

Response Letter

The value of satellite soil moisture and snow cover data for the transfer of hydrological model parameters to ungauged sites

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In the following document, we reproduce all the comments of the Referees in italic characters followed by our responses in blue.

Response to Referee #1

I did not find the corresponding modification to my previous comment (5). In my opinion, the model performance assessment in calibration and validation period is the base information for deep or further assessment. I would like to suggest the authors to present this information in the final version of the manuscript.

In response to this comment and the comment of the second reviewer, we have modified Figs. 2-4. We split the presentation of calibration and validation efficiencies into separate figures, plot the variability in the form of bars and, as suggested by the reviewer, we plot also the median of at site model efficiency for comparison.

Response to Referee #2

In this manuscript the authors analyze the potential value of constraining models with observations of multiple variables, i.e. stream flow, snow cover and soil moisture, for the spatial model transferability.

This is a highly relevant topic that has so far remained under-explored in literature. The experiment is well designed, follows a logical sequence and a sound, exhaustive methodological approach, thereby providing a comprehensive picture of the effect of the tested multi-variable calibration strategy on model transferability – i.e. >200 catchments, 11 different calibration weights for 8 regionalization techniques, further discretized into low- and upland regions as well as into model calibration and evaluation periods. The results are clearly described and documented. Altogether the analysis is impressively complete.

However, the authors could further strengthen their manuscript by more clearly emphasizing their overall objective in the description and discussion of the results but also in the Figures. In

its current state, the text and figures may give the reader the impression that the emphasis and novelty is on the 8 transfer methods, as also reflected in the reaction of one of the previous reviewers. I believe that this was not the intention of the authors and I think the manuscript would strongly benefit from re-directing the attention of the reader to the actual objective: the effect of multi-variable calibration on model transferability. To achieve this, I am convinced that this requires only some minor twists and rephrasing in how results are presented and discussed in the text/figures.

We would like to thank Prof. Hrachowitz for his positive and constructive evaluation of our paper. In response to this comment, we have tried to improve the presentation of the results.

Minor comments:

Tables 3, 4 and 5: I find these tables not really intuitive. Why is this information not added in Figures 2, 3 and 4, e.g. as error bars? This would make it much more convenient to appreciate this information.

Thanks. In response to this comment, we have revised Figs. 2-4 as suggested by the reviewer. To keep the clarity of presentation, we split the results of calibration and validation efficiencies. We present the variability of the efficiency in the form of bars and (as suggested by another reviewer) we plot in the figures also the median of at site calibration and validation efficiency.

Figures 5 and 6: the irregular steps in the color-scale are a bit confusing. It may be helpful for the reader to use a continuous color-scale.

In response to this comment, we have changed the colour scale (as suggested).

l.406: perhaps worth mentioning other studies that report similar conclusions. Here just a few examples from our group, but there quite some from other groups as well: Dembele et al., 2020; Hulsman et al., 2021 (please only see those as mere suggestions and do not feel obliged to cite them)

Thanks for recommendation and reference to a recent interesting study. One of the suggested references is already cited, the second was added as suggested.