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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Impact of model structure on flow simulation and hydrological realism: from lumped to semi-distributed approach" *by* Federico Garavaglia et al.

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The manuscript "Impact of model structure on flow simulation and hydrological realism: from lumped to semi-distributed approach" by Garavaglia et al., analyses the respective values of three different model formulations (2 lumped, 1 semi-distributed) for the prediction of snow dynamics and seasonal evaporation pattern.

I really liked the approach and enjoyed reading the manuscript, which is well-structured and, as far as I can tell, relying on a sound methodology. The overall topic is of high relevance, as it directly addresses the urgent need of the modelling community to improve their predictions (and thus their models), and as it picks up a much advocated and potentially fruitful way forward: the use of semi-distributed formulations together

with more efficient use of available data within multi-objective/multi-variable model calibration and post-calibration evaluation techniques. I would therefore be glad to see this work eventually published, but I would nevertheless strongly encourage the authors to address a couple of points which will strengthen the manuscript.

(1) The manuscript will benefit from being proof-read by a native English speaker to reduce the number of typos and language errors (grammar, syntax and use of specific words/terms)

(2) It will be of tremendous help for the reader if the author provided tables of

(a) the catchments used (including names, geographical positions, catchment areas, elevation range, slopes, annual P, annual potential E, annual Q, modelling time period, and time step

(b) the parameters of each model, the associated symbols, units, prior distributions (are these the same for all catchments?) and descriptions

(c) all model components (i.e. states and fluxes), including their symbols, dimensions and descriptions. This would make it much more convenient to follow the Appendix, in which many symbols are not clearly defined at this point.

If deemed suitable, these tables can be provided as Supplementary Material.

(3) Section 3.2.2 will benefit from a clearer description of the different criteria. For example, it remains unclear what is meant by "streamflow regime". I suppose it is the long-term seasonal pattern, but please make this more specific. Similarly, the cumulative distribution of flows is commonly referred to as flow-duration curve. A more consistent terminology will help the reader to better appreciate the manuscript. It is also not clear what is meant by 1st-lag flow derivative. Does this refer to the lag-1 autocorrelation? Of flows? Of the recession? Please elaborate!

(4) The post-calibration evaluation of the models with respect to snow and evaporation dynamics is an important point in this paper. Yet, no mention of this is made in section

3.2.2. How are MODIS data used to compare to model output? Spatial averages? What about the temporal resolution of the evaluation? Which performance metric was used? Some of this is mentioned later in the manuscript but I think this needs to be made clear in the methods section.

(5) Related to (4), I did not understand how a fractional snow cover can be reproduced with lumped model formulations (VO and V1). This makes clearly sense for a semi-distributed model (Mordor SD). But obviously I missed something for the lumped versions. Please clarify!

(6) What is the reason behind using KGE for calibration (which is completely fine) but NSE for evaluation? Why is not the same metric used for both?

(7) The presentation of the results and discussion section would strongly benefit from a bit more detail. Detailed results are only shown for a few catchments with good overall performance. And even for these, it remains unclear how the modelled hydrograph looks like (in comparison to the observed one) and what the values of the individual associated calibration objective functions (i.e. the 3 individual KGEs) and evaluation metrics (the remaining criteria) are. In addition, I think it would also be valuable to show examples of catchments where the model adaptation did not work and also discuss why.

(8) Related to (7), it is mentioned that V1 provides substantial improvements compared to V0. As V1 is changed in various respects in comparison to V0, it would be great if the authors invested a bit of effort to analyse and document which part/adjustment of V1 contributes most to the improvement.

(9) P.1,l.6: what is meant by “inflected”? Please rephrase.

(10) P.1,l.8: should read as “. . .evapotranspiration estimates. The model comparison is. . .”

(11) P.1,l.22: should read as “. . .semi-distributed. . .”

(12) P.1,l.23: Nijzink et al. (2016) would fit in nicely here

(13) P.1,l.23: what is meant by “To overpass hydrological singularity. . .”? Please rephrase.

(14) P.2,l.8: I may be worth referring to Hrachowitz et al. (2014) here.

(15) P.2,l.15: should read as “. . .framework on the MORDOR. . .”

