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Hydrology and Earth System Sciences Discussions

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

K. Beven (Referee)

k.beven@lancaster.ac.uk

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Review of HESSD- 7-7779-2010 by Schaefli et al.

I very much agree with the authors' analysis of the problem at the end of Section 1.1. of this paper:

The trajectory of ecosystem evolution in these highly complex coupled models de15 pends very much on the realism and accuracy of the various process descriptions and the associated parameter values. Under these circumstances, what confidence do we have that such predictions turn out to match reality, or even come close to what might actually happen in the future? The descriptions of individual processes, process

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interactions and feedbacks are intrinsically imprecise and uncertain, and may highly depend on initial conditions, which are also possibly unknown.

The central thesis of this paper is that these problems will be mitigated by invoking yet-to-be-elucidated general organizing principles of catchment processes that will allow models to be differentiated as either behavioural (in accordance with these principles) or non-behavioural (even if they appear to give good fits to the data). In this way we are more likely to get the right results for the right (behavioural) reasons.

I also have no arguments with this as a general principle. Any hydrological modeller would want to use any type of proper constraint in deciding whether a model is behavioural or not. That idea effectively underlay the first use of the term behavioural by Hornberger, Spear and Young in their generalized sensitivity analysis, and is implicit in defining the set of behavioural models in the GLUE methodology (Beven and Binley, 1992, Beven, 2009). However, in most if not all applications of these methodologies – as the authors point out - behavioural has been defined with respect to how well a model reproduces the observed responses rather than any other organizing principle (though it is worth pointing out that even as far back as Beven and Binley, 1992, they suggested that process information could also be used in evaluating models, including binary or qualitative measures reflecting whether a process, such as overland flow, was operative or not. That is actually not so different from what is being suggested here).

So the concept is not new. What is being suggested as new is the type of constraining principle that might be used. But here we seem to run into a problem. Most hydrological models already use some fundamental organizing principles. Nearly all use the water balance; some use the energy balance; the REW concepts would also like to impose the momentum balance. But they do so in a circular way; the principles are imposed as part of the model structure and are therefore necessarily satisfied whether the model gives good reproduction of the observations or not.

In some cases, however, the forced implementation of these principles in this way

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might be quite wrong: because the inputs are consistently biased, because the high discharge data is poorly estimated because of an incorrect rating curve extrapolation; because there is leakage or underflow or groundwater discharges as well as that measured at the stream gauge etc. If the water balance is posed as a hypothesis, it will often be the case that it cannot be demonstrated to be valid with out allowing for significant uncertainty in its component terms. Similar issues arise in the satisfaction of energy and momentum balances (and by extension to other organizing principles). Forced application of an organizing principle could therefore be misleading (and going back to the very early days of hydrological modelling, the Stanford Watershed Model had a rainfall multiplier parameter to try and allow for consistent bias in the water balance.. . .. . ..it also had a potential evapotranspiration multiplier so it could in fact achieve this end in more than one way!!).

The implication is, therefore, that any organizing principle will have a range of validity in applications across a wide range of catchments, that will depend on the uncertainties in the modelling process, and we should be wary of applying any organizing principle by fiat.

What are the principles suggested in this paper? One is a general principle of nature works through imperfect searches for dynamically accessible optimal configurations (quote from Rinaldo). This is interpreted in terms of the system having a preference for some states likely to be more "stable" than others. A second class of principles are derived empirically – the example is used of the Budyko curve (though presumably the authors feel that these are likely to have a more fundamental organizing foundation if we could only find out what it might be.

The process of building a model based on these principles is summarized as follows:

(i) understand current or potential stable system states resulting from the co-evolution of interacting processes; (ii) summarize this understanding in some time-invariant organizing principle useful for hydrologic prediction at many places, scales and times;

(iii) build a model to simulate a range of different system behaviors; (iv) use the organizing principle to identify the most probable system behavior, i.e. to identify the most appropriate model parameterization for a given case study; (v) validate or falsify the model.

This seems to suggest a different strategy to that noted above for the use of balance equations as organizing principles. Step (iii) does not imply that the organizing principle should already be built into the model, only that it be used in the evaluation of step (iv). Thus we might not build a model that necessarily satisfies the Budyko curve, but models that in simulation did satisfy the Budyko curve would be preferred.

This would appear to be a particularly bad example to choose. If the Budyko variables are analysed from observations for individual water years for a catchment, the resulting annual points are often far from the Budyko curve (e.g. Beven and Quinn, 1994). So if the observations do not satisfy the regularity, to what extent should we expect a model to be forced to do so?

OK, so the Budyko curve may not considered to be, or in practice imposed as, a very strong constraint (though then it also does not support the authors' case strongly), but there may be better ones to be found, such as the maximization of net carbon profit by the vegetation in Schymanski et al. Using this principle, vegetation parameters were defined without reference to evapotranspiration or other output observations. The evaluation by "comparison of the fluxes predicted by this optimal model against observations then becomes an exercise in falsification of the hypothesis that plants maximize net carbon profit".

But this also seems problematic. There have to be uncertainties in evaluating the net carbon profit (in the same way as uncertainties in assessing the Budyko variables), so that the difference between some predictions for some "optimum" vegetation performance and observed outputs might also not be significant.

The same would apply to the example given of NPP optimization in the RHESSys

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model. There are many parameters that will be subject to uncertainty in models of this type. Varying only one or two of them to examine whether the NPP is optimized does not mean that the full range of potentially behavioural solutions has been found. It might be more significant if it could be shown that a model could NOT reproduce a set of observations (see the example of BIOME-BGC in Mitchell et al., 2010, or of INCA-P in reproducing P concentrations in Dean et al., 2009) but this then reverting to a definition of behavioural as assessed against uncertain observations.

There seem to be problems with all the examples given in the paper. Real optimality might be difficult to assess when faced by uncertainty in the characterisation of catchment biophysical characteristics and forcing data (and – as with the water and energy balances - I am not convinced that principles can be invoked without reference to these uncertainties). Looking around any of the catchments I have worked in, I do not see optimality principles generally at work. The soil-vegetation interactions and runoff-geomorphology interactions I see are conditioned on what happened in the last ice age and immediate post-glacial period and have little to do with current conditions. They were also already not optimal in any general sense in that era, only a response to specific conditions and events at the time. The hydrology is also strongly conditioned by the geological history from even longer ago (including whether we should expect to have water balance closure where there are Pleistocene drumlins overlying karstic limestone). I also see the more recent influence of man on the vegetation and flow pathways, in ways that do not seem to conform to any underlying principles.

I cannot see what organising principles I might usefully apply here (except perhaps in terms of the vegetation canopy making best use of the net energy available, though only in the short term and even that depends on factors beyond the plant's control, such as drainage from the root zone. A perennial plant may be adapted in some sense to the balance of inputs and drainage; it might also die when there are departures (either too wet or too dry) from the norm (which might be considered as a form of non-optimality).

None of these comments invalidate the principle involved that we might be able to

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provide better constraints on model behaviours. As noted earlier, any modeller should want to use all forms of constraint to try to ensure getting the right results for the right reason. As noted above, however, the devil is in the detail of what constraints might be appropriate where and for what purpose. It does suggest that the authors need to add more to the paper about the range of validity of the constraints that they would like to impose, particularly in the face of uncertainty. I do not really think it is enough to say that these still have to be elucidated. The examples given are not convincing: they should at least add an example that is more than a statement of principle before the paper is acceptable.

Keith Beven

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