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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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I agree with virtually everything in this opinion piece, but a misrepresentations needs to be corrected and it would be useful to have more comment about the issue of model/theory comparison that is somewhat glossed over here.

The misrepresentation is about 'models of everywhere'(P3158, Line 10 / P3165, line 4). The only recent paper that I know of written about models of everywhere is my paper in HESS 2007. I think the author should read it again, because it advocates exactly the type of flexible model structure and model testing approach that is also advocated here. The opinions expressed are, in fact, almost identical, and in that paper I also suggest

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

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that we learn most when our models really are shown to be wrong; both the Okavango delta and Meuse/Mosel examples in this opinion piece are actually good examples of this. I would suggest, in fact, that they are good examples of the learning process about appropriate model structures that was suggested as a way of approaching models of everywhere in the 2007 paper.

The issue that is glossed over is how do we know when one model is more appropriate than another, whether using a top-down or other approach (see, for example, Beven, 2008)? Flexibility is fine, but also gives a model structural dimension to equifinality, as well as the parameter dimensions that are usually evaluated for a given model structure (and even hydraulic models are not immune from equifinality or model failure, see for example, Pappenberger et al., 2005, 2006).

If we do as Savenije suggests and treat models as hypotheses or theories, then this implies hypothesis testing. Allowing that hydrological modelling is an Art also implies that different hydrologists can have different interpretations and create different model components. They should be confronted with data, as suggested here, but in hydrological situations the inputs are uncertain, parameters cannot generally be estimated a priori (especially for top-down models) and any observations with which we compare model predictions are also uncertain. The uncertainties are not just statistical, so hypothesis testing is difficult and model falsification is all too easy. We have to compromise in accepting any model as useful, but some opinions on just how do we tell when one model might be more useful than another would also be valuable (I, for one, have been struggling with this problem for many years)?

References

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