I have reviewed the manuscript egusphere-2022-1307 “Propagation from meteorological to hydrological drought in the Horn of Africa using both standardized and threshold-based indices” which illustrates an assessment of drought propagation, from meteorological to soil moisture and hydrological drought, in the Horn of Africa by using well known approaches.

First of all I would congratulate with the authors on the very important research effort, given the large number of catchments studied and the peculiarity of the region, including the difficulties in gathering the data. As highlighted by the same authors in the introduction, the novelty aspect introduced is mainly represented by the case study which suffers, as in many other areas of the world, from a lack of information and data available for evaluation. However, there are several major (and minor) comments that should be addressed before the publication stage.

Major points to be addressed:

1) First of all, the question of the uncertainty relating to the data used should be addressed. Since the research work deals with modeled, re-analyzed and gridded data, coming from different temporal and spatial aggregation scales and from different models, each data has its own uncertainty and in their combined use it is not possible to verify this question, not even the different uncertainties can be compared each other. GLEAM for example uses MSWEP as input, but what can we say about GloFAS? What rainfall input does it use? If different from MSWEP, how can we compare the modeled flow data with the rainfall data of MSWEP? Another source of uncertainty is also probably due to the fact that the different standardized indices (SPI, SSMI, SSI) are calculated for each basin with reference to different probability laws. How this would have impacted the analysis?
2) I understand that the research idea is the propagation of drought, but perhaps a risk analysis (see Figure 2), which also showed the quantitative evaluation and the relative characteristics of the indices, also from a spatial point of view, would have helped to sort out the problem. For example, on line 443 it is said, if I understand correctly, that the drought indices used “fail to capture the water deficit amount…” how can we know this if we do not have an illustration of the quantitative assessment of the indices?
3) In general, the quality of preparation should be improved. Some sections of the manuscript were particularly difficult to read and sometimes repetitive. For example, section 3.3 says little more than 3.3 so maybe they could be merged. In general, the paragraph on methodology should be improved. In the same way, the presentation of figures (figure 5) and tables (table 1) in points of the body of the text where the analyses have not yet been inserted are misleading.
4) In general, even the figures should be improved in their quality and also in the formatting of the characters, which even when printed are too small for easy reading. I wonder why in figure 3 the same colours are always used except for panel (a) and the same for figure 6. Why don’t change them or just have always the same colour? In figure 3 and 6 there are no units of measurement. Furthermore, in table 1 it is not clear to me (not even from the caption) what is the difference between the two SPI-to-SSMI (months) columns. The two columns have the same header but different values.

Other minor points to be addressed:

1) Line 94: what is “level 6 boundaries of HydroBASINs?
2) Line 110: You probably selected the study region not because it is an “interesting” region but because, given the large number of catchments and relevant catchments attributes, it would have been suitable for the purpose of the study.
3) Line 116: “followed by the calculation of the indices”. Which indices?
4) Line 146: “three hourly temporal”, please clarify
5) Line 169: what “HAD” is?

6) Line 209: it is questionable the fact you selected 70th percentile because otherwise you have a too small number of events. The selection of the percentile deals with the severity of the drought events. Probably another message would have come from the analysis.
7) Line 215-218: it is not clear to me the explanation provided when P/Q is close to zero. This should happen when the duration of the meteorological drought is significantly smaller than the one for the hydrological drought, and this would happen when you have a small number of meteorological drought events or short meteorological drought events. Also I believe authors should better explain why they decided to use P/Q and P/SM ratio as indicators of drought propagation, what does this ratio means and why they did not use more convention drought propagation index (line 233 it is said that you did not consider the lag because in some previous study it is said that the largest correlation occurs at lag 0)
8) Line 242-243: reformulate
9) In my opinion figure 3 panel (b) and do not properly give the same message, much better in the case of streamflow (figure7).
10) Line 319: in my opinion it is improper to discuss about "precipitation variability" as you only considered the mean annual precipitation for each catchment and this is not a variability index for precipitation (also on line 425)
11) Paragraph 5.1 and 5.2 present different obvious sentences, not really of interest for the research (lines 451-456, …)