Review of the manuscript

A retrospective on hydrological modelling based on half a century with the HBV model

(Authors: By Jan Seibert and Sten Bergström, submitted to HESS)

Dear colleagues,

This manuscript tells us a story of the history of the HBV-model, in connection with some reflections on the development of hydrological modelling over the past decades. The manuscript is well written, nice to read and embedded into a fictional interview of a “young” (i.e. less experienced) PhD-student. This is a funny and unusual style-method to make a story more entertaining, which otherwise might be a bit less exciting for outsiders.

I find the manuscript interesting and it contains and summarizes various information regarding the HBV-model. Of course, this information are not “new” in the sense that they have not been presented before. However, as this article is planned as a contribution to a special Issue on ’History of hydrology’, it fits very well into this overall theme.

I have some more detailed and minor comments (see below) and three more general remarks and suggestions. If those are followed / incorporated into the article, the final article could lead towards a broader discussion and thus have a more relevant impact.

General remarks and suggestions:

1) **“Physically based” vs. “conceptual” ?**
I think that the ‘modelling philosophy’ presented here is a bit narrowed to the personal view of the HBF developers, and to the state of discussion in the 1990ths. For example, when I was a young PhD student (I am in something between the ages of the two authors), I always followed the discussion about which models "are allowed" to be called "physically based" and which are "only" considered "conceptual" with some incomprehension but also amusement. My feeling was – and still is – that this is an artificial or at least not a meaningful distinction, which is not really worth to follow. Why? First, in principle, all the mentioned models follow the basic physical principles of mass and energy conservation. The authors also claim this in their manuscript and that is why they demand that HBV also be classified as a physically based. But second, there can always be a higher degree of physical detailing. Thus, even the “most physically based” model will have shortcomings regarding some peculiarities of the hydrological cycle or some hydrological processes.

   Therefore, I think that the more relevant questions are: which practical tasks can a hydrological model solve and which hydrological processes can be distinguished and quantified? Therefore, I would recommend to better use the term “process oriented model” rather than “physically based”. It just makes more sense. Following this line, one can elaborate on which hydrological processes are represented in the model by which approaches. In this regard, HBV is a model with a focus on snowmelt processes and runoff generation as a consequence of catchment wetness (a subsurface hydrological process). It is not made for, e.g., hydrological conditions, where soil-moisture-evapotranspiration interactions are key, or – another example – where Hortonian overland flow is the main runoff generation process.

So my recommendation in this regard: on should more clearly state, for which questions/tasks and hydrological conditions the model is well suited, and for which less.

2) **Number of parameters and desire for optimization**

Another issue, which is related to this issue: One should not compare the number of parameters of lumped and distributed models. This is comparing apples with oranges. The authors are proud that HBV has only 5 (or even 4) parameters and compare that with the well-known SHE-model (lines 395/396) stating that "such as SHE with hundreds of parameters". These "hundreds of parameters" are a consequence of its areal distribution. If HBV is run in a distributed mode (what is possible and stated by the authors), it can also have hundreds of parameters. Of course, when one looks on one "SHE-grid" only (i.e. SHE in a nun-distributed mode), that SHE has much more parameters (may be around 20 – 30?). The reason is that it entails more processes and a higher degree of detailing.
A few parameters are advantageous for optimization procedures, but not necessarily sufficient to find a clear optimum (as shown many times by Keith Beven and followers). So, if an optimized model (usually optimized for discharge at the catchment outlet only) is desired, a low-parameter model is the best choice. However, if detailed spatial patterns and /or various processes are aimed for, one needs a higher degree of distribution and detailing of processes. This will inevitably yield (much) more parameters, lesser Optimization capability and the "danger" of equifinality. Still, one should keep in mind, that also in the hydrological nature certain states (or hydrography) might be reached by different potential conditions and system states. Thus, why should our models behave in a more convergent than nature does?
Finally, I find it absolutely essential to discuss the issue of which process should be described well by our model. The authors say that HBV performs well, several times. I agree, if one looks on the runoff dynamics at the catchment outlet. But I would not recommend this model for some other purposes, e.g. for (areal patterns of) root water uptake and evapotranspiration, or let's say surface runoff and subsequent erosion.
Thus, I think model purpose and why and why not distributed models might be appropriate needs to be discussed. In this regard, an important purpose is also the (easy and/or quick) applicability. One of the major strengths of HBV.

3) **Need for a classification of hydrological models**

Based on the previous thoughts, instead of comparing between different hydrological models in a competitive manner, I would ask for a kind of classification / categorization along the question, which tasks should be solved and on which processes the focus of the model lies. In this regard, I would claim there is a "family" of models where the runoff response of a catchment is simulated through a non-linear function of the catchments wetness. HBV is probably the most prominent member of this family. Others, most already mentioned by the authors, belonging to this family and about the same age are NAM (Nielsen & Hansen, 1973), Xinanjiang model (which, was 1st presented already in 1977 in China (Zhao, 1977) and than internationally in 1980 (Zhao et al, 1980)), VIC-model (Liang et al. 1994), GR4J (Michel, 1983). Even the famous TOMPODEL has ties to this family, because the overall runoff response of a catchment is also related to a non-linear function,

which is derived, however, from the catchments DTM. Other similar models have founded the sons and daughters of this first generation, such as the Arno-model (Todini, 1996) or in Germany LARSIM (Ludwig & Bremicker, 2006). And many others.
In this regard, the first generation of this family, with HBV as its best-known member, has been very successful. Still active and in use, and so many children and grand children.