(16) P.2,l.20-22: irrelevant. Can be condensed.

(17) P.2,l.26: should read as “. . .. . .mainly in the Alps (18 catchments), the Pyrenees (5 catchments) and the Massif Central. . .”

(18) P.2,l.29: should read as “. . .hydrological conditions. The average area of the study catchments is. . .”

(19) P.3,Table 1: not clear if the 22/17/19 parameters are all calibration parameters, as it seems in the Appendix that some of them are fixed. Please clarify.

(20) P.3,l.4: should read as “. . .1635 mm/yr. With regard to. . .”

(21) P.3,l.10: should read as “. . .sub-daily time steps. . .”

(22) P.3,l.11: what is meant by “ the shape of local gauges”? Please clarify.

(23) P.3,l.12ff: that is ok, but it should be underlined that these are not observations but modelled estimates which can be subject to considerable uncertainty.

(24) P.4,l.1-2: should read as “. . .for being affected by many. . .”

(25) P.4,l.5: should read as “. . .provides fractional snow cover. . .”

(26) P.4,l.5: please explain what “fractional snow cover” describes. Are these spatial fractions? If yes across the entire catchment? Across a pixel? Which value was used to compare the modelled values with?

(27) P.4,l.15: should read as “. . .interconnected storages.”

(28) P.4,l.15: what is meant by “continuously”? Please clarify.

(29) P.5,l.1: No, what is required is a *representative* estimate of areal precipitation. The mean (or any other measure of central tendency) will average out extremes, which will, due to the non-linear nature of your (or better: any meaningful hydrological model), result in biased results.

(30) P.5,l.19: “(ii) snow modelling have to be improved. . .” reads awkward. Please rephrase.

(31) P.5,l.31: should read as “. . .evapotranspiration, the model. . .”

(32) P.5,l.32: what is meant by “neutralized”??

(33) P.6,l.1: It is not clear which part of the system the ground-melt component represents. What exactly does it do? Please clarify.

(34) P.6,l.30: that is fine, but please specify if the gradients are set to fixed values or if they are calibrated (similar to rainfall multipliers). Where do the values (fixed or prior distributions) come from? Literature? Please provide references.

(35) P.7,l.2,section 3.2: I would suggest to rearrange this section for a better flow and to start with the calibration approach, followed by the split sample test and the post-evaluation criteria.

(36) P.7,l.5: does this mean that you end up with 2 parameter sets for each catchments? Is the following analysis then based on these 100 parameter sets (i.e. 2 for each catchment)? Please describe in more detail what you are doing.

(37) P.8,l.1-2: this resembles an approach described by Gharari et al. (2013). It would be good to refer to that paper.

(38) P.8,l.4ff: please clearly separate between criteria that are used for calibration (i.e. q, reg and qlc) and those used for post-calibration evaluation (i.e. etg, dq, snow cover, evaporation).

(39) P.8,l.17, eq.1: should this not read “KGEqcl”?

(40) P.8,l.20: “Numerous applications if this OF. . .” please provide references.

(41) P.8,l.29: Do the V1 and SD models in *all* catchments outperform V0 or is it just on average? Please provide some representative examples for both – cases of improvements and cases where V1 and SD did not result in improvements

(42) P.9,l.4: the improvement is obvious, but I struggle to see the “spectacular” improvement. In addition, “most” seems also a bit exaggerated here: reg, qcl and etg show only minor improvements, if any. Please tone the statement down a bit to actually reflect what we can see in the figures.

(43) P.10, Figure 5: are the NSE values the NSE values of the snow cover? Please clarify. In addition, please make sure that *all* figure captions in the manuscript are stand-alone, i.e. that the reader can fully understand a figure only by reading its caption.

(44) P.10,l.1: what is meant by “overpasses”? please rephrase.

(45) P.10,l.2: what is meant by “. . .the interest of the. . .”? please rephrase.

(46) P.11,figures 6,7: see (43)

(47) P.12,l.2: should read as “. . .that cannot be. . .”

Best regards, Markus Hrachowitz

References:

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The importance of system signatures, expert knowledge, and process complexity. Water Resources Research 50(9): 7445-7469.

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