Hydrol. Earth Syst. Sci. Discuss., 9, C3846–C3849, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C3846/2012/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Continuum: a distributed hydrological model for water management and flood forecasting" *by* F. Silvestro et al.

Anonymous Referee #2

Received and published: 16 August 2012

Review of Silvestro et al.

This paper presents a considerable effort of designing and implementing a distributed hydrological model from scratch, and implementing it for two nested catchments in Italy. The authors state that the focus of the model is to develop a model that "[balances] the need to reproduce the physical processes with the practical goal of avoiding over-parameterization".

Although the work undoubtedly has significant technical merit, the scientific contributions are far less clear. Indeed, many models based on a similar combination of conceptual and physically-based elements have been designed with different levels of complexity and assumptions. The authors name several on page 7641, but the actual

list is of course much longer (some others that spring to mind are VIC, HBV, SWAT, ...). The explicit representation of the energy balance is perhaps less widespread, but commonly found in land-surface schemes such as JULES, HTESSEL, ORCHIDEE and others. Hence, it is hard to avoid an initial "yet another hydrological model" reaction.

That said, the development of hydrological models is of course no closed case, and many aspects of hydrological model development (several of which are mentioned throughout the manuscript) such as overparameterization, identifiability, equifinality, robustness, non-stationarity and others are still highly problematic and in need of further research.

It is therefore a pity that the manuscript does no attempt to push any of these boundaries. Instead, a very simple calibration - validation procedure is presented that goes little beyond the conclusion that the model "produces good results" (p 7666). Frankly, I would expect that any of the above-mentioned models would produce good results (i.e. . There is surely scope for a far more detailed and scientifically novel analysis. Some opportunities that occur to me are:

* multiobjective calibration

I find the section on model parameterization quite confusing (How is the short period calibration brought forward to the global calibration, what sampling method was used, which prior parameter distributions, how many model evaluations etc) but essentially the model is calibrated in a classic way on streamflow using the Nash-Sutcliffe efficiency as a performance measure. The surface temperature is then compared with surface temperatures obtained from satellite data.

Again, this approach is little informative. It is not surprising that the modelled surface temperature is in good agreement with the observed values, especially if air temperature is used as a model input. In fact, without plotting air temperature values in Fig. 15 and 16 it is very hard to evaluate the contribution of the model. Instead, it would be much more interesting to explore the possibility of assimilating the observed LSTs and evaluate their potential contribution to constraining model parameters and internal states and improving (if at all) streamflow predictions.

* overparameterization

The model claims to be a balance between representing physical processes and avoiding overparameterization, but very little argumentation is given for this claim. The model is probably no less overparameterized than any physical model of which all but a few parameters are kept fixed. It seems that the reason for doing so is the availability of satellite-based information about land-cover (LAI, and presumably also CH?), from which relevant parameters are obtained, and keeping many (often empirical) parameters of the various equations constant. Also, despite the claim that it is a distributed model, soil parameters seem to have been kept homogeneous over the catchment. This of course limits the parameters to be calibrated, but also greatly reduces the ability to represent spatial patterns and thus the claim of being a distributed model.

This approach of course suffers from the classic issues of errors in the observations and non-commensurability of the processes. So how does the presented approach differ from standard practice? One potential option would be to use a data assimilation approach (as is suggested but not elaborated upon) and check the contribution of data sources to model performance, the robustness of the model etc.

* Identification of model errors

It is hard to interpret the model performance. Although the performance measures (tables 3 and 5) look quite OK, tables 4 & 6 and the hydrographs in Figs 6 and 7 show very big discrepancies between simulated and observed peak flows (up to 70% ?!). This indicates severe issues with the catchment water balance, which are likely to be caused by the input data (precipitation). Also, the apparently very flashy regime of the catchment makes it impossible to evaluate the model performance in base flow conditions. Is there any base flow at all in the catchment, or is it zero most of the time? If there is base flow, plotting the hydrographs on a logarithmic y axis may facilitate the

visualisation.

To conclude, the main reason why I do not recommend rejection of the paper at this stage is because the design and implementation of the model seems to be well thought out and technically sound. However, to be publishable in HESS, the paper will need a major revision to increase the scientific significance. This would include a rethinking of the concept of the paper, and several more analyses, including probably the implementation to other catchments and a much more thorough calibration and validation procedure aimed at testing a clear scientific hypothesis. The current catchments seem problematic because of the very flashy hydrological regime, which makes that most of the hydrological time series is uninformative. Also, the problems with the catchment water balance (although very common in hydrological analysis) may jeopardize any attempts for hypothesis testing in the presented catchments.

Alternatively, this paper may be better placed for a journal that focuses more on the development and implementation of models, such as for instance Geoscientific Model Development or Environmental Modelling and Software. But in this case, it will need to expand on the technical implementation, availability etc. For instance, one question that arose while reading the paper is the type of time stepping that is used. The emphasis on being a continuous model would suggest that an implicit time stepping scheme is used (which would cater for some of the issues highlighted by Clark et al. (2010)) but no information at all is given about this.

References:

Clark, M. P., & Kavetski, D. (2010). Ancient numerical daemons of conceptual hydrological modeling: 1. Fidelity and efficiency of time stepping schemes. Water Resources Research, 46, W10510. doi:10.1029/2009WR008894

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 7639, 2012.