Review of opinion paper

'Hydrological models are mediating models'

By L. V. Babel and D. Karssenberg

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

1. Scope

The article is well within the scope of HESS.

2. Summary

The authors start with the statement that in Hydrology, only few accounts on the nature and function of models exist and that traditionally, models in Hydrology have been classified as either 'physically-based', 'conceptual' or somewhere in between and that 'physically-based' models are usually associated with a higher degree of realism.

From this starting point, the authors argue that a novel and better view on models is that of 'mediators', which implies that i) all models are partially dependent on observations and theory, ii) also include non-deductive elements (such as the numerical scheme, or the model choice being dependent on the goal or personal preferences) and iii) that they are instruments for scientific inquiry about both theory and the world (i.e. for induction, deduction and abduction).

The overall goal of the article is to discuss the applicability of this 'mediating' paradigm to hydrological modeling, which is carried out by discussing three different hydrological models (TOPMODEL, SHE, HydroGeoSphere) with respect to points i) to iii). From this the authors conclude that the traditional distinction in conceptual and physically-based models in Hydrology is too simplistic and that devoting more importance to identifying and communicating all facets involved in model development will increase its transparency.

3. Evaluation

I have several comments and concerns about the paper. Most of these points reflect on general aspects, so I will discuss them in the order of the above summary and will point to the places where they have been addressed in the manuscript:

'Only few accounts on the nature and function of models exist in Hydrology'

(see p. 10536/3, 10537/13)

I agree with the authors that there is not a large body of literature on the topic, but there is more relevant work on it than the cited literature, such as Oreskes (2003), Oreskes et al. (1994), Odoni (2010), Refsgaard and Henriksen (2004), Cunge (2003), Gupta et al. (2008), Gupta et al. (2012).

'Hydrological models are divided into physically-based and conceptual models (or somewhere in-between). This traditional distinction is too simplistic.

(see p. 10536/5, p. 10536/20-23, 10537/14-21)

I agree with the authors that this traditional classification of models is too simplistic, and that there is no fundamental difference between models coming from either domain: Any scientific law formulated for a dynamical system to describe its transformation of an input signal to an output signal is valid for a specific process, domain and spatiotemporal aggregation level, neglecting subgrid variability, often making use of effects of self-organization acting among smaller-scale system constituents (see e.g. the scale-way principle of Vogel and Roth, 2003). In that sense, Newton's laws of motion are not fundamentally different to a catchment-scale storage-discharge relation. The only difference can be the generality of its applicability.

However, as the authors correctly state on p. 10547/21-29, the 'extreme ends' of modeling approaches are not physically based and conceptual models. On the data-driven side, it is neural networks or similar approaches that only weakly rely on prior knowledge encapsulated in models when establishing input-output relations. So the question is why the authors did not argue from these two extremes.

'Physically-based models are usually associated with a higher degree of realism'

(see 10537/21-25)

The authors claim that the above statement is widely assumed and in support cite Beven (1989) and Mulligan (2005). Actually, Beven (1989) claims exactly the opposite (see the abstract therein): 'This paper argues that there are fundamental problems in the application of physically based models for practical prediction in hydrology. The problems result from limitations of the model equations relative to a heterogeneous reality; the lack of a theory of subgrid scale integration; practical constraints on solution methodologies; and problems of dimensionality in parameter calibration.'

This nicely sums up all the concerns about physically based modeling and I dare say that this is still valid and generally agreed on in the hydrological community.

A more recent publication by Refsgaard et al. (2010) comes to the same conclusion when reflecting on the physically based model SHE: ' The fundamental scientific problems related to the inability to incorporate local scale spatial heterogeneity, scaling and uncertainty that were formulated are fundamentally still unresolved. Thus, in spite of the original visions, the hydrological community has not yet witnessed a model that in a universal sense (i.e. at all scales and for all internal variables) simulates accurate results for the right reasons'.

In general, I agree that physically-based distributed modeling can have a higher degree of realism and potential for extrapolation, but only if it is supported with system properties, initial and boundary conditions in adequate detail. So I think claiming that the above statement reflects the general opinion in hydrology is misleading. It should at least discuss its limitations.

'A novel and better view on models is that of mediators', which implies that i) all models are partially dependent on observations and theory, ii) also include non-deductive elements and iii) that they are instruments for scientific inquiry about both theory and the world.

(see p. 10536/14-20, and section 3)

The authors claim that this view on models (models here are defined as 'time-transient mathematical models, with properties described by state variables, which are mostly spatial and changing over time as a result of a state transition function with associated parameters'), as formulated by Morrison and Morgan (1989) is fundamentally new, especially in hydrology.

I doubt that this view is really novel in hydrology. The limitations and mainly heuristic value of models have already been formulated by Oreskes et al. (1994), which is even cited by the authors(!): 'Verification and validation of numerical models of natural systems is impossible. This is because natural systems are never closed and because model results are always nonunique. … The primary value of models is heuristic'. I dare say that this is not only known in the earth sciences in general, but also in hydrology in particular.

With respect to i), I know of no one that would claim that a hydrological model is purely 'theory-based' or 'data-based', rather models are classified in between these hypothetical extremes. There are recent approaches based on information theory aiming at quantifying (in model-based deduction/prediction) the relative information contribution of forcing data, boundary conditions, model structure, model parameters etc. about a target quantity of interest. The 'information' view is in my eyes a promising way to classify different modeling approaches. See recent work by Steven

Weijs, Hoshin Gupta, Grey Nearing and others. In that context, I also missed a general definition of the terms 'theory', 'world' and 'observations', which are frequently used in the text.

With respect to ii): I agree with the authors that models include non-deductive elements, but I dare say that this is general knowledge in hydrology: See e.g. the five formal steps of model building in Gupta et al. (2012), also the many references therein on the same topic (Iliev 1984; Beven 2001; Anderson and Woessner 2002; Neuman and Wierenga 2003; Refsgaard et al. 2006; Gupta et al. 2008; Clark and Kavetski 2010; Kumar 2011). Even in standard literature of hydrological engineering (e.g. Van Waveren et al. 1999) this is acknowledged, and there is a vast literature on the influence of specific non-deductive model elements, such as the numerical scheme (e.g. Clark and Kavetski 2010, Kavetski and Clark 2010).

With respect to iii): model have since long been used in hydrology for scientific inquiry both about 'the world' and 'theory', i.e. for induction, deduction and abduction (see e.g. Kleinhans et al. 2010, Sivapalan 2009), for example inference to the best theory (Kleinhans et al. 2005), pursuing multiple working hypotheses (Clark et al. 2011), model structural assessment (Gupta et al. 2012), etc. Also, in this respect a reference to the long-standing discussion on the comparison/combination of top-down and bottom-up modeling in hydrology is missing, as this is precisely what the authors refer to as 'theory to world' and 'world to theory'. See e.g. Sivapalan et al. (2003).

'Devoting more importance to identifying and communicating all facets involved in model development will increase its transparency.'

(see p. 10536/25-27)

I agree with the authors, but again I think that this statement comes as no surprise to the hydrological community, see e.g. van Waveren et al. (1999) or Gupta et al. (2012).

To conclude: Although the topic of the manuscript, reflecting on the role of models in hydrology is interesting and important, the paper is motivated by statements that are partly outdated and comes to conclusions that are not really new. In addition, some literature is not cited correctly.

Yours sincerely,

Uwe Ehret

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