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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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Detailed response to the comments of referee 2

We want to thank M. Hrachowitz for his accurate and helpful review of our manuscript. In this author comment, we list how each of the remarks provided by the referee was addressed. The comments made by the referee will be referred as RC and printed in bold; the authors' comments and answers as AC.

1 RC: 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).
AC: To answer your suggestion, the final manuscript will be checked by native English speaker.

2 RC: 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.
AC: We agree. We propose to add as Supplement Materials a specific section (S1) that presents more in details the dataset of the 50 catchments. Table S1 presents the main features of the catchments dataset, including name, geographical position, area, elevation range, slope, annual P, annual PET, annual Q, time step, modeling periods P1 and P2. Concerning model description and parameters we added a supplementary table in Appendix A which summarize MORDOR V1/SD free parameters, units, prior range (the same for all the catchments) and description. In addition we completed the description of model fluxes and states in Appendix A. Table 1 was also improved. On the other hand, concerning historical model version (MORDOR V0) we only added explicit references to existing publications which describe the model.

3 RC: 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!
AC: We agree. We reformulate section 3.2.2 as follows :
"The runoff signatures are viewed in such a way that streamflow data may be broken up into several samples, each of them a manifestation of catchment functioning (Euser et al., 2013; Hrachowitz et al., 2014; Westerberg and McMillan, 2015). Five different signatures are used in this study and are described in the following:

- time serie of flow is obviously the first signature which has to be reproduced by the model (hereafter called Q);
- long-term mean daily streamflow is used to focus on the capacity to reproduce seasonal variation of observations (hereafter called $Qsea$);
- flow duration curve focuses on the capacity to reproduce streamflow variance and extremes (hereafter called FDC);
- flow recessions during low flow period focuses on streamflow recessions (hereafter called $Qlow$);
- $lag-1$ streamflow variation is the last signature focusing on short term variability (hereafter called dQ and computed as follows: $dQ(t) = Q(t) - Q(t-1)$).

To go further, model realism is also evaluated in regards to three other hydrological variables: (i) fractional snow cover (FSC); (ii) snow water equivalent (SWE); (iii) actual evapotranspiration (ET).

However, observations available for these variables suffer from many limitations and uncertainties (see section 2). Consequently, a specific evaluation is conducted and is explained in sections 4.2 and 4.3."

4 RC: 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.
AC: We agree. We propose to add a specific comment in section 3.2.2 to mention the other hydrological variables, see response to comment 3 above. In addition we added more details in sections 4.2 and 4.3 to clarify how the satellite data (MOD10 and MOD16) are used.

5 RC: Related to (4), I did not understand how a fractional snow cover can be reproduced with lumped model formulations (V0 and V1). This makes clearly sense for a semi-distributed model (Mordor SD). But obviously I missed something for the lumped versions. Please clarify!
AC: For the lumped versions of the model, the fractional snow cover FSC estimates are based on a statistical formulation founded on the hypsometric curve. More in details, during accumulation the FSC is computed according to a monotonic crescent function as follows:

$$FSC(t) = 1 - \frac{\arctan \frac{\gamma(t) - fp1}{fp2} + fp3}{fp4} \quad (1)$$

where the parameters $fp1$, $fp2$, $fp3$ and $fp4$ vary from a catchment to an other as a function of the hypsometric curve and the orografic gradient. The state variable $\gamma(t)$

depends on the snow pack $S(t-1)$, its temperature $t_S(t-1)$, the snow $N(t)$ and its temperature $t_N(t)$ as follows :

$$\gamma(t) = \frac{S(t-1) \cdot t_S(t-1) + N(t) \cdot t_N(t)}{S(t-1) + N(t)} \quad (2)$$

For melt, $FSC(t)$ depends on the previous FSC and the evolution of the snow pack as follows :

$$FSC(t) = FSC(t-1) \cdot (\frac{S(t)}{S(t-1)})^{0.5} \quad (3)$$

6 RC: 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?
AC: We use the KGE for calibration because of its good statistical properties, which are helpfull for parameters identification. On the other hand, model evaluation is based on NSE because this criterion is commonly used for evaluation of hydrological models and is therefore suitable to use as a benchmark for this study. In addition, it allows to consider different metrics for calibration and posterior evaluation.

