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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.

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We thank Reviewer 1 for her/his comments on our manuscript. In the following, the specific comments by Reviewer 1 are copied, followed by our replies to each point.

1) 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 analy-

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ses 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.

The analyses presented on section 4.1 were carried out using the downscaled and bias-corrected variables of precipitation and temperature, since we need accurate estimations of the hydrologic variables to run the hydrologic models. This information is now better conveyed in the revised manuscript (lines 257-258). With reference to the last suggestion, we agree on the importance of studying the effect of bias correction and downscaling on the results. This issue was examined for the Rio Mannu catchment by Piras et al. (2014), cited in the paper, and we are currently conducting a separate study on the effect of different downscaling techniques on the hydrologic cycle of another Sardinian basin.

2) 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.

We have added at the end of Section 3.1 further details on the calibration and validation procedures for the five hydrological models (lines 192-208).

3) 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.

We agree with the reviewer that it would have been interesting to apply these performance indices to the time series at their original resolution. However we are using climate model outputs, and the results must be averaged to have projections of climate variability. For this reason we decided to apply the Pearson and Duveiller coefficients using the monthly averages.

4) 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.

We have added the scatter plots in the revised Figure 3.

5) 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?

The SWAT and WASIM models anticipate the peak of actual evapotranspiration during spring months: this could be explained by the fact that these models incorporate also vegetation processes, and also by limited water availability in the summer. As we can see from Fig. 8c, SWAT and WASIM simulate very low soil water content during July and August. This point is now added in the revised manuscript (lines 349-351).

6) 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.

We have resized the lower panel to better interpret Figure 5.

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