Weather Clim. Dynam. Discuss., referee comment RC1
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Comment on wcd-2021-19

Anonymous Referee #1

Referee comment on "Seasonal forecasts of the Saharan heat low characteristics: a multi-model assessment" by Cedric G. Ngoungue Langue et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2021-19-RC1, 2021

ERA5 reanalysis data is used to evaluate the performance of two seasonal forecast model systems, the SEAS5 and MF7, in simulating the variability of the Saharan Heat low (SHL) at various timescales over the 1993 – 2016 period. Strengths and weaknesses of each forecast model are discussed, and statistical bias correction methods are applied to improve the forecast of the SHL in the forecast models. While the methods applied in this paper seem appropriate for the analysis conducted, I do have a serious concern (see below) that needs to be addressed.

My concern is that the entire validity of the results presented rely on the assumption that the ERA5 reanalysis is providing reliably accurate information since it is being used here as the "target" for comparison and the bias correction being applied. In my opinion this may not be a sound assumption to make over a relatively remote region of the Sahara where there are far fewer observations constraining the ERA5 reanalysis. This means that there certainly is some degree of uncertainty in the ERA5 reanalysis data, but the authors do not explicitly address this uncertainty anywhere in the manuscript. What is needed is for the analysis to be expanded such that not just ERA5 is used as a "target", but also other atmospheric reanalyses (e.g., such as JRA-55 and/or MERRA2, to name a couple) are also evaluated. In doing so, results from multiple reanalyses can be compared and explicitly discussed to address this uncertainty and provide a means to talk about the greater robustness of the findings in general.

Without the inclusion of this expanded analysis the findings only have limited value because they are not placed in a broader context. Unfortunately, what I am suggesting above will likely result in a large reworking of the entire manuscript and will take some time to complete. Thus, for this reason I have recommended to reject and resubmit for this manuscript. I encourage the authors to do so because there is good potential to advance our weather prediction capabilities from a study such as this one. Below are some additional comments I had for the authors as they update their manuscript accordingly.

- Lines 21 – 23: sentence is worded awkwardly and can be misinterpreted. How it is currently written implies the only reason the SHL emerged as a key component of the WAM system is because of AMMA, which of course is not true. Suggest the authors update the text to better clarify that the AMMA project significantly highlighted the importance of the SHL in influencing variability of the WAM system.
- Line 34: Typo: Thorncroft and M. 1999 Need to fix this as you appear to be missing the second author's last name both here and in the references.
- Line 76 – 77: "very hot temperature" Can you provide a range of temperatures here to show what you mean comparable to what you did for RH later in the same sentence?
- Line 87 – 91: "… detected the SHL with occurrence of more than 70% during the boreal summer, …." Using what data? Daily? Hourly? Can you provide more information here about what you mean from all these prior studies that you presumably are taking the same regions? Likewise, you discuss detecting the SHL here, but you have not yet mentioned exactly how you plan to detect the SHL. What metric(s) are you using? I presume this is coming a little later, but maybe it should come first.
- Line 95: Is the daily temperature just for a specific level/levels? If so, which? Again – this is related to my other comment earlier that it may be better to explain how you intend to detect the SHL before the discussion in 2.1 and 2.2.
- Lines 132 – 135: What data was the Lavaysse (2015) using (certainly not ERA5), and have you confirmed that it is indeed valid for ERA5 and the MF7 and SEAS5? It would be helpful to convey this explicitly to build confidence in your methodology here.
- Lines 222 – 226: In Figure 1 and other figures with shading (Figs. 4, 6, 7) there is not enough contrast between the different color hues making it hard to visually interpret values from the figure. Thus, it is hard to evaluate how well SEAS5 and MF7 are doing compared to ERA5. Recommend the authors improve the figures by increasing the contrast between the color values used and possibly add line contours to label interval levels.
- Line 223 – 225: I don't understand what the authors mean by "coherent climatologies of the SHL over the Sahara". I think they mean that the SEAS5 and MF7 reasonably replicate the climatology of ERA5, but I am not certain. Please clarify.
- Line 225 – 226: It is unclear what is meant by "A progressive decrease in the intensity of the SHL is also observed over the North of Libya". MF7 does not appear to yield the relatively cooler temperatures over northeastern Africa that are shown in ERA5. Is this what is meant? In any case the authors need to clarify this comment better.
- Line 237 – 239: " … to get a robust selection of events at different periods." Can you explain more explicitly what is meant by robust selection here? Also – it would be helpful if the authors would explicitly mention with a sentence or two in the manuscript how the distributions change when the arbitrary threshold changes from 0.5 to 10.
- Line 240: " …. in all our products …." By products, do you mean seasonal model forecasts? Suggest clarifying to appeal more to readers less familiar with the seasonal forecasting lingo.
- Lines 277 – 279: This seems speculative. Given that you have all the output you could nail down whether or not this is what is happening.