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Comment on nhess-2021-121

Anonymous Referee #2

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-RC2, 2021

GENERAL COMMENTS

This study aims to provide further insights on the selection of Global Climate Models (GCMs) -Regional Climate Models (RCMs) combinations, according not only to their skills to reproduce the local climate during the selected historical period but also the local hydrology. Concretely, the authors calculate an error index for basic and drought statistics and use it to classify the GCMs-RCMs combinations according to their reliability for the assessment of meteorological and hydrological impacts. The selected methodology involves the bias correction of climate models' outputs through a quantile mapping (QM) approach based on empirical quantiles, the use of a lumped rainfall-runoff model to simulate monthly inflows from climate data and the use of standardized indices for drought characterization (namely the Standardized Precipitation Index (SPI) and the Standardized Streamflow Index (SSI)).

In my opinion, the paper addresses an important issue on the use of climate models' outputs for the assessment of climate change impacts at the basin scale. Besides, it is properly written and well presented. However, I miss a more critical approach to the potential shortcomings of the selected methodology, such as the underlying assumption of stationary bias, the impact of bias correction on the tails of the distribution (e.g. induced changes on the original climate change signal of the climate models), the pros and cons of pre-processing and post-processing the variables derived from climatic ones with regard to bias correction (e.g. performance of the hydrological model simulations over the validation period) or the potential effects of neglecting the inter-variable dependence of climate variables (e.g use of univariate bias correction methods against multivariable ones or ignoring the role of temperature in drought onset) on the assessment of climate change impacts on water resources.

SPECIFIC COMMENTS

Lines 27-29: *"For instance, we have Palmer Drought Severity Index (...)"*. I will also mention the Standardized Precipitation Evapotranspiration Index (SPEI, Vicente-Serrano et al., 2010).

Vicente-Serrano S.M., Santiago Beguería, Juan I. López-Moreno, (2010) A Multi-scalar drought index sensitive to global warming: The Standardized Precipitation Evapotranspiration Index - SPEI. Journal of Climate 23: 1696-1718.

Lines 91-92: *"This is the reason that justifies the selection of quantile mapping (using empirical quantiles) for this study"*. Have the effects of inter-variable dependence been considered before selecting an univariate bias correction method? In the case of alpine catchments, Meyer et al. (2019) found that incorporating or ignoring inter-variable relationships between air temperature and precipitation data could impact the conclusions drawn in hydrological climate change impact studies.

Meyer, J., Kohn, I., Stahl, K., Hakala, K., Seibert, J., Cannon, A. J. (2019). Effects of univariate and multivariate bias correction on hydrological impact projections in alpine catchments. Hydrology and Earth System Sciences, 23, 3, 1339-1354, https://hess.copernicus.org/articles/23/1339/2019/

Lines 105-109: "*The meteorological drought analysis was developed by applying the Standard Precipitation index (SPI)"*. What about the role of temperature? As multiple authors have already pointed out, SPEI usually shows more severe increases in future drought events than those from SPI (e.g. García-Valdecasas Ojeda et al., 2021) and therefore I recommend to include it in the analysis. Which aggregation periods, statistical distributions and thresholds are considered for both the SPI and the SSI?

García-Valdecasas Ojeda, M., Gámiz-Fortis, S.R., Romero-Jiménez, E., Rosa-Cánovas, J.J., Yeste, P., Castro-Díez, Y., Esteban-Parra, M.J. (2021). Projected changes in the Iberian Peninsula drought characteristics, Science of The Total Environment, Volume 757, 143702, ISSN 0048-9697, https://doi.org/10.1016/j.scitotenv.2020.143702.

Lines 136-146: In my opinion, the climate and hydrological regime of the Cenajo basin should be properly characterized in the Case study section.

Line 149: "*CORDEX project (2013)*". Reference?

Lines 151-152: *"We also used official monthly natural streamflow data within the Cenajo basin for the historical period 1972 -2001 (adopted as reference)"*. This reference period is not consistent with the calibration period of the rainfall-runoff model (October

1971 to September 2007, line 157).

Line 153: *"(...) Spanish Ministry of Agriculture, food and environment"*. The competences of this former ministry have been assumed by the current Ministry for the Ecological Transition and the Demographic Challenge.

Lines 156-161: What is the validation period? Goodness of fit for the validation period?

Lines 178-180: *"The fit of the corrected control simulation series of streamflow to the historical series is not as good as for precipitation and temperature, but a remarkable improvement is observed"*. What could be the reasons for this? What about the performance over the validation period? (e.g. see Chen et al., 2021).

Chen, J., Arsenault, R., Brissette, F. P., & Zhang, S. (2021). Climate change impact studies: Should we bias correct climate model outputs or post-process impact model outputs? Water Resources Research, 57, e2020WR028638. https://doi.org/10.1029/2020WR028638

Line 189*: "Note that in this case we refer to the Standard Streamflow Index (SSI)"*. This index should be properly defined previously (in the Methodology section), along with an appropriate reference.

Lines 198-199: *"Therefore, we demonstrated that RCMs that allow better approximations of the meteorology provide better assessments of hydrological impacts"*. Although it seems quite straightforward (as rainfall-runoff models require climatic variables as inputs), I think that this statement should be carefully discussed before generalizing it: would it hold true if basins with very different hydrological regimes were considered? (e.g. important groundwater or snowmelt components?).

Lines 208-209: *"Both RCMs predicts a decrease of the variability in precipitation and an increase of the variability of temperature in the future"*. This is an interesting result, as precipitation variability is generally expected to increase in a climate change context (e.g. Pendergrass et al., 2017). Concretely, for the Mediterranean regions, Polade et al. (2017) concluded that a decrease in the frequency of daily precipitation events, combined with an increase in the amount of precipitation delivered in relatively rare heavy events, yielded greater year-to-year variability in total precipitation. In my opinion, this result should be discussed in the context of existing literature on future climate variability in the Mediterranean area. Which could be the potential role of bias correction in this result? For example, Maraun (2013) investigated the role of bias correction in modifying relative trends in annual precipitation maxima from a RCM and found that the RCM underestimated observed variability, which led to substantial amplification by quantile mapping of modeled trends in extremes. Besides, it would be interesting to examine the future trends obtained from the rest of the GCM/RCM combinations.

Maraun, D. (2013). Bias correction, quantile mapping, and downscaling: Revisiting the inflation issue. J. Climate,26,2137–2143, doi:10.1175/JCLI-D-12-00821.1

Pendergrass, A.G., Knutti, R., Lehner, F. et al. Precipitation variability increases in a warmer climate. Sci Rep 7, 17966 (2017). https://doi.org/10.1038/s41598-017-17966-y

Polade, S. D., Gershunov, A., Cayan, D. R., Dettinger, M. D., & Pierce, D. W. (2017). Precipitation in a warming world: Assessing projected hydro-climate changes in California and other Mediterranean climate regions. Scientific reports, 7(1), 10783. https://doi.org/10.1038/s41598-017-11285-y

Line 211: *"predict significant decreases of streamflow (-43.5 and 57.2%)"* Should it be -57.2%?

Lines 215-217: *"In the case of the meteorological droughts the first SPI threshold for which droughts periods are detected in the historical scenario is -3.0. In the future scenarios this value is -5.2 and -4.6 for the RCM2 and RCM9 respectively"*. I think that it will be interesting to assess the changes in the parameters of the future distribution with regard to the historical one (even if only the historical distribution is used to obtain the future SPI).

Lines 219-224: Check the signs of the SPI values.

Lines 231-257: in my opinion, the Discussion section does not address properly the limitations of the selected methodology (see my previous comments).