

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

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As yet I remain unconvinced by the authors’ arguments – although I would not deny that there may well be some useful principles that might serve as useful constraints on model calibration and performance.

The question I was trying to get the authors to clarify in their paper was under what range of circumstances might they be useful (given that even the water balance constraint is not always useful).

The authors’ response that longer-term non-stationarities, which may not be “optimal” with respect to current conditions, only serve to provide the boundary conditions for

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current optimality, rather reinforces my point about defining the range of conditions over which various feasible constraints might be useful. The point I was trying to make is that all responses are conditioned on both the nature of the forcing and the past (non-stationary, non-optimal) history. That is as true for the long term (post-glacial) evolution of catchments as it is true for the next event (which might be both natural or man-induced).

I would also suggest that circularity really is an issue. It is already with imposing a water balance. A model is written to conform to this constraint (even though that might not be consistent with the observed data – there are very few hydrological modelling studies which are free from water balance problems in the data – see Beven and Westergberg, in press). Any other similar constraint (optimality of NCP being an example) can also be enforced as part of a model structure in this way. Models would then be behavioural in the sense of the authors by definition, even if not all the factors affecting NCP might not be included (because, in part, this would require adding more poorly known parameters).

So, taking the primary example that seems to underlie the authors' behavioural principle, that of optimality of NCP by the vegetation, then vegetation response will be dependent on the energy and water available at any given time (with other dependencies on air temperature, CO₂, radiation frequency, nutrient status, ABA signaling, growth stage etc). At any given time, these are boundary conditions for the response of the plant which might, conditional on these conditions, aim to maximize NCP.

But, as with the water balance constraint, the optimality constraint might not be always be appropriate over longer periods of time, particularly in extreme conditions. Consider an extended drought when water becomes limiting. Stomatal control is then concerned with damage limitation rather than NCP. This could (of course) be considered as part of the optimality behaviour, as conditioned by water availability. . . .but only up to the point that the plant drops its leaves or dies. This still provides the boundary condition for what follows (which will be conditional on a different regrowth optimality) but might

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not be considered an optimal strategy for the individual plants that have died.

As I suggested in my review, all such events fit happily into a forcing/relaxation framework (as do man-induced events like deforestation, planting and harvesting etc) but these multiple layers of conditionalities would seem to provide difficulties for general principles of what might be “behavioral” in the sense of the authors.

Again, that is not to say that we do not need additional constraints in getting the right modelling results for the right reasons. It is only to suggest (again) that the range of validity (the conditionalities) of any “behavioural” constraints will need to be understood and specified. We should be wary of treating these as universal principles – otherwise we may get surprises. Hence my encouragement to the authors to provide more detail about the range of validity of the types of constraints they wish to impose.

What would be really interesting, given time, is to explore the equifinality of model structures and parameter sets that could meet such constraints within the limitations of the data uncertainties and in periods where extremes might be important. Stan Schymanski did indeed show that the parameter set that appeared to optimize NCP also gave the best hydrological simulation (noting that in this case these constraints are not independent, NCP will be linked to the partitioning of the water balance). . . .but was this really the only consistent solution within a forcing/relaxation framework given uncertain inputs?

Keith Beven

Reference

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

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