Hydrology and Earth System Sciences

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Discussions

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Interactive
comment

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 1

We want to thank referee 1 (M. Herrnegger) 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.

Concerning general comments:

1 RC: The authors show interesting and relevant simulation results that are in good agreement with data sets not used in calibration (FSC, SWE, ETA). These detailed results are however only presented for 6 out of 50 catchments. It is clear that (i) snow is not relevant in all catchment, (ii) availability of SWE measurements may be limited and (iii) that the ETA estimates from MOD16 are unreliable in alpine regions. It would however be of interest to see results of the model performance for more catchments in this context.

AC: We agree. We propose to extend the results to 8 catchments for FSC (a compromise between nival behaviour and data availability). For AET, we also propose to extend the results to 8 catchments (a compromise between pluvial behaviour and data availability). Unfortunately for SWE, we are not able to present other results due to data availability of at site snow gauges.

2 RC: The paper reports on different versions of the hydrological model MOR-DOR. It would therefore be important for more clarity to add two overview tables, containing (i) model parameters, units, range of parameter values and description and (ii) model fluxes and states, also including units and a description of the variables.

AC: To address this issue we added a supplementary table in Appendix A which summarize MORDOR V1/SD free parameters, units, prior range 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: MORDOR v1 and SD include a modified and improved snow routine. How is snow sublimation considered in the models?

AC: Snow sublimation is not taken into account in the models. Although it can be a significant process at local scale, it is considered to be of second-order at catchment scale in our regions. See for example Strasser et al. (2008).

4 RC: From the manuscript it is unclear, what temperature data is used
AC: The temperature data used in the study are gridded and provided by (Gottardi et al. 2012). These data result from a statistical reanalysis based on ground network data and weather patterns (Garavaglia et al. 2010). They are available for the 1948-2012 period at 1-km^2 / 1-day resolution. See P3L3-6.

5 RC: How is PET / ET0 estimated? How large are the differences between the PET values in version v0 compared to version v1/SD? This is crucial since it will influence the AET results.
AC: PET is estimated differently for the three model structures.
For V0, PET is calculated from a statistical formulation driven by air temperature T, as follows:

$$PET = a.(T - b)^2$$

with a and b two free parameters which are calibrated with the other model parameters. For V1/SD, PET may be estimated by any PET formula. In this study, we use the Oudin formulation, which is expressed as follows:

$$PET = 0.408.Rpot.\frac{T + 5}{100}$$

with $Rpot$ the potential solar radiation ($MJ.m^{-2}.d^{-1}$) and T the air temperature (C). On the study catchments, Oudin PET vary from about $420\ mm.yr^{-1}$ to $890\ mm.yr^{-1}$. On the other hand, MORDOR V0 calibrated PET vary from about $220\ mm.yr^{-1}$ to $1750\ mm.yr^{-1}$.
We agree with the reviewer, these differences in the PET estimates obviously have a

great impact on AET results. We added a specific comment in section 4.3.

6 RC: What is the reason to use KGE in the objective function and NSE for evaluation?
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 authors state that the model runs at different temporal resolution. Are the calibrated parameters comparable between the different temporal resolutions?
AC: Most parameters do not depend on temporal model resolution. However some parameters like the unit hydrograph parameters and the parameters used in the L storage outflow equation remain dependent on the temporal resolution. Concerning the model calibration process, we use a wide prior range for parameters values which is relevant for both daily and sub-daily time steps.

8 RC: In the Appendix the model formulations are given. Here it would also be interesting to give some technical details on the models: In what language are they written? Is there an internal time discretisation implemented? How long does a run take?
AC: We propose to add this paragraph in the Appendix : "The MORDOR SD model is written in FORTRAN 90. The model runs at different temporal resolution. The duration of a simple model simulation (i.e. model run and evaluation criteria computation) is about 1 sec and depends on the time step and on the length of time series. For instance a daily simulation over 50 years takes less than 1 sec and an hourly

simulation over 10 years takes about 2 sec. Concerning the calibration process (about 40.000 model runs), the algorithm takes about 10 min for a daily time step over 50 years and about 45 min for an hourly time step over 10 years. Post-processing and graphical tools are developed in R language."

Concerning specific comments:

RC: P1L22: semiâĂŘdistributed
AC: It has been changed in the revised manuscript.

RC: P2L1: Most studies
AC: It has been changed in the revised manuscript.

RC: P2L18âĂŘ19: It is unclear why the two best solutions are selected (I presume the presented MORDOR v1 and SD) and why these three new formulations are then compared with the historical version.
AC: "Three" has been replaced with "two" in the revised manuscript.

RC: P2L23: . . .in quality and length of available records.
AC: It has been changed in the revised manuscript.

RC: The number of free parameters are not underlined in the table.
AC: We propose a new redaction of the legend of Table 1 : ". . . For each module and model, the number of free parameters is given."

RC: Table 1: Could you please clarify what is to be understood under "adjusted

PET from a statistical formulation driven by temperature"?
AC: See response to RC 5.

RC: Figure 1: It would be good to highlight and name the catchments shown in detail in the analysis.
AC: We propose to provide as Supplementary Material : (i) a table of the main features of the catchments used, including name, geographical position (i.e. coordinates of the outlet), catchment area, elevation range, slope, annual P, annual PET, annual Q, time step, modelling periods P1 and P2; (ii) a specific figure with the catchments location.

