EGUsphere, referee comment RC1
https://doi.org/10.5194/egusphere-2022-449-RC1, 2022

Comment on egusphere-2022-449

Anonymous Referee #1

Referee comment on "Seasonal forecasting skill for the High Mountain Asia region in the Goddard Earth Observing System" by Elias C. Massoud et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-449-RC1, 2022

Review of Massoud et al. entitled "Seasonal forecasting skill for the High Mountain Asia region in the Goddard Earth Observing System"

General comments:
This manuscript evaluates the seasonal forecast skill for hydrometeorology over the High Mountain Asia (HMA) region from the Goddard Earth Observing System (GEOS). As the author suggests, "S2S forecasting for HMA is in its infancy". Their results show that the GEOS-S2S system's ability to forecast HMA hydrometeorology on the seasonal timescale is limited. The authors raise some issues of the GEOS for seasonal hydrometeorological forecasts. These results help to improve the ability of the seasonal forecast model in the future. Therefore, the scientific questions are of interest. Their introduction provides context and objectives for their work, which catches the reader's interest. The data and methods are described in detail and are reasonable. The results of the evaluation and discussion are well written. In general, this paper is well prepared and fits the scope of ESD.

Specific comments:
1. The credibility of verification data is a great challenge. Even though the authors use multiple data, I still think the current results are quite uncertain due to credibility of verification data.
2. The S2S (subseasonal to seasonal) prediction project database (http://www.s2sprediction.net/) provides reforecasts by many operational forecast systems. Can the forecasting skills of the GEOS be compared with models that participate in the S2S prediction project?
3. It appears that the area of focus includes some low-elevation areas within the range of Figure 1 (e.g., parts of India). Are the authors calculating some statistics (e.g., Figure 2, Table 2) for the entire area of Figure 1? Should low-altitude areas be masked out?
4. Section 3 describes the results in detail. However, there seems to be a lack of an in-depth scientific explanation. For example, what are sources and effects of the forecast errors.
5. I noticed that the skills of GEOS vs. MERRA-2 and observation are quite different

(Figure 2a vs. 2b). The ubRMSEs in Figures 5d and 6d show the issue. Which result should I believe? Why are there such obvious differences in skill when using different verification data (especially SM, TWS)? How do the authors interpret such differences of results?
6. High anomaly correlation or low ubRMSE indicates better forecasting skills. Both the anomaly correlation and ubRMSE represent the correspondence between forecasts and observations. It looks like it is acceptable to use just one metrics. Why use both anomaly correlation and low ubRMSE?
7. Section 3.2 and Figure 4: It appears that the annual cycles have large uncertainties, mainly hydrological variables. The anomalies are derived by removing the annual cycle. This might greatly affect the credibility of the results. How does the author address this issue? There should be an explanation.
8. The reviewer did not get the point of Figure 3. This figure depicts the difference in skill between variables and between forecast lead times. Different variables have different predictability. Forecast skill decreases with forecast lead time as a matter of course. What is the purpose of comparing their relative skills?

Minor comments:
Line 23 and 25: "ranges" shoud be "range".
Line 118: "…five mountain ranges, including the Himalayas, Inner Tibetan Plateau, Karakoram, and Hindu Kush." Shoud be "four"?