7 RC: 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.
AC: We propose to add as Supplement Materials two specific sections about the model comparison over calibration periods (S2) and over evaluation periods (S3). The section

S2 presents more in details the performance of MORDOR V0, V1 and SD over the calibration periods P1 and P2. For the three considered models, we show values of the individual associated calibration metrics (KGE(Q), KGE(Qsea) and KGE(FDC)) for all the catchments over the calibration periods P1 and P2. In addition we show, for each of the 50 catchments, the observed hydrographs and those modeled by MORDOR V0, V1 and SD (calibration mode). Similarly, section S3 presents more in details the performances of MORDOR V0, V1 and SD over the evaluation periods P1 and P2. For the three considered models, we show the values of the individual associated evaluation metrics (NSE(Q), NSE(Qsea), NSE(dQ), NSE(FDC) and NSE(Qlow)) for all the catchments over the evaluation periods P1 and P2. We show also, for each of the 50 catchments, the observed hydrographs and those modeled by MORDOR V0, V1 and SD (evaluation mode). Concerning snow and evapotranspiration processes, we extend the analysis to other catchments, with 8 nival catchments for FSC and 8 pluvial catchments for AET.

8 RC: 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 analyze and document which part/adjustment of V1 contributes most to the improvement.
Ac: Mordor V1 differs from V0 especially for water balance formulation and snow modelling. However is very difficult to trace the origin of the various improvements. Logically, for nival catchments the changes in snow modelling (and also the semi-distribution, see figure 4) are efficient. Concerning the water balance and so the improvement in the representation of evapotranspiration processes, we propose to add a specific comment in section 4.3 in order to analyze the origin of AET differences.

9 RC: P.1,l.6: what is meant by "inflected"? Please rephrase.
AC: Is has been changed in the revised manuscript.

10 RC: P.1,l.8: should read as "...evapotranspiration estimates. The model comparison is...."
AC: It has been changed in the revised manuscript.

11 RC: P.1,l.22: should read as "...semi-distributed..."
AC: It has been changed in the revised manuscript.

12 RC: P.1,l.23: Nijzink et al. 2016 would fit in nicely here.
AC: Good suggestion. We added the reference in the text.

13 RC: P.1,l.23: what is meant by "To overpass hydrological singularity...."? Please rephrase.
AC: It has been changed in the revised manuscript.

14 RC: P.2,l.8: I may be worth referring to Hrachowitz et al. (2014) here.
AC: Good suggestion. This paper is well suited to this context and we added the reference in the text both in introduction and in section 3.2.2.

15 RC: P.2,l.15: should read as "...framework on the MORDOR...."
AC: It has been changed in the revised manuscript.

16 RC: P.2,l.20-22: irrelevant. Can be condensed.
AC: We agree. We removed this paragraph.

17 RC: P.2,l.26: should read as "......mainly in the Alps (18 catchments), the

Pyrenees (5 catchments) and the Massif Central..."
AC: It has been changed in the revised manuscript.

18 RC: P.2,l.29: should read as "...hydrological conditions. The average area of the study catchments is..."
AC: It has been changed in the revised manuscript.

19 RC: 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.
AC: We tried to clarify this in Table 1. See comment to RC 2.

20 RC: P.3,l.4: should read as "...1635 mm/yr. With regard to...."
AC: It has been changed in the revised manuscript.

21 RC: P.3,l.10: should read as "...sub-daily time steps..."
AC: It has been changed in the revised manuscript.

22 RC: P.3,l.11: what is meant by " the shape of local gauges"? Please clarify.
AC: We agree. We propose a new formulation : "the hourly records of locals gauges are used to compute areal precipitation and temperature at 12-, 8- and 6-hours time step."

23 RC: P.3,l.12: that is ok, but it should be underlined that these are not observations but modelled estimates which can be subject to considerable uncertainty.
AC: We agree. We propose a new formulation: "It has be noticed that these data

are not observations but modelled estimates which can be subject to considerable uncertainty."

24 RC: P.4,l.1-2: should read as "...for being affected by many..."
AC: It has been changed in the revised manuscript.

25 RC: P.4,l.5: should read as "...provides fractional snow cover..."
AC: It has been changed in the revised manuscript.

26 RC: 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?
AC: The satellite MOD10 product provides gridded snow cover time-series. In this study we average the gridded values at catchment scale in order to compute a fractional snow cover. It has been rephrase in the revised manuscript.

