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Hydrology and Earth System Sciences
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Discussions

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

Anonymous Referee #1

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GENERAL COMMENTS The authors present an assessment of the impact of climate change on a Mediterranean catchment based on the comparison of basin response obtained from the combination of four climatic forcings and five hydrologic models. They focus on the analysis of monthly averages of variables linked to water availability, such as discharge, soil water content and actual evapotranspiration. The authors discuss methodological issues related to the objective comparison of the outputs from different rainfall-runoff models and present an application to the Rio Mannu basin in Sardinia.

The topic is relevant for the audience of Hydrology and Earth System Science, the objectives are clearly identified, the methodology for the analysis is adequate and the conclusions are relevant and correctly supported by the results and discussion. The paper is well organized and written The analysis clearly shows the agreements and

discrepancies between results obtained with different climatic forcings and hydrologic models. Therefore, I believe the paper deserves publication in Hydrology and Earth System Science.

SPECIFIC COMMENTS The authors are addressing a formidable task. They are reporting years of work under the constraints imposed by the length of a research paper. It is only natural that some parts of their work have necessarily been left unexplained. I am suggesting a few points where I believe the reader would benefit from some additional details, such as the following:

a) On page 6, lines 187-193 the authors introduce the climate models used in their analysis and later they specify the spatial and temporal resolution of the models (25 km, 24 h) and of the downscaled variables (5 km, 1 h). However, we do not know if the analyses presented on section 4.1 were carried out on the original model output at coarse resolution or on the downscaled variables. If the analyses were carried out directly on model output, model resolution (25 km) is similar to basin size, and the process of computing basin averages should be explained in better detail. If the analyses were carried out on the downscaled variables, I think a discussion of the possible influence of bias correction and downscaling on the results should be added.

b) I think model calibration also deserves additional discussion. On page 4, lines 112-114, the authors say: "The hydrologic models were independently calibrated and validated against observed data, with each modelling group using the type of data most suitable to that model, such as field-scale soil moisture, evapotranspiration patterns, and discharge". I have the impression that some of the differences observed in model behaviour, like the discrepancies in the monthly distribution of soil water content shown in Figure 8, may be explained by how the different models were calibrated. Perhaps the authors should consider a brief discussion of this issue.

c) The authors provide a reference to Duveiller et al., 2016 to introduce their bias coefficient "alpha". I found it to be a very interesting paper and thank the authors for calling my attention to it. From reading this paper, I gathered the impression that it was intended for comparison of large data sets. However, the authors chose to apply it only to monthly averages, although they had the full time series available for comparison. Perhaps they should explain the reasons for their decision.

d) Following on the same argument, I think Figure 3 would be more useful if it included examples of scatter plots corresponding the four cases shown. This would allow the reader to grasp the kind of agreement obtained in each of the four cases.

e) The shift in the seasonal distribution of actual evapotranspiration between SWAT and WASIM observed in Figure 9 and the rest of the models may also deserve additional discussion. Could it be due to limited water availability in the summer? May it be due to spring vegetation growth?

TECHNICAL CORRECTION From the formal standpoint, the paper is very well written, correctly organized and adequately illustrated with tables and figures. Interpretation of Figure 5 is handicapped by the fact that the upper row contains four cases for comparison and the lower row contains five cases. I would suggest resizing one of the two so that both rows plot on the same scale.

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