

## ***Interactive comment on “Identification and simulation of space-time variability of past hydrological drought events in the Limpopo river basin, Southern Africa” by P. Trambauer et al.***

**Anonymous Referee #1**

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### **General Comments**

This manuscript is a very interesting study that analyse the occurrence of severe droughts in the Limpopo river basin using a set of drought indicators as SPI, SPEI, ETDI, RSAI, SRI and GRI. Most of these indicators (except SPI and SPEI) were computed from the output of a finer resolution version of the hydrological model PCR-GLOBWB that was forced with ERA-Interim meteorological data. The authors shows the added value of using hydrological drought indicators jointly with meteorological indicators (SPI, SPEI) to best represent the main drought features in

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the basin. The objective is straightforward, the paper is well written and follows an adequate methodology. This research is relevant to scientific questions within the scope of HESS. Therefore, my overall recommendation is to publish the paper into the Journal after some minor revisions suggested below.

### **Specific Comments**

**Page 2640, Line 11:** what are the other meteorological variables used? In section 2.2 (P 2645, L25) the authors state that all the meteorological variables used were precipitation and 2m maximum and minimum temperature.

**Page 2642, Line 8:** Consider adding the word “accepted” or “used” after internationally if this is what you want to mean here.

**Page 2642, Line 13:** It is not necessary and quite redundant to define again in this line the meaning of SPI-3 as in the previous sentences (lines 10-11) was already defined.

**Page 2644, Line 20:** the abbreviations R and P were not defined previously. Either define or expand it.

**Page 2644, Line 26:** At the beginning of the section the average annual rainfall in the basin was defined as 530 mm/yr and in this sentence the same value is 506 mm/yr. This differences are due to different estimations or datasets used? Please explain or homogenize it.

**Page 2646, Lines 13-25:** Computation of Potential Evapo-Transpiration (PET): The authors state that PET was computed using the Hargreaves formula that is based in temperature alone. Then is stated that the ERAI data was obtained at a resolution

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of  $0.5^\circ$  for the entire continent. Why this data has a different resolution than the temperature dataset depicted before ( $0.7^\circ$ )? are PET data obtained directly from the ERA-I reanalysis or computed using other source of temperature data? Then it was re-interpolated to  $0.7^\circ$ ? Please provide more elements that can help to clarify this points. Moreover, afterward in section 3.2.1 the SPEI computations were performed using the Thornthwaite method. Do the authors think that using different estimations can lead to discrepancies in the derived indicators?

**Page 2645, L 16-17:** It's not clear how the indicators were "aggregated over several sub-basins". Which sub-basins? The sub-basins are not formally defined in the manuscript. In Figure 5 to 8 the captions are confusing as well; how the time series were aggregated over each station? or each sub-basin was named after the hydrometric station? This points should be clarified, please explain or rephrase.

**Section 4.2.1:** The results in this section are rather vague and qualitative. Agricultural drought is depicted here with two indicators (RSAI and ETDI). However Agricultural droughts are related with more than only physical aspects as economic factors that are related with impact indicators as well. Here, at least is possible to state that these indicators provides information for the assessment of agricultural drought. Also the statement that the indicators are able to reproduce the dry/wet conditions is at least too qualitative. Please provide more elements that can support this affirmations.

**Section 4.2.3:** Again here is not completely clear how the time series were aggregated. This should be clearly stated even if a sub basin is named after the station number. Also, this section could be benefited by a more quantitative analysis. For instance a correlation analysis that includes the different indicators and aggregation periods could help to support the main affirmations in the conclusions section.

**Page 2656, L24:** For the spatially aggregated time series the threshold that de-

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fines the drought severity changes depending on the size of the sub-basin (number of points to be averaged) but in any case it is not anymore comparable with the thresholds of a single point time series. The authors can still use any threshold but this is not anymore related with a probability of occurrence. This should be clear stated in the description.

**Page 2657, L19:** The authors states that the 2003-2004 drought was "quite mild when averaged over the whole basin" However I don't see any Figure or table that can support this affirmation. Then it's not clear if this affirmation is due to the spatial aggregation over the whole basin. Please either remove this comparison or add any supporting material.

**Page 2660, L 3-7:** I don't see any quantitative argument or even a discussion in how SPEI-3 can be a good option to replace ETDI and RSAI in absence of evaporation or soil moisture data. Even if it's a reasonably argument it should be evaluated in the manuscript. Section 4.2.1 or 4.2.3 will be benefited with a more quantitative analysis (like correlation analysis) between these indicators and the SPEI/SPI. Without this is hard to agree with this affirmation.

**Page 2660, L10-12:** Again, here I don't see any evidence in the paper that can lead to the conclusion that "GRI generally represents drought periods similar to SPI-24". From Figure 5 to 8 it's clear that the indicator is lower bounded and not reacting to dry conditions as the other indicators including SPI and SPEI-24. Either remove or provide more information that supports this affirmation.

#### Technical corrections

Figure 1: Expand the caption adding a reference of the sub-basins that are used in the analysis.

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Table 4. Add to the caption the variable evaluated. Runoff?

Glantz, 1988 (Page 2641) is listed as Glantz 1987 in the reference list. Please check and modify accordingly.

Consider removing the “a” after Trambauer et al., 2014a as there is only one reference for these authors and year.

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