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Comment on nhess-2021-121

Anonymous Referee #1

Referee comment on "An approach to identify the best climate models for the assessment of climate change impacts on meteorological and hydrological droughts" by Antonio-Juan Collados-Lara et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-121-RC1>, 2021

GENERAL COMMENTS

The authors present an assessment of the implications of bias correction methods for the assessment of the effect of climate change impacts on hydrological drought in a Mediterranean catchment. They applied a well-known bias correction method and chose to evaluate the performance of each RCM based not only on conventional statistics, but also on drought statistics. The authors discuss methodological issues related to the comparison of the RCM performance through the application of a rainfall-runoff model at the monthly time scale.

The topic is relevant for the audience of Natural Hazards and Earth System Science, the objectives are properly identified, the methodology for the analysis is adequate and the conclusions are relevant and correctly supported by the results and discussion. The overall organization of the manuscript is adequate, and it is clearly written. The analysis clearly shows the agreements and discrepancies between results obtained with different climatic forcings for the hydrologic model of choice. Therefore, I support publication of the work in Natural Hazards and Earth System Science.

SPECIFIC COMMENTS

I have several suggestions and comments, which I believe would improve the paper:

- a) On section 4.2, the authors present their first assessment of discrepancy between historical observations and RCM control simulations. From Fig 4 and Fig 5, I gather that

most models do a poor job at reproducing observed climate in the case study basin, particularly in seasonality of rainfall and temperature. I suggest adding a table with a comparison of mean annual values of precipitation and temperature to provide an objective comparison.

- b) The application of the quantile mapping technique is a critical step in the analysis. However, the authors do not provide much information on the procedure or the results while applied to the case study. There is a very brief introduction in the methodology section, with no details on how the original series are transformed. Regarding results, we can only see that bias for the three basic statistics has been eliminated. I think the authors should provide more information on the application of the technique to the case study and illustrate it with at least a figure showing the quantiles.
- c) The authors chose to use SPI as drought index to characterize precipitation, but they should state the aggregation time step chosen in the analysis. The descriptive statistics used later in the paper (frequency, duration, magnitude, and intensity) should be formally introduced.
- d) I was a bit confused by the classification procedure. If I understood correctly, the RCM are assigned penalty values from 1 to 10 according to their ranking in each of 7 statistics. The final classification is obtained by averaging of the penalization for all statistics. However, the index chosen is divided by a normalizing value to allow comparisons across statistics. Why not directly use the index values instead of the penalties based on the ranking, to account for the relative deviations shown by each model?
- e) On section 4.3, line 195, the authors state that there is a "correlation" between the order classification of corrected RCMs for meteorology and hydrology. By looking at Figure 9, I am not sure of this and I am afraid I must disagree. Figure 9 shows a scatter plot of nine values. The fitted regression line for the nine points has an R^2 of 0.34, which is very low to conclude that there is a correlation (what is the significance level?). Even the blue line, which corresponds to only to 4 points, has a very low R^2 , of only 0.46. Finally, the authors should refrain from plotting the regression line for the two points corresponding to classification order <2 , which obviously renders a perfect fit because there are only two points. By looking at the figure, I can also see an opposite "correlation" for the 5 points corresponding to classification order >4 . The fitted regression line would have a negative slope, contradicting the initial statement. I think this discussion should be reformulated. We all agree that good bias correction would improve the agreement between climate models and observations, but the authors need to provide objective results to draw this conclusion, which, by the way, is a central part of their contribution. I suggest separating the analysis of conventional statistics and drought statistics, since the bias correction procedure is specifically focused on fitting the results of climate models to observations and therefore one can expect (as shown in Figure 4 and Figure 5), that the index values are very low. This does not necessarily have to be the case for drought statistics, which are linked to the tail of the distribution. Perhaps showing the scatter plots of the actual index values obtained with all models would illustrate better the comparison of performance for meteorological and hydrological drought.

TECHNICAL CORRECTIONS

From the formal standpoint, the paper is well written, correctly organized and adequately illustrated with tables and figures. I think the authors should rethink Figure 9 entirely.

The authors should consider changing the term "asymmetry" coefficient for "skew" coefficient.

Page 4, line 119. I believe the normalizing value used in the denominator of equation 1 is useful for comparisons across statistics, not across RCMs, because the normalizing value (historical observations) is the same for all RCMs.

Figures 8 and 13. Please change SPI into SSI, since the plots refer to streamflow droughts.

Although I am not a native English speaker, I believe the following expression should be corrected:

On page 4, line 114, ... applying the "following" error index.

On page 4, line 119, ... in order to make it comparable .

On page 6, line 174, ... when "they" compared different statistical techniques.