Answers to the review of HESSD Manuscript
'Continuum: a distributed hydrological model for water management and flood forecasting'
By F. Silvestro, S. Gabellani, F. Delogu, R. Rudari, and G. Boni,

Dear Reviewers dear Editor,

In the following we report the responses to the reviewer 1. Some issues are discussed also in the responses to the other reviewer.

We thank both reviewers for their accurate and frank review and used their precious suggestions to improve the paper. We tried to answer to all the comments made and we are ready to prepare and submit a new version of the manuscript.

The point by point answers are written in italics.

With our Best Regards,

Francesco Silvestro
Simone Gabellani
Fabio Delogu
Roberto Rudari
Giorgio Boni
Reviewer #1:

Scientific significance
The authors present a new conceptual hydrological model, 'Continuum'. However, the components of the model (spatial discretization, processes considered and process representations) are not new, and many similar conceptual distributed models already exist. To name just a few: Larsim, HBV, MHM, WASIM-ETH. Also, the parameter estimation and calibration strategy are not novel. A notable exception is the sophisticated representation of evaporation which allows for model-based estimates of LST. Comparing them to satellite derived LST estimates offers an addition (apart from streamflow) to the calibration objective function and hence has the potential to constrain the search space for calibration parameters. In order to make the manuscript acceptable for publication, the authors should focus on this interesting topic and investigate the additional value of LST data in the model calibration process (e.g. by comparing parameter sets obtained from streamgauge based optimization vs. streamflow + LST based calibration and discuss the effect on model equifinality).

AND

In the introduction, the authors give an overview on the history and state-of-the art of hydrological modeling. What is presented is mainly an overview on conceptual modeling, but not on reductionist physically based model such as MIKE SHE, HYDRUS, InHM etc. This makes sense if the scale of interest of the authors is known, i.e. basins of several hundred km$^2$, which preclude the use of such 'data-greedy' models. To make this point more clear to the reader, I recommend to insert a discussion of space/time scales of interest and appropriate model concepts and explain where in this Continuum has its place.

We put these two comments together as the combination of the two suggestions shed a new light on the potential of the work and we are thankful to the reviewer for that. We agree in general with the reviewer comment regarding the need of highlighting the novel contribution of the work. The suggestion of emphasizing the use of LST could make the difference in terms of scientific novelty brought by the paper to the scientific community. This is a common statement from both reviewers. On the other hand, a better clarification of the scales addressed by this work could help in shrinking a bit the list of models that could be taken as reference for Continuum and in bringing up the specific features of the model. Following these suggestions we revised the introduction clarifying that the model is conceived for small ($O(A)=10^0-10^1$ km$^2$) to medium size ($O(A)=10^3$ km$^2$) basins. Normally these scales are modelled using data greedy models that are mentioned by the reviewer as well. Some models belonging to the same typology of Continuum have been applied on basins larger than several hundred km$^2$, but the data demand to have a good implementation is usually very high. A breakthrough would be represented by possibly calibrating the model only on the basis of LST without streamflow data and using a small subset of ancillary data such as land cover infos derivable from optical satellite imagery. Along these lines in the new version of the manuscript we used soil and geomorphologic information to calibrate part of the model parameters and LST to calibrate the remaining ones (i.e., the ones that are more sensitive to it) These two parameters are $c_t$ and $c_f$ since they are the ones determining the soil moisture distribution as represented in the model, by construction soil moisture is the state variable that has the most influence on LST simulations. The results are then compared with those obtained by the calibration using classical streamflow data.

When the authors talk about the energy balance (e.g. p. 7640 l. 14-15, p7644 l. 15, p. 7652 l. 13 etc.) they should be more precise for which system (boundaries) and which energy forms (geopotential, pressure, temperature, etc.) the energy balance is closed (as it is not for the entire system under consideration).
The force-restore equation, used in continuum, describes the energy budget including net longwave and shortwave radiation, \( R_n \), latent heat \( LE \) and sensible heat \( H \). The budget is closed by the heat propagated by diffusion towards the deep layers of the soil \( G \).

