

Author Comment to Referee #1

We thank referee#1 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.

- *This paper analyzes the northern-summer South Asian High (SAH) at 100 hPa and related variables in six reanalysis data sets, and found that the "bimodality" of the SAH is only significant in NCEP1 (and with a lesser extent in NCEP2). All more recent reanalyses do not show such a strong bimodality. This is a very interesting and important work. However, to me, it would be much more appropriate to hypothesize that NCEP1 and NCEP2, the old 1990s reanalysis systems, are wrong in terms of the possible SAH bimodality and to discuss why they are wrong. The authors, however, do not give their answer (or hypothesis) clearly to the question, and continue to make further data analyses. Thus, after reading through the manuscript, I am somewhat at a loss regarding the question in the manuscript title.*

We thank referee#1 for the encouraging classification of the manuscript. Regarding the question of which reanalysis is right or wrong, we can not give a definite answer (although the natural reflex would be that the newer "and thus better" reanalyses should be right) and the title was supposed to be in a way rhetorical. In response to referee#3's comment we changed the manuscript title to: "**Movement, drivers and bimodality of the South Asian High**" to better incorporate the whole manuscript. Also, especially in the new Conclusion section we address implications of this result and in the Discussion section we touch how difficult it is to assess which reanalysis is wrong and why. For example, already NCEP1 and NCEP2 show huge differences of the PDFs, although the base model and assimilation data is in principle the same, except for the changes mentioned in Kanamitsu et al. (2002). Nevertheless, we address possible reasons for the difference of NCEP1 data in the new discussion section and stress that it is likely that the bimodality is an artefact of NCEP1.

- *It is unfortunate that the authors do not analyze the latest NCEP reanalysis, the CFSR (Saha et al., BAMS, 2010). If the CFSR also shows similar tendencies to the modern reanalyses, the authors would also be able to obtain advice from the NCEP colleagues why the old NCEP reanalyses are wrong regarding the 100 hPa geopotential height over the middle to south Asian regions. Some hints might be as follows.*
 - (1) *NCEP1 and NCEP2 are the only reanalyses available now that assimilate temperature retrievals for TOVS and ATOVS measurements; all more recent reanalyses assimilate radiance data directly.*
 - (2) *NCEP1 and NCEP2 are the only reanalyses that use the sigma coordinate, while all others use the hybrid sigma-pressure coordinate.*

(3) As discussed by Kanamitsu et al. (2002), NCEP2 is an updated version of NCEP1, where several errors in the original NCEP1 system were corrected; thus, NCEP2 is generally closer to the truth than NCEP1.

We thank the reviewer for this helpful comment. Consequently, we have included CFSR data in our study. Regarding the placement of the SAH centre this data set is mostly in agreement with ERA-I and JRA55. Regarding the possibility to get advice from NCEP colleagues: Our motivation was to spread the word about this discrepancy between NCEP1 and the other reanalyses, because a couple of studies have been referring to the concept of bimodality. We hope that our motivation and the implications are more clearly stated now in the separate Conclusion (Sect. 7). Still we included the hints (1) and (2) in the Discussion (Sect. 6). Hint (3) has already been mentioned in the first manuscript version (maybe a little too hidden/detached) and was hence shifted to the Discussion (Sect. 6) as well.

- Also, it may be useful to make a separate analysis using data only for the recent 10 years when several advanced satellite measurements are available (which NCEP1 and NCEP2 cannot utilize because of the oldness of their system). (Also, though it may not be a key here, investigation on GNSS Radio Occultation impact may also be interesting. MERRA does not assimilate GNSS RO, while MERRA-2, JRA-55, CFSR, and ERA-I do assimilate it.) If there is an influence of changing observing systems, the results of such an analysis would give us another hint.

The results for daily data have been analysed for the recent 10 years without significant changes compared to the full time period. We mention this analysis in the Discussion (Sect. 6).

- In Introduction, and in other places, the authors cite several previous works that discuss the SAHs bimodality. It would be important and useful to summarize the information (in a table) on: (1) data used, (2) period of the analysis, (3) variables used, and (4) details of how to define the SAH centre(s) for the SAH bimodality study in each paper. Are there any works that use a reanalysis other than NCEP1?

As suggested by the referee, we have added a table summarising this information (Table 2 in the revised manuscript). To our knowledge the study by Wei et al. (2014) is the only study on pressure levels not working with NCEP1 data that shows a clear bimodality. Wei et al. (2014) use ERA40 data at 200 hPa, as does their follow up study (Wei et al., 2015). We did not include ERA40 in our study as this reanalysis is only available until 2002.

- In the latter half of Section 4 (page 8, lines 34-), and beyond (to the end), I cannot follow the discussion fully because the authors do not give any conclusion which is correct, NCEP1 or ERA-I (and others) regarding the

SAH bimodality, and because they switch the main data set to ERA-I and continue discussion.

We agree with the referee, however, as said before we cannot claim that NCEP1 is false, as we do not have certain proof of this fact. Hence we expanded the transition from Section 3 to Section 4 which now reads: **”The salient disagreement of the reanalyses in the distribution of the SAH center location is our motivation to revisit the questions of how the SAH moves on various time scales and how this movement is caused. To tackle these questions, we will focus on results based on observational and ERA-I data during the next two sections (Sects. 4 and 5). We choose ERA-I as it is a heavily used reanalysis with the most recent data assimilation scheme. Apart from that, our choice is arbitrary and we address the sensitivity of the presented results with respect to the reanalysis in the discussion (Sect. 6).”** Before, the discussion of the sensitivities has been spread between the respective Sections and the previous Section 6 ”Discussion and Summary”. We hope that our line of argument is easier to follow now.

- *In conclusion, I think that the SAHs possible bimodality problem is a very interesting and important one, but the current manuscript is not conclusive. Major revisions explained above are necessary.*

Again we thank the reviewer for this positive feedback and hope that our reply and the revised version of the manuscript answer the points raised by the referee.

References

- M. Kanamitsu, W. Ebisuzaki, J. Woollen, S.-K. Yang, J. J. Hnilo, M. Fiorino, and G. L. Potter. NCEP-DOE AMIP-II Reanalysis (R-2). *B. Am. Meteorol. Soc.*, 83(11):1631–1643, Nov. 2002. ISSN 0003-0007. doi: 10.1175/BAMS-83-11-1631. URL <http://dx.doi.org/10.1175/BAMS-83-11-1631>.
- W. Wei, R. Zhang, M. Wen, X. Rong, and T. Li. Impact of Indian summer monsoon on the South Asian High and its influence on summer rainfall over China. *Clim. Dynam.*, 43(5-6):1257–1269, 2014. ISSN 0930-7575. doi: 10.1007/s00382-013-1938-y. URL <http://dx.doi.org/10.1007/s00382-013-1938-y>.
- W. Wei, R. Zhang, M. Wen, B.-J. Kim, and J.-C. Nam. Interannual Variation of the South Asian High and Its Relation with Indian and East Asian Summer Monsoon Rainfall. *J. Climate*, 28(7):2623–2634, Dec. 2015. ISSN 0894-8755. doi: 10.1175/JCLI-D-14-00454.1. URL <http://dx.doi.org/10.1175/JCLI-D-14-00454.1>.