Hydrol. Earth Syst. Sci. Discuss., 7, C3954–C3956, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C3954/2010/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Hydrologic predictions in a changing environment: behavioral modeling" *by* B. Schaefli et al.

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Received and published: 8 December 2010

I think there is still misunderstanding between us in a number of ways. Going back to read my 1993 paper (it was a long time ago), that paper certainly did not conclude that physical principles would not be useful to the hydrologist, only that the principles in use at that time (and actually still widely in use today) were not adequate and that data limitations would compromise their use in consistent ways leading to potential equifinality in model and parameter sets (that paper was the first use of equifinality in this context, though the paper does not mention that the concept goes back at least as far as the book by Ludwig von Bertalanffy on General systems Theory in 1968).

This can be illustrated by using mass balance as an analogous "behavioural" principle.

It is generally enforced in many hydrological models (though some allow for rainfall multipliers etc). But there is potential for error in all of the terms of the water balance equation, as well as model structural error. These are (very often) epistemic errors rather than stochastic variability (e.g. time variable biases resulting from underestimation of rainfall inputs in higher parts of catchment because of lack of raingauges or inadequate sampling of convective rainfalls relative to synoptic rainfalls etc.... Mass balance is a good physical principle but might not always hold for the data available (see for example Beven and Westerberg, 2010)

I am suggesting that similar (often unknown) forcing errors might also compromise application of any other behavioural (e.g. optimiality) principle, whether tested against flow predictions or, more strongly, against latent, sensible and carbon fluxes. Some periods of data might be disinformative in calibration but we can never be quite sure if a period is disinformative or not when there is a mismatch. It could be that a part of the model that is wrong, including the behavioural principle being applied inappropriately for that part of the data. But these possibilities are difficult to differentiate - that is why I am still encouraging the authors to consider that there may be a range of validity for any behavioural principle.

We should EXPECT that such behavioural principles will not always apply for two reasons - one (as above and in my original comment) because it might be inconsistent with the data available so some degree of sub-optimality that allows for such uncertainties might need to be allowed; secondly (as in my original comment) because some external forcing (that might not be recorded) overides optimality (even NCP might have some relaxation time after a disturbance; other longer time scale features, as noted previously, might go back to the last glacial retreat).

It also therefore follows that there might be two reasons for mismatches between observations and predictions of a model that implements a behavioural principle. One might be because the principle is invalid (at least in part of the simulated period); the other might be because the forcing data (or observations with which the model is being

evaluated) available is inconsistent with the principle. But, as with any problem that involves the disaggregation of residual errors, it is impossible to tell the difference between them. There remains the possibility therefore of circular reasoning since, if there is a de facto belief in a behavioural principle, any discrepancy must be due to some other cause and the principle (as in the water balance principle analogy) cannot be falsified.

Stan Schymanski also raised the possibility in his response of checking model outputs for consistency with the behavioural principle, even when the model does not have the principle built in as part of its assumptions. This, certainly, would be less circular but such checks would also be subject to the same data limitations and possibility of equifinality in model structures and parameter sets meeting the tests (or not, see Mitchell et al., 2010).

Reference

Beven, K J and Westerberg, I, 2010, On red herrings and real herrings: disinformation and information in hydrological inference, Hydrological Processes, in press.

Mitchell, S, Beven, K J, Freer, J and Law, B, Processes influencing model-data mismatch in drought-stressed, fire-disturbed, eddy flux sites. JGR-Biosciences, in press

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 7779, 2010.