The equation is related to the mass balance of the root zone through the term of latent heat transformed into evapotranspiration dividing by the specific heat of vaporization per unit volume \( \rho_w \lambda \), where \( \rho_w \) is the water density (see next answer for further details about this coupling).

Theoretically the control volume to which the balance is applied is then the unit area bounded vertically by the surface of the soil and the top of the canopy, assuming the thermal capacity of this volume negligible. We also neglect the horizontal energy fluxes (see figure below). In practice, the volume is extended to the unit cell of the numerical scheme used.

As a consequence the net radiation budget is done at the top of the canopy. The values of the atmospheric variables \( T_a, U \) and \( \theta_a \) are referred to a reference elevation above the canopy. The LST is assumed representative of the skin temperature at the top of the canopy.

We know that this assumption is questionable, but to take account of the diffusion through the canopy to the surface of the soil would have to adopt schemes of energy balance more complex, such as the one presented in Caparrini et al. (2003).

We therefore believe that the approximation is a fair trade-off between parsimony in parameterization and accuracy in the description of the processes. The acceptability of the approximation is supported by the results shown by Boni et al, (2001a, b) and Sini et al. (2008).

A more detailed description of this scheme has been included in the paper (par. 2.5)

From eq. (22) to (27) it is not clear to me – how the link between soil moisture in the energy-balance equations and the state variables of the hydrological model components is achieved. More specifically the link between \( C_{soil}, K_{soil} \) (eq. 24), \( \beta_f \) (eq. 26) and hydrological soil moisture. This should be stated more clearly as it forms the link to insert LST observations in the model calibration process.

In the new version of the manuscript we tried to better clarify such links and specifically the beta function (see e.g. Dingman, 2002) \( (\beta_f) \) used to link the saturation degree, represented by the ratio \( V(t)/V_{max} \), actual and potential evapotranspiration. That clarifies the link between soil moisture, LST and evapotranspiration. The thermal inertia \( (K_{soil}) \) and its heat capacity \( (C_{soil}) \) are related to the saturation degree by the relationships described in Peters et al (1998), in the following we report the main equations:

\[
C_{sat} = (1-k) \cdot C_{satur} + K \cdot \theta_{sat} \cdot C_{green} \\
K_{sat} = K' \cdot (K_{sat(t)} - K_{satur}) + K_{satur}
\]
\[
K_e = \begin{cases} \frac{\log_{10}(\theta_{ds})}{0.1} + 1 & \text{if } \theta_{ds} \geq 0.1 \\ \frac{\log_{10}(0.1)}{0.1} + 1 & \text{if } \theta_{ds} < 0.1 \end{cases}
\]

Where \( n \) is soil porosity and \( K_e \) is the Kersten number: a function of the saturation degree \( \theta_{ds} \), in the paper \( V(t)/V_{\text{max}} \).

This parametrization is relatively robust in terms of the values chosen to represent the soil characteristics, while it is fairly sensitive to the saturation degree.

We did not report all these in the paper to avoid lengthening it excessively preferring to provide the key references to the reader.

The basic concept to highlight is that LST depends first on the latent heat flux and as a second model dependency on thermal inertia and heat capacity, all these terms are in turn related to the soil moisture (model state variable). In the text we explicitly added the fact that both \( C_{\text{soil}} \) and \( K_{\text{soil}} \) increase when soil moisture increases. All these dependencies lead to the fact that high values of soil moisture reduce the daily variability of LST and determine its value in a fairly complex non linear way.

– how evaporation from the interception storage is calculated.

The evaporation from the retention storage is calculated as a diagnostic variable. First the total evapotranspiration \( ET_i \) is derived for the i-th timestep by the force-restore equation, then it is distributed according to the relation

\[ ET(i) = \text{Retention}(i-1) + V(i-1) - V(i) \]

(if \( ET(i) > \text{Retention}(i-1) \))

This is a negligible approximation with respect to the bulk formulation at cell size.

– How net radiation \( R_n \) (eq. 22, 23) is calculated. Or is this taken from observations?