RC: P5L23: Why not call the section 3.1.2 simply MORDOR V1
AC: We propose to homogenize the three sections titles as follows:
3.1.1. MORDOR V0: Initial lumped formulation
3.1.2. MORDOR V1: Revised lumped formulation
3.1.3. MORDOR SD: Semi-distributed formulation

RC: P5L24: revised model formulation
AC: It has been changed in the revised manuscript.

RC: P6L18: … timeâĂŘstep based on air temperature.
AC: It has been changed in the revised manuscript.

RC: Figure 2: A subdivision in 10 elevation zones is shown. Is this the standard number of zones used for spatial discretisation, also for the other catchments?
AC: No. In this study, the number of elevation zones depends on the hypsometric curve of the catchment according to the following criteria: (i) the relative area of each elevation zone has to be greater or equal to 5% and less or equal to 50%, (ii) the

elevation range of each zone has to be lower than 350m.
We propose to add this comment in section 3.1.3.

RC: P8L7: The term "streamflow interannual daily regime" is not easily understandable.
AC: We agree with your comment, so we propose a new formulation for the 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. 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: $dQ(t) = Q(t) - Q(t-1)$).

RC: P8L8: Do you mean the flow duration curve with "streamflow empirical cumulative distribution"?

AC: Yes. See comment above.

RC: P8L9: How are the streamflow recession periods defined in practice?
AC: Streamflow recessions sequences are extracted from the low flow period for each catchment. We only select recessions with a minimum duration of 7 days.

RC: P8L10: What is meant with the 1 st âĂŘlag streamflow derivates and how are they calculated?
AC: Yes. See comment above.

RC: P8L11: . . . **performance results are resumed using**. . . **What is meant with resumed?**
AC: We replace "resumed" by "summarized"

RC: P8L27: (i.e. 100 simulations)
AC: It has been changed in the revised manuscript.

RC: P9L8: in figure 4
AC: It has been changed in the revised manuscript.

RC: Figure 4: Legend is missing
AC: We added the legend in figure 4.

RC: Figure 7: Why is R^2 shown and not NSE (as in other plots)?
AC: Your remark is relevant. We therefore change the figure criteria, with NSE instead of R^2. The conclusion remains the same.

P12L7: (see Figure 2?)
AC: It has been changed in the revised manuscript.

RC: P12L20: What is the second order impact? Not being important?
AC: We replaced by "... has a second order effect".

Concerning Appendix A:

RC: Additional tables as mentioned above showing parameters, fluxes and states would very much improve clarity when reading the appendix. It is very frequently unclear what the used variables mean. Figure 10 does not help very much in this context.
AC: See response to comment RC 2.

RC: P13L31: What is meant with "flow length of each gridcell to the outlet", since we are talking about zones?
AC: The $\bar{fl}$ parameter is the average of the flow length of each DEM pixel to the outlet. This parameter is used in the routing function (diffusive wave equation, see A.6) that remains lumped, i.e. there is no relation between $\bar{fl}$ and the elevation zones.

RC: Eq. A1 & A2: Please check equations. They seem to be erroneous.
AC: We checked and we corrected equation A2.

RC: Eq. A3: Why ET0 and not PET? This is not consistent with the other parts of the manuscript.

AC: We agree. We propose to replace ET0, which has a very precise acceptation, by PET.

RC: Eq. A4: Should Kc and Rpot not have a time component?
AC: We agree. We changed equation A4 and A5.

RC: Eq. A5: It is unclear, for what A5 is needed.
AC: A5 equation is needed to have a mean value of K_C equal to 1, whatever the $kmin$ parameter value. In fact, $kmin$ is a shape parameter representing the seasonal variability of the vegetation, and we want to reduce the sensitivity of the evapotranspiration amount to $kmin$.

RC: Eq. A11: Kf : Is the time component missing? It is unclear, what the difference between kf and kfp is.
AC: K_f is a time variable coefficient. We therefore modified equations A10 and A11. We also added a specific comment to clarify k_f and kf_p terms.

RC: Eq. A14: From the equations it is unclear, how ui(t) is calculated.
AC: As illustrated on Figure A2, $u_i(t)$ is calculated as follows :

$$u_i(t) = u_i(t-1) + in_{U,i}(t) - evu_i(t)$$

RC: Eq. A21: From the equations it is unclear, how zi(t) is calculated.
AC: As illustrated on Figure A2, $z_i(t)$ is calculated as follows :

$$z_i(t) = z_i(t-1) + in_{z,i}(t) - evz_i(t)$$

RC: Eq. 23/25: What is si ?

AC: s_i is the relative area of the zone i, see P14L1

RC: Eq. 24: What is k_n ?
AC: The parameter k_n is the outflow coefficient of the deep storage N. We added a specific comment in Appendix A.

RC: Eq. 26: What is fl?
AC: fl is the mean of flowlength of each gridcell to the outlet [km], see P13L31 and comment above.

References
Strasser, U., Bernhardt, M., Weber, M., Liston, G. E., Mauser, W. (2008). Is snow sublimation important in the alpine water balance?. The Cryosphere, 2(1), 53-66.