Hydrol. Earth Syst. Sci. Discuss., 12, C5389–C5391, 2015
www.hydrol-earth-syst-sci-discuss.net/12/C5389/2015/

Hydrology and Earth System Sciences Discussions

Interactive comment on "HESS Opinions: Advocating process modeling and de-emphasizing parameter estimation" *by* A. Bahremand

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Received and published: 4 December 2015

I am sure there are very few hydrological modellers who would not agree with the sentiments of this opinion piece. The original development of "physical-based" models in hydrology, from Freeze and Harlan (1969) onwards was driven by similar aