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Hydrology and Earth System Sciences Discussions

Interactive comment on "HESS Opinions "The art of hydrology"[1]" *by* H. H. G. Savenije

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HOLISM

In my opinion, the opinion paper by Savenije (2008) is an excellent piece. My review has nothing important to suggest. I agree with the ideas expressed and I like the author's tone. In particular, I agree with the overarching idea that reductionism cannot help very much and that holism is needed in hydrology. In this respect, I like the author's courageous and clear-cut statements, such as:

"The underlying idea that upscaling physical laws within imposed morphologies will lead to a reliable model of reality is flawed. The concept is wrong ..."; "The idea that

[1]Invited contribution by H. H. G. Savenije, the EGU Henry Darcy Medallist 2008 for outstanding contributions to Hydrology and Water Resources Management.

physically based models can do without calibration is based on the erroneous reductionist concept"; "The only way to do this [correct modelling] is through a top-down approach."

I also like that Savenije does not care too much to be politically correct (cf. his footnote with a grammatical suggestion, interestingly also made by Klemes, 2008, which I have adopted).

From the other reviews and discussions that I have read, I understand that there is no strong rebuttal of Savenije's ideas (which pleases me) except perhaps for a caution for a more balanced approach (Sivapalan, 2008). I think that, when a certain approach and logic (the reductionism) dominates, such a caution, not to overlook its merits, is not necessary. What is demanded is not to advocate the (inexistent) balance, but to shed light to the problems of the dominant practices – and Savenije does a good job in this. When "much of our research is going the wrong direction" (and I agree with Savenije on this), what is needed is to show what is wrong and propose what could potentially be right (see also Koutsoyiannis et al., 2008a, b).

This is my "holistic" view of Savenije's (2008) paper. Unfortunately, my agreement on the entire contemplation of this paper does not serve well the dialogue. To counterbalance this (and perhaps to make my review more balanced ...) I am turning into reductionism, discussing in the remaining part of my review a few details of the paper. Clearly, with these piece-wise comments I do not wish to perturb the holistic image of the paper.

ANOMALIES

I view the magnificent examples that Savenije provides in his section "What is hydrology?" as another demonstration of the limitations of the reductionist approach in hydrology. Reductionism is not only the decomposition of a spatial entirety into small elements but also the splitting of a process into different components (e.g. the separation of time series into additive parts such as deterministic periodic, trend and stochastic

HESSD

5, S2096–S2104, 2008

Interactive Comment

parts; Koutsoyiannis, 2006, 2008b) and the decomposition of the entire system operation into different functions (e.g. the surface vs. ground water discussion in Savenije, 2008). The former case (reduction of a process into components) is meaningless, while the latter is acceptable if the bidirectional interactions of the different functions are all well represented. However, it has been a common practice in hydrology to regard such interactions as unidirectional (Efstratiadis et al., 2008), because this offers a more convenient modelling approach (a chain of models, each item of which takes an input from a previous item and gives its output to the next item). This unidirectional chain approach extends beyond hydrology, as the numerous current climate impact assessments testify, all of which are fed by climate model outputs. Another weakness of this approach is that, depending on one's specialization, some of the functions tends to be overemphasized and other functions tend to be neglected. As Savenije puts it, "Many hydrologists consider river runoff as a surface process. Such "surface" hydrologists are equally limited in view as the "groundwater" hydrologists who only look at what happens within an aquifer." The solution is clearly suggested by Savenije in the Okavango River example: "The anomaly disappeared when he considered the appropriate surface water-groundwater interaction and the interplay between local rainfall, local evaporation and the floods generated by the Okavango river."

However, the term "anomaly" that is used several times in these examples to describe paradoxical behaviours may not be ideal. "Anomaly" is Greek for "abnormality". If anything is anomalous or abnormal in the described cases, certainly is not related to the natural behaviour per se, but to our expectation that our models should necessarily represent this natural behaviour. Thus, the correct Greek term for these cases is "paradoxical" [$<$ paradoxos $<$ para + doxan = despite (our) belief]. Obviously, Nature is free to behave differently from our belief (i.e. paradoxically) and this is not at all anomalous. This is not the only case where "anomaly" is incorrectly used to describe normal natural behaviours. Recently, hydrologists, following the bad example of climatologists, have used "anomaly" to describe the departure from the average. Obviously, the departure from the average is not abnormal at all. Nor is it paradoxical, unless we have a very

HESSD

5, S2096–S2104, 2008

Interactive Comment

narrow and wrong perception of natural processes (e.g. the perception of a constant climate that has been only perturbed recently by anthropogenic influences). Rather the opposite, the constancy of natural processes so as not to depart from the average, would be anomalous. (I apologize for emphasizing on terminology but I regard it essential in science).

MODELS AND REALITY

This naturally brings me to the next point I wish to discuss. Savenije correctly states: "Unfortunately practitioners often confuse a mathematical tool (advanced though it may be) with reality." However, I do not think that this is only the practitioners' sin. We all, scientists, and engineers, in hydrology and beyond, often fall to this logical error. We hear for instance, almost every day, that "hydrological processes are nonlinear" or "climate is nonstationary". Is it not a confusion of a model with reality? Linearity and nonlinearity are properties of models, not of Nature. Stationarity and nonstationarity are properties of models – stochastic models in particular (see Koutsoyiannis, 2006). Not only do all these properties pertain to models rather than to natural reality, but even their content depends on the modelling context. For instance, we will never get an irregular system trajectory assuming linear deterministic dynamics, but we can easily get such a trajectory with a linear stochastic model. Thus, linearity and nonlinearity in deterministic modelling are quite different than in stochastic modelling.

Having distinguished models from reality, Savenije's subsequent statement "we, as scientists, understand that models are wrong, almost by definition" is natural. It is also anti-dogmatic and exculpatory. For example, one has any right to make a linear model (e.g. unit hydrograph) and this (ignoring nonlinearity) is not lack of understanding; it is just a modelling convenience (linear models are easier to apply). Also, one has any right to build a lumped model, if it serves his purposes, and again this does not contrast understanding. In turn, there is no need at all to "dismiss models that proved to work well in their context" (Montanari, 2008), even if they do not comply with our highest expectations of what a perfect model should be. In the end, the performance of any

HESSD

5, S2096–S2104, 2008

Interactive Comment

model is judged based on data, which are the ultimate criterion. As neatly formulated by Savenije, a model is a hypothesis and "[t]his hypothesis needs to be tested against empirical evidence". Naturally, this is quite similar as in probability and statistics (which, according to Jaynes, 2003, provide the Logic of Science). We formulate hypotheses and test them against empirical evidence. The outcome of this testing could well be the rejection of the hypothesis (or model) and its replacement with a better model. But such a rejection is always related to the local conditions and the problem specifications and objectives. A model rejected in one catchment may be acceptable for another catchment or for another objective.

MODELS, HYPOTHESES AND THEORIES

While I agree with Savenije's view of a model as a hypothesis, I would not characterize the model or the hypothesis as a "theory". Thus, I would not adopt Savenije's formulation that "Hydrological modelling is the same as developing and encoding a hydrological theory. A hydrological model is not a tool but a theory." In my view, "theory" is a strong term more appropriate for mathematics (e.g. set theory, group theory, probability theory) and physics (e.g. special and general relativity theory, quantum mechanics theory, statistical mechanics theory). I do not think that we have developments in hydrology of equal importance and depth to deserve the characterization of a theory. We necessarily rely on theories in mathematics and physics.

HYDROLOGY AND HYDRAULICS

The same is also true for hydraulics, which mostly relies on the theories in fluid mechanics (e.g. theories of turbulence). Hydraulics has, thus, some similarities with hydrology, but I agree with Savenije that the two are essentially different disciplines. I, too, agree with his statement "Hydraulics ... has observable or imposed boundary conditions and – as a result – makes use of a bottom-up and reductionist approach." In my view there are some additional differences. Hydraulics refers to generalized abstract objects (e.g. idealized pipes and canals, open channel flows, and even the water as an abstract

HESSD

5, S2096–S2104, 2008

Interactive Comment

fluid) whereas hydrology refers to localized and specified objects (the Okavango River, a specific flood event, the water on Earth). In addition, hydraulics deals with simple, mostly artificial systems or parts of systems (hydraulic constructions) whereas hydrology deals with natural systems that are much more complex. Even the most complex hydraulic system (e.g. a water distribution network in a large city) is composed of repeated segments of simple types (pipes of circular cross-section with diameters taking values from a limited set). This makes the reductionist approach possible and preferable (though measurements are again very important for a faithful representation of the system). In contrast, in a hydrological system, whatever separation into pieces will be attempted, each component will be unique, different from all other components. This calls for a holistic approach and a probabilistic description of the hydrological system. A probabilistic approach of the properties of the system or its parts (if the problem objectives demand to distinguish some parts of the system) is the only way to make a faithful yet parsimonious description – and this is far different from the detailed representation in reductionism.

