

Interactive comment on “Is there bimodality of the South Asian High?” by Matthias Nützel et al.

Anonymous Referee #3

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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.

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The main conclusion of the study is that only the NCEP1 reanalysis shows a clear bimodal structure of the longitudinal location of the SAH for daily and 5-day mean data. Moreover, the study demonstrates 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 is 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.

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.

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 a separate discussion section. Further, I believe that the paper will benefit from a shorter conclusion section highlighting the main results. Some of the discussion could be shifted in the separate discussion section.

Specific comments:

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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.

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.

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.

p. 3, line 31: 'Second, along this ridge line the maximum of the daily (pentad/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.

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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.

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.

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?

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 can't see a bimodal structure in JRA25 in Fig. 8. Do you mean here NCEP2 data?

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comment

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?

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

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.

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?

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

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