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 Authors, dear Editor,
I have reviewed the aforementioned work. My conclusions and comments are as follows:

1. Scope

The work is well within the scope of HESS.

2. Summary

The authors present the new conceptual, distributed hydrological model 'Continuum'. It contains conceptual yet physically based representations of interception, overland flow, channel flow, infiltration, subsurface flow, percolation to groundwater and groundwater flow. Evaporation is calculated explicitly with an energy balance approach. Based on this, the additional prognostic variable land surface temperature (LST) is calculated. The model contains comparably few calibration parameters. The model is applied to the Orba catchment in Northern Italy, calibration and validation results are shown and discussed for a 6-month period each. Model performance is assessed by comparing modeled and observed streamflow of two gauging stations and by comparing modeled and satellite-derived LST estimates. The model is found to be performing acceptably. Further, the authors point to the possibility to constrain parameter space during model calibration if LST is used as addition to the model objective function.

3. Overall ranking

The work is ranked **'Major revision'**. This is due to some aspects of scientific quality as explained below.

4. General evaluation

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).

Scientific quality

- 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 km², 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.
- 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).
- From eq. (22) to (27) it is not clear to me
  - how the link between soilmoisture in the energy-balance equations and the state variables of the hydrological model components is achieved. More specifically the link between Csoil, Ksoil (eq. 24), betaf (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
  - how evaporation from the interception storage is calculated.
  - How net radiation Rn (eq. 22, 23) is calculated. Or is this taken from observations?
- 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.
- 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., Schaefli, B., and Zehe, E.: Analysing the temporal dynamics of model performance for hydrological models, Hydrology and Earth System Sciences, 13, 999-1018, 2009.

Presentation quality

The paper is well structured and comprehensible, tables and figures are helpful, equations are mainly properly described.

A few minor points (leading number indicates page/line):

- 7656/3-13: Please name the basin size
- 7657/16-20: What are typical ranges for uh and uc?

Yours sincerely,
Uwe Ehret