27 RC: P.4,l.15: should read as "...interconnected storages."
AC: It has been changed in the revised manuscript.

28 RC: P.4,l.15: what is meant by "continuously"? Please clarify.
AC: With this phrase we want to emphasize that MORDOR is a continuous hydrological model and not a event-based model. We propose a new formulation: "Is is a continuous model that can be can be used with a time step ranging from hourly to daily."

29 RC: 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.
AC: We agree, the term "mean" is improper in this context. It has been changed in the revised manuscript.

30 RC: P.5,l.19: "(ii) snow modelling have to be improved..." reads awkward. Please rephrase.
AC: We rephrased as follows: "...representation of snow processes have to be improved..."

31 RC: P.5,l.31: should read as "...evapotranspiration, the model..."
AC: It has been changed in the revised manuscript.

32 RC: P.5,l.32: what is meant by "neutralized"??
AC: We propose a new formulation as follows: "(i) a surface interception: net rainfall and evapotranspiration capacity are calculated from the subtraction of MET from rainfall"

33 RC: P.6,l.11: It is not clear which part of the system the ground-melt component represents. What exactly does it do? Please clarify.
AC: The snow ground melt corresponds to the melting component coming from the ground heat flux, see for example (DeWalle, D. R., Rango, A.,2008). From experimental studies (e.g. Whitaker and Sugiyama, 2005), the ground-melt rates range form 0.5 to 1 mm/day.

34 RC: 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.
AC: The orographic gradients are calibrated with a uniform prior distribution whose upper and lower limits come from the climatic reanalysis used in this study (Gottardi et al., 2012). See the revised version of table 2 and appendix A.

35 RC: 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.
AC: We agree. Section 3.2 has been rearranged considering your suggestion. See response to points RC 3 and RC 4.

36 RC: 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.
AC: Yes we do that as explained in the text, see section 3.2.1. For each catchment we have 2 sets of parameters (θ_1 from P1 and θ_2 from P2) and all the results and performances are calculated from the 100 (2*50) simulations.

37 RC: P.8,l.1-2: this resembles an approach described by Gharari et al. (2013). It would be good to refer to that paper.
AC: Good suggestion. We added the reference in the text.

38 RC: 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).
AC: We agree. Section 3.2 has been rearranged considering your suggestion. See

response to RC 3 and RC 4.

39 RC: P.8,l.17, eq.1: should this not read "KGEqcl"?
AC: It has been changed in the revised manuscript.

40 RC: P.8,l.20: "Numerous applications if this OF..." please provide references.
AC: With numerous applications we refer to industrial studies made at EDF in an operational context. In spite of this, this OF is inspired from Paquet et al. (2013). We rephrased the paragraph in section 3.2.3.

41 RC: 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.
AC: According to section S3 of Supplement Materials (table S4 and S5), we can observe that for 73% of cases Mordor SD performs better than V0 in regard to streamflow signature. The Agout at La Raviege catchment (period P1) is a good example of important improvement of SD model. On the other hand for Ubaye at RocheRousse catchment Mordor V0 (period P1) performs better than SD.

42 RC: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.
AC: We agree. We qualified our statement as follows : "As a conclusion, the new formulation (V1) provides a significant improvement of performances, specially for q and dq signatures."

43 RC: 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.
AC: Yes. Captions of figures 5, 6,7 and 8 were completed in order to take into account your suggestion.

44 RC: P.10,l.1: what is meant by "overpasses"? please rephrase.
AC: We propose to change "overpasses" by "outclasses".

45 RC: P.10,l.2: what is meant by "...the interest of the..."? please rephrase.
AC: We propose to change "...the interest of the..." by "Therefore, the semi-distributed scheme clearly shows its added value for nival catchments.

46 RC: P.11,figures 6,7: see (43)
AC: See comments above.

47 RC: P.12,l.2: should read as "...that cannot be..."
AC: It has been changed in the revised manuscript.

References
DeWalle, D. R., Rango, A. (2008). Principles of snow hydrology. Cambridge University Press.

Whitaker, A. C., Sugiyama, H. (2005). Seasonal snowpack dynamics and runoff in a cool temperate forest: lysimeter experiment in Niigata, Japan. Hydrological Processes,

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