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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

H. H. G. Savenije

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Reply to Makarieva and Gorshkov (2008)

First of all, I would like to thank the discussants for their elaborate and very interesting contribution. The discussion on the art of hydrology has led to a very rich debate, indeed. I would like to reply to two issues. First, the need for making art explicit in hydrological research, and second the balance between top-down and bottom-up approaches, which Sivapalan (2008) highlighted as well in his contribution to this discussion.

The final remark by Makarieva and Gorshkov (M&G) that art should be implicit in science and engineering is, of course, completely endorsed. In fact, if all engineers and

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

scientists would do their research with imagination, creativity, ingenuity, experience and skill, the call for art in hydrological research would have been redundant.

On the balance between top-down and bottom-up approaches, M&G state that a top-down approach only leads to a predictive model if the mathematical relations used to fit the data correspond to, or are based on, physical laws that allow extrapolation beyond the range of the data used for calibration. Clearly, we wouldn't need a top-down approach if a completely reductionist framework, without the need for calibration, would work in hydrology. But as a result of the overwhelming heterogeneity in the media through which the water moves and the subsequent fussy scale breaks that occur when we gradually upscale, calibration will always be necessary.

The point is that when we use regression analysis to calibrate a model, it is essential that we use the right mathematical equations to fit the data. MG give examples of cases where a wrong theory, an incomplete theory, or a theory that violates physical laws was used to fit the data. In such situations sometimes a decent fit can be obtained with observations, but such a relationship does not work beyond the range of data on which the relationship was calibrated.

I'll add an example from my own experience. Many researchers have tried to find empirical relations to predict salinity intrusion in estuaries. Most of these researchers used laboratory experiments to test their equations (e.g. Rigter (1973), Fischer (1974)). In doing so they made two conceptually wrong assumptions. The first is that they assumed a simple power function for the relation between salinity intrusion and physical drivers, such as topography, tide and river discharge. The second is that they implicitly assumed that laboratory flumes with constant cross-sections were good representations of real estuaries. The regression equation used was:

$$Y = a \prod_{i=1}^{n} X_i^{b_i} \tag{1}$$

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where Y is the dimensionless intrusion length and Xi are dimensionless numbers representing topography, tide and river discharge; a and bi are dimensionless constants. Of course, it would be purely coincidence if this regression equation would indeed correspond to the correct mathematical model. Savenije (1993, 2005) showed that a physically more correct equation would be of the type:

$$Y = \ln\left(1 + a\prod_{i=1}^{n} X_i^{b_i}\right) \tag{2}$$

One can easily see that for small values of Y, the first equation is an asymptotic solution of the second. A serious cause for concern is that many researchers have used the first equation, without even challenging its validity. The reason why they continued to use the wrong equation is because in their laboratory experiments, they never encountered large values of Y, unlike in nature where this is the rule rather than the exception. Had they used field experiments (indeed more difficult and costly to obtain), then they would have quickly seen that their regression equation was flawed. And this is the second (and I would say more fundamental) mistake. Different generations of researchers had become used to the erroneous idea that a laboratory flume of constant cross-section was a correct representation of real estuaries. Natural estuaries, however, have no constant cross-section, but exponentially increasing width. The length scale of the exponential function is a crucial parameter in the equation, and if disregarded (assuming an infinite length scale) large errors are made, particularly in estuaries with a strong funnel-shape. The second equation, however, made dimensionless by the length scale for width convergence (the most crucial parameter neglected by traditional researchers), gave an excellent fit to observations in real estuaries, and represented the laboratory experiments in flumes with constant cross-section as well.

So summarizing, I fully agree with M&G that a top-down approach needs to be combined with the correct physical relationships. This was also emphasised by Sivapalan during the discussion of this opinion paper, when he wrote that there is a need for both

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top-down and bottom-up thinking.

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