Comment on nhess-2022-65

Anonymous Referee #2

Referee comment on "Interactions between precipitation, evapotranspiration and soil-moisture-based indices to characterize drought with high-resolution remote sensing and land-surface model data" by Jaime Gaona et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2022-65-RC2, 2022

The manuscript "***Interactions between precipitation, evapotranspiration and soil moisture-based indices to characterize drought with high-resolution remote sensing and land-surface model data***" by Gaona et al. submitted to NHESS-discussion an analysis of the atmosphere-soil-vegetation interaction, performed through a time correlation analysis among indices of precipitation anomalies (SPI computed at weekly scale), evapotranspiration deficit index (ETDI) and soil moisture deficit index (SMDI) for the period 2010-2017. ETDI e SMDI input data are provided from both remote sensing and from modelling approaches. The study area is the Ebro basin (Spain). The goal of the work is to get more insights into the drought propagation mechanisms.

The manuscript is within the scope of the Journal and potentially of interest for the readers of NHESS. However, I have some main concerns that prevent from publishing the manuscript in its present form. Here below my general comments:

- I found very interesting the adopted methodology. My main concern is on the use in the specific case study of standardized indexes. The time span analysed is 8 years. This means that whatever the adopted method for standardization, the statistical population is 8 (maximum). In the original work by Narasimhan and Srinivasan (2005) the ETDI and SMDI are computed on a dataset covering a time span of 70 years (1911-1980), making robust the statistical approach necessary to compute SPIn (fitting of the gamma or Pearson III distribution), ETDI and SMDI (setting the range of variation through the definition of the min and max values, as well as the median to compute the deviation). In my opinion the authors should wide the database extending the time span to 2021 in order to perform an uncertainty analysis on the robustness of the adopted statistical approach. For example, it would be interesting to study the variability of the fitting for SPI and of the min-med-max values necessary to compute the ETDI and SMDI by considering n subset of n-1 elements (12 subset of 11 y data if you consider the time span 2010-2021) and studying how the statistical metrics and the indexes themselves vary in relation to the subset. I know that it is a lot of work, but in my opinion, this is mandatory to ensure a sound and robust time lag analysis.

Therefore my concerns are not on the methodology adopted for the analysis of the relationships among the indexes, but on the indexes themselves.

- In my opinion, results on table 1 could be presented in a more effective way. I suggest to present four different correlation matrixes (2010-2017m, dry periods m, 2010-2017 w, dry periods w). Each matrix has on the rows [ETDI RS; SMDI RS; SPIm-1; SPIm-3; SPIm-6; SPIm-12] and on the columns [ETDI RS; ETDI LSM; SMDI RS; SMDI LSM]. A colour code to highlight the Pearson correlation, ranging [0,1] would help the readability of the tables, supporting the presentation of the outcomes.
- Figures 4-7. These are the core business of the work, but the outcomes did not convince me. I focus on the bars showing statistically significant correlations (blue or red coloured bars). It is clear that the fraction of the basin presenting high correlations) lasts approximately for a time span equal to the time scale of the SPI: more or less 4 weeks when I use SPI1, more or less 13 weeks when I use SPI12 and so on. I'm not convinced that this is not simply due to time autocorrelation of the pairs SPIn(t), SPIn(t+n) and not to real physical processes as proposed in the discussions. Please, clarify this point as it is very important
- Line 162 "In order to fill the gaps … interpolation". Please, specify the methodology adopted to interpolate and the maximum time span interpolated (this may strongly affect the results if the original time series is very fragmented, or the missing data interval are long)
- Equations 1 and 3. I would suggest to indicate the median with an overbar, avoiding MWS
- Equation 1. As written, the first equation is always positive and the second is always negative. Is it correct? Shouldn't it be the opposite?

To conclude, I'm a bit uncertain on my evaluation of the present manuscript. On the one hand, I appreciated the goal of the work, the general methodologies adopted to carry out and the rigorous presentation. On the other hand, I have serious concerns on the robustness of the computation of the indexes and on the real physical meaning of the founded time correlation . Due to these reasons, I suggest the editor to ask the authors for major revisions in order to them the possibility to convince me that I am wrong.