But one must also be aware other families, which are able to contribute significantly to some of today's hydrological tasks, e.g. the ones which focus more on (Hortonian) surface runoff generation, such as KINEROS (Smith et al, 1995), QSOIL (Faeh, et al., 1997), HLIIFLOW (Bronstert & Plate, 1997) or its extension CatFLow (Zehe et al., 2001)
And there are more such families, as the ones concentrating on groundwater dynamics, urban hydrology and the ones which try to combine as much as possible, such as the previously mentioned SHE-model, or focus on agricultural landscape such as the most applied SWAT (Arnold et al, 1994) or its younger sister, the eco-hydrological SWIM (Krysanova et al, 1998).

Now, let that be enough of categorization. It would certainly be profitable if the authors could include some of these aspects in their manuscript.

Some detailed comments:

- Line 115: "advocation for realism in model development"? What is 'realism' in this regard? And Is this still valid for today?
- Line 130: Instead "represent all hydrological processes", better term it "represent all relevant hydrological processes",
- Line 285: what do you mean with "re-organising observed flood generating factors'?
- Line 300: "risk for overparameterisation"? You may discuss it in the relation with complexity of model and nature? Also nature may have several means to come to one state? What can one do in this regards?
- Line 315: "groundwater dynamics"? You may elaborate a bit how far the catchments wetness in the HBV model can be related to observed gw-dynamics.
- Lines 465-480: about the application for ungauged catchments: You mention the option of ensemble applications (many sets of parameters). You may elaborate a bit more on this. For my understanding this is a very valuable approach.
- Figure 6: The value of a model depends on its purpose!! If the purpose is to look like the reality, the left figure is much "better".

Mentioned literature:

Arnold, J.G., Williams, J.R., Srinivasan, R., King, K.W., Griggs, R.H.(1994): SWAT, Soil and Water Assessment Tool, USDA, Agriculture Research Service, Grassland, Soil & Water Research Laboratory, 808 East Blackland Road, Temple, TX 76502.

Beven, K., Kirkby, MJ. (1979): A physically based variable contributing area model of basin hydrology Hydrological Science Bulletin, 24, pp. 43-69

Bronstert, A., Plate, E.J. (1997): Modelling of Runoff Generation and Soil Moisture Dynamics for Hillslopes and Micro-Catchments. Journal of Hydrology, 198 (1-4), 177-195.

Faeh, A.O., Scherrer, S., Naef, F. (1997): A combined field and numerical approach to investigate flow processes in natural macroporous soils under extreme precipitation Hydrol. Earth Syst. Sci., 1, 787–800, 1997

Krysanova, V., Müller-Wohlfeil, D.-I., Becker, A. (1998): Development and test of a spatially distributed hydrological/water quality model for mesoscale watersheds. Ecological Modelling, 106, 261-289.

Liang, X., Lettenmaier, D. P., Wood, E. F., and Burges, S. J. (1994): A simple hydrologically based model of land surface water and energy fluxes for general circulation models, J. Geophys. Res., 99, 675.

Ludwig, K., Bremicker, M. (Eds) (2006): The Water Balance Model LARSIM - Design, Content and Applications, Freiburger Schriften zur Hydrologie, No. 22

Michel, C. (1983): Que peut-on faire en hydrologie avec modèle conceptuel à un seul paramètre?, La Houille Blanche, 69, 39–44.

Nielsen, S. A. and Hansen, E. (1973): Numerical simulation of the rainfall-runoff process on a daily basis, Nord. Hydrol., 4, 171–190.

Smith, RE., Goodrich, DC., Woolhiser, DA., Unkrich., CL. (1995): KINEROS – A kinematic runoff and erosion model; Chapter 20 in V.P. Singh (editor), Computer Models of Watershed Hydrology, Water Resources Publications, Highlands Ranch, Colorado, 1130 pp.

Todini, E. (1996): The ARNO rainfall-runoff model, J. Hydrol., 175, 339–382.

Zehe, E., Maurer, T., Ihringer, J., Plate, E. (2001): Modeling water flow and mass transport in a loess catchment, Physics and Chemistry of the Earth, Part B: Hydrology, Oceans and Atmosphere, Volume 26, Issues 7–, 487-507.

Zhao, R.J. (1977): Flood forecasting method for humid regions of China. East China Institute of Hydraulic Engineering, Nanjing, China.

Zhao, R.J., Zhang, Y.-L., Fang, L.-R., Liu, X.-R. & Zhang, Q.-S. (1980): The Xinanjiang model. In: Hydrological Forecasting (Proc. Oxford Symposium, April 1980), pp 351-356. IAHS Publ. 129, IAHS Press, Wallingford, UK.