MODELS, SOFTWARE AND RESEARCH PAPERS

With his statement "models should never be rigid, as most commercial software is" makes a very interesting point that deserves to be discussed. In most people's views, "model" and "software" (whether commercial or not – but in general a ready product) have become synonymous. This may be somewhat justified for practitioners, but sadly, it extends also in education and research. It has not been uncommon that students are taught specific software products. (Personally, I dislike this practice and I urge my students to use the lowest level of computational means, e.g. spreadsheets, and the highest level of cognitive means, i.e. their brains). It has also been a common practice in universities to conduct graduate or postgraduate theses that are applications of a specific software product with data from, say, catchment X. Change X with Y and you have another thesis. Of course, the second will have some differences from the first (e.g. different infilling of missing data). Even worse, this extends to research published

HESSD

5, S2096–S2104, 2008

Interactive Comment

in scholarly journals. Savenije neatly states "in practice I see a lot of papers, both in review and in print, that do not include elements of art. There are many papers that deal with the application of an existing hydrological model, or that describe automated calibration, or that apply standard statistical methods, without much creativity, empiricism or innovation." My experience is absolutely the same, and I wonder if this indicates any progress in science. It may rather be a regression.

Certainly, a proper discussion of this problem would be out of the scope of the paper (and my review) but I am pleased that the author touches it. I wish only to add a note related to our discussion: The more reductionist our approach, the more complex the models are and the less understanding and control (e.g. changes of code) by the user is allowed. Simpler and more parsimonious models allow better unfolding of the "artistic" skills of scientists.

SCIENCE, TECHNOLOGY AND ART

The latter comment brings me to my last part of this discussion, devoted to "art". I am not intrigued by Savenije's extended reference to art in the paper or by his title "The art of hydrology". After all, art and technology are closely related in Greek: "techne" is the Greek word for art, and thus technology must mean "reason in art". "Polytechneion" (changed in English to "polytechnic") means a "multi-art" centre.

Savenije claims that "modelling is an art" and clarifies what he means by "art" by listing the qualities pertinent to it, i.e. "imagination, inspiration, insight, field experience, creativity, ingenuity and skill". I do not have any objection on this, but I think that these qualities pertain to science and technology, too. I cannot imagine (good) science and technology without any of these qualities. May I also add that science itself is an art. According to AAAS (1990; quoted from Gauch, 2003) "Science is one of the liberal arts and ... must be taught as one of the liberal arts, which it unquestionably is."

I think I understand Savenije's reasons in shaping "The trinity: science, technology and art" (personally, I would add a bit of philosophy, but this is another paper). However,

HESSD

5, S2096–S2104, 2008

Interactive Comment

there is some risk of confusion or downgrading of science and technology, if we identify them with reductionism and if we exclude the necessity for "imagination, inspiration, insight, field experience, creativity, ingenuity and skill" from science and technology.

A similar risk may be implied by his statement "The operation of piecing together small elements and generating progressively larger elements, which is at the heart of 'physically based' hydrological models, is a natural operation for the human brain." I think that the opposite operation, the holistic perception or contemplation, is also natural to the human brain. Perhaps it is less common in hydrology but, in a wider perspective, those who have substantially advanced science, the real "artists" of science, may have developed more this opposite natural operation of their brain.

I propose, thus, to read Savenije's "trinity" in a holistic manner, in which the three parts are not separate or antagonistic but synergistic, indivisible and co-essential components, that, in addition, together make a whole that is more than the sum of its parts (following Aristotle's view of holism).

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HESSD

5, S2096–S2104, 2008

Interactive Comment

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HESSD

5, S2096–S2104, 2008

Interactive Comment