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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Multimodel assessment of climate change-induced hydrologic impacts for a Mediterranean catchment" *by* Enrica Perra et al.

Anonymous Referee #2

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GENERAL COMMENT:

The paper, through the analysis of the effects induced by climate change on a Mediterranean basin (Rio Mannu, Sardinia, Italy), describes a methodology of objective comparison among hydrological and climate models. Specifically, by means of the responses of five hydrological models calibrated and validated on the same basin, each one forced by outputs of four combinations of global and regional climate models, monthly values of hydrological quantities relative to discharge, soil water content and actual evapotranspiration are compared.

The topic is of significant interest and it agrees with the journal's editorial lines. The objectives presented in the paper are clear and according to the results obtained seem to be partially achieved. The results are interesting and contribute to clarify some

aspects of the uncertainty associated with both the hydrological and climate models. The paper is well organized and correctly written. Tables and figures are adequate, even though some figures (Figures 5, 10 and 11), although original, are not able to explain in detail (numerically) the differences between the pairs of models compared. References are complete and updated.

In light of the above considerations the paper can be accepted with minor revision for publication in Hydrology and Earth System Sciences.

SPECIFIC COMMENTS:

Introduction: while describing the state of the art, it would be appropriate, in addition to listing different sources of uncertainty, also highlight with appropriate bibliographic references that the uncertainty is greater in the climatic modeling of future scenarios rather than in hydrological models.

L.187: According to the sentence "those [models] exhibiting the best performance" it may be useful to provide some further explanations about the criteria involving the choice of climate models.

L.191: Although I am aware of the large amount of work done both in the calibration and validation of hydrological models and for what concerns the determination of the climatic scenarios, it is worth highlighting that the SRES scenarios used in the paper are now outdated and that perhaps it would have been more useful to refer to the new RCP scenarios. It would be appropriate to motivate this choice.

L.205: The authors use a bias coefficient "alpha" proposed by Duveiller et al. (2016), which is interesting from a statistical point of view, but in terms of graphic rendering it does not seem very readable, especially if the number of models is expected to increase. In this sense, Figures 5, 10 and 11 provide summary indications not allowing to appreciate differences, not necessarily macroscopic, between models. The use of tables could better integrate the information content of the aforesaid figures.

In the paper it would be useful a "Discussion" section dedicated to a detailed description of the causes of the main differences between the hydrological models, since they are only partially hinted at when results are introduced and at the end of Conclusions.

It would also be useful to evaluate such differences among the models also in the light of their performances compared to the observed data, which is not evident in the manuscript.

To this end, at least it is necessary to recall in detail the results related to the performances of the single models, not only reporting citations (ll. 114-115), among which there is a manuscript in preparation.

The Conclusions should be improved. For example, it is said (ll, 418-420) "CATHY, for instance, has the most detailed subsurface representation of the five models, and as such will tend to retain more water in subsurface storage, making some of this water available for subsequent evaporation ". Is it possible to achieve a more general conclusion from this statement? Is it possible to state only that a more detailed model increases the subsurface storage or one can infer that a more detailed model is more credible and therefore the forecast of increased subsurface storage is to be considered more likely? The same is true for models with a more detailed description of vegetation. This question can be answered only considering also the performances with respect to the observations (see previous point).

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