monsoon until now... Further the authors could cite some results of papers that were already published to these (previous) campaigns (e.g. ESMVal/TACTS/OMO) addressing the Asian monsoon anticyclone and its impact on the stratosphere or cite overview papers instead of citing project web pages.*

We agree with the reviewer and hence we rephrased this part. We now mention studies related to TACTS/ESMVal and the Asian monsoon (Vogel et al., 2014; Müller et al., 2016; Vogel et al., 2016). For the more recent and directly monsoon targeted campaigns OMO and StratoClim (to our knowledge) no studies incorporating measurements (or overview papers) are available so far.

- *p. 3, line 31: Second, along this ridge line the maximum of the daily (pen- tad/monthly/seasonal) geopotential field at 100 hPa is determined. My question is here, in case you would have simultaneously two maxima in geopotential field (e.g. splittings of the anticyclone), using this method you would only count the strongest maximum. The impact of the second lower maximum is not taken into account by this method. Is that correct? If yes, the used method would not reflect the full variability of the Asian monsoon anticyclone.*

This is correct. However, this is the common method to analyse the location of the SAH centre as can be inferred from the new Table 2. As we are referring to these previous studies we chose to keep our analysis consistent. In first tests with an adapted analysis using ERA-I daily data, we allowed for a second maximum (when a clear, spatially separated second local maximum was found). An additional maximum was found in roughly 16% of the days. The qualitative results of our analysis remained unchanged, however.

- *p. 4, line 12: As in the studies mentioned before, we choose the 100 hPa level in our study to be consistent with these previous works. The authors cite before papers by Qian et al. (2002), Zhang et al. (2002), Yan et al. (2011) using the 100 hPa level. In the introduction the authors mention some papers demonstrating the strong variability in strength and location of the SAH: Hsu and Plumb, 2000; Popovic and Plumb, 2001; Garny and Randel, 2013; Ploeger et al., 2015; Vogel et al., 2015. To my knowledge all these studies use levels of potential temperature for their analysis. Thus more studies are mentioned within the paper using level of potential temperature.*

We agree with the referee's comment. This was a unclear statement in the first manuscript. The sentence "As in the studies mentioned before,..." was meant to refer to the studies mentioned directly ahead of it in the Section 2.2 (Method). These studies explicitly deal with and report bimodality and are all based on pressure levels. This part has been rephrased and we hope that the new Table 2 clarifies our original statement.

- *The authors do a good job of analyzing the bimodality using six different reanalysis data including daily, pentad, monthly, and seasonal geopotential fields taking into account the high temporal variability of the location of the SAH and differences between the used data set. However, would the same analysis on a fixed level of potential temperature (e.g. at 370 K or 380 K) result in the same conclusions as for the 100 hPa level? If yes, the result would strengthen the results of the paper. If not, it would be interesting to discuss the differences. I suggest to include in the paper the same analysis, but on a level of potential temperature in addition to the 100 hPa pressure level.*

This point is also connected to the comment before regarding analyses on pressure levels vs. isentropes. We hope that the clarification in the previous comment explains why we are focusing on pressure levels. Nevertheless, we analysed the maximum location of the Montgomery streamfunction on the 395K from ERA-I. (This level is available directly from ECMWF.) Consequently this paragraph has been added to the discussion section: **”Previous studies which address the bimodality of the SAH have mostly focused on the 100 hPa level (see Table 2). To see how robust our results are, we employed ERA-I on the 395 K level. The SAH centre location was defined as the maximum of the Montgomery streamfunction along the ridge line. We found that the PDFs of the SAH centre location with respect to daily and monthly data are similar to ERA-I on 100 hPa. For the seasonal mean data we have found that the distribution changes in favour of the TM and IM, i.e. for seasonal data 12, 10, 14 years are located in the IP, mid and TP region, respectively.”**

- *p. 7, line 4-5: Common to all reanalyses is that there is a shift of the distribution to the west from June to July and a shift back to the east from July to August. Is this shift also found in monsoon rainfall patterns?*

We hope that the inclusion of daily GPCP data in the new Fig. 11 b) and the corresponding paragraph in Sect. 4: **”The seasonal east–west shift can be also found in daily precipitation data from GPCP during the period 1997–2013 (see Fig. 11 b) and the seasonal northward migration of precipitation has been noted in previous studies (e.g. Yihui and Chan, 2005, their Figure 3).”** answers this question. Out of structuring reasons we refrain from stating this at this point in the manuscript, though.