Short Wave component is derived from radiometer observations (paragraph 3.2)., when the density of observations is appropriate (as is the case in this paper), otherwise it is estimated by combining extraterrestrial component (computed on the basis of Dozier et al., 1990) regarding the terrain parameters and Yang et al, (2001 and 2006) regarding solar radiation ) attenuated using meteorological variables and cloud cover (including MSG Cloud cover) and bias-adjusted (depending on cloud cover footprint) with meteorological ground stations observations when available

Long Wave components are rarely available from observations and they are therefore estimated using Stephan-Boltzmann’s law as a function of air temperature and humidity.

The authors use 6-month periods for calibration and validation (plus 6 months of model warm-up). These periods contain a few strong rainfall-runoff events (less than 10). These periods are in my eyes too short to obtain stable model parameters and meaningful model performance statistics. Also, the model’s capability to correctly simulate inter-annual dynamics cannot be evaluated on 6-month periods. The authors indicate that estimating initial conditions especially for the baseflow storage was difficult: This problem could be reduced by multiannual model runs.

The use of 5 months of model warm up for calibration was chosen after several attempts in order to obtain negligible influence of initial conditions on streamflow simulations that are in the end used for validation. This period depends on the catchment characteristics and the model used, and in this case it proved to be sufficient for the presented application. The validation and calibration period contain important pluviometric events that in the opinion of the authors test the model performances in a variety of conditions, so that a good grip on model performances can be derived. The fact that the calibration parameters give good results also for validation period is encouraging about their stability.

Similar (in terms of length) periods have been used in other papers published on HESS to test model performances of continuous hydrological models calibration and validation: as an example in “Liu & Todini (2002), Towards a comprehensive physically-based rainfall-runoff model. ), Hydrology and Earth System Sciences, 6(5), 859–881”.
As stated by the reviewer multi-annual model runs certainly help to solve both the problem of initialization and of parameter stability and would enrich the analysis of the model capability to capture interannual variability.

The availability of observations in the catchment is however limited and the two periods selected are the ones that have a negligible contribution from the snow cycle, which should otherwise be considered and modelled (such module is already developed, but it would introduce an increased level of complexity moving the reader away from the message intended in this work). We would retain the comment as a precious suggestion for future work.

Anyway, in the reviewed version, we will better address this problem explaining what led to the final choice of calibration and validation periods as well as the consequent limitations.

In the new version of the paper, in the Conclusions, we will highlight this fact in order to avoid misunderstandings and to evidence that further work is needed on this topic. Specifically, We will introduce the following sentence:

"Further work is needed to introduce the modeling of the snow cover evolution and of the snow-melting in order to carry out multi-annual simulations"

Page 7660/line1-7: Most, if not all conceptual models allow for a mapping of parameters to components of the hydrograph (base flow, flood rise, recession, etc.) and there is extensive literature on the topic of hydrograph-component specific model parameter estimation (e.g. Reusser, D. E., Blume, T., Schaefl, B., and Zehe, E.: Analysing the temporal dynamics of model performance for hydrological models, Hydrology and Earth System Sciences, 13, 999-1018, 2009.

Yes, we inserted the suggested reference as representative of the literature of the topic. We changed the sentence as follows.

Old version:
"...Because of its internal structure it is possible to map different processes..."

The new version:
"...Because of its internal structure, similarly to other hydrological models, it is possible to map different processes (Reusser et al., 2009)..."

A few minor points (leading number indicates page/line):
7656/3-13: Please name the basin size
We inserted the drainage area at the confluence with Bormida river

7657/16-20: What are typical ranges for uh and uc?
We added a table with the range of variation of the parameters that have been defined basing on a prior knowledge of the parameter significance and on their mathematical and physical range of validity.

<table>
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<th>Max</th>
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<tr>
<td>R_f</td>
<td>[-]</td>
<td>0.5</td>
<td>50</td>
</tr>
</tbody>
</table>

References:

Boni, G., D. Entekhabi, and F. Castelli, 2001b: Land data assimilation with satellite measurements for the estimation of surface energy balance components and surface control on evaporation, Water Resources Research, 37(6), 1713-1722

Caparrini, F., F. Castelli, and D. Entekhabi, 2003: Mapping of land-atmosphere heat fluxes and surface parameters with remote sensing data, Boundary-Layer Meteorology, 17(3), 605-633


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