- *p. 7, line 7: Based on seasonal mean (JJA-mean) data the SAH shows a bimodal structure in the reanalyses NCEP1, NCEP2 and JRA25 (see Fig. 8). Here, NCEP1 and JRA25 show two pronounced peaks over the TP and IP... I cant see a bimodal structure in JRA25 in Fig. 8. Do you mean here NCEP2 data?*

Agreed. We changed this paragraph to: **”Based on seasonal mean (JJA-mean) data the SAH shows a bimodal structure in the re-**

analyses NCEP1 and NCEP2 (see Fig. 7). Here, NCEP1 shows a pronounced peak over the TP and a second one over the IP, whereas NCEP2 shows only a sharp peak over the IP. Additionally, JRA25 shows low probabilities around 70° E. In contrast, CFSR, ERA-I, JRA55 and MERRA show high probabilities over the whole centre region (~60° E–85° E, depending on the reanalysis).”

- p 8, line 6: *To the right we can observe exactly the opposite behavior, i.e. propagation to the east. Please clarify what does that mean?*

We rephrased this part and hope that our statement is clearer now. The paragraph now reads: **”In Fig. 8 the lowest OLR values are mostly confined to the area 75° E–105° E and are mostly located east of the highest geopotential height values. East of the OLR minimum we can observe eastward migration of high geopotential, associated with eastward eddy shedding of the anticyclone. A strong shedding event is observed in mid August 1983 (turquoise star in Fig. 8a). West of the OLR minimum region, the core of the anticyclone usually propagates westwards.”**

- p 8, line 24: *...e.g. identifying the Somali-Jet, which brings moisture from the Arabian Sea to India Please add here a citation.*

Done. The reference Rodwell and Hoskins (1995) has been added.

- p 8, line 21: *For seasonal mean and deseasonalised monthly mean data all reanalyses show that westward (eastward) movement of the SAH is related to northward (southward) movement. The separate analysis of June, July and August yields that this relationship is strong during June and July. Does that mean that during August the SAH is shifted to the north and convection areas are further south and can not trigger an east-west shift? Please clarify that.*

This analysis is simply looking for the general movement without explicitly naming the drivers here. The changes in correlation are indicating that the withdrawal is not as clearly towards southeast as the shift towards northwest during the build up phase of the SAH. The withdrawal seems to be first rather eastwards and probably consequently back southwards/equatorwards. This can be inferred from the black solid lines in Figs. 11 and 12. By the end of September the longitudinal position is the same as at the beginning of May. The southward withdrawal, however, seems to take longer than the northward migration during the first part of the monsoon season.

- p. 8 (sect. 4) *The east-west and north-south movement (shift) of the SAH is also found in previous studies. The authors should add here some references and discuss their results in the context of previous studies. In addition, an east-west and north-south shift is also found in rainfall patterns*

over India/Asia. I think it would strengthen the findings of the paper, if the authors would discuss this connection. Further, convection and rainfall in this region is strongly impacted by El Nino and La Nina events. Have El Nino events an impact on the correlation between westward (eastward) movement and northward (southward) movement of the SAH?

The shift of the SAH and precipitation is included in the new Fig. 11 b) and mentioned in Sect. 4. The related discussion is also referring to the work of Lau et al. (1988) (which we did not know of during the preparation of the first manuscript) and Yihui and Chan (2005). If referee#3 wants a specific reference to be named, we would be happy to include it in the manuscript. Further, we hope that the discussion of Wei et al. (2014, 2015) is thorough enough in the new discussion section. The connection of ENSO (in terms of Nino 3.4) with the ISM/SAH location was investigated using data from CPC and the corresponding results are now mentioned in the Discussion section (Sect. 6).

- p. 10, line 7-15: Again is there a connection between east and west phase to the El Nino/Southern Oscillation (ENSO)?

Please see comment above.

Technical corrections:

- p.1, line 10: shortcut ORL is used within the abstract without explanation  
Done.
- p.2, line 11/13: downloaded → used ?

Done. "Downloaded" is no longer mentioned in the paragraph. The paragraph now reads: "The data used in this study cover the NH summer seasons 1979 to 2014 (2010/2013 for CFSR/JRA25). Meteorological fields (geopotential height, wind and surface temperature) of all reanalysis data sets have been used with the provided resolution of  $2.5^\circ \times 2.5^\circ$ , except for MERRA which has been regrided from the native resolution ( $0.5^\circ$  latitude by  $0.67^\circ$  longitude) to a  $2.5^\circ \times 2.5^\circ$  grid..."

- p.4, line 21: long term → long-term

Done.

- p.6, line 30: remove line break

Done.

- Figure 1: The grey box.. I assume the authors mean the box marked by the dashed dotted line. If not, please clarify.

Done. Following the reviewer's description, this part in the figure caption has been changed to: "The box marked by the grey dashed dotted line indicates the range of the data which is used to diagnose the centre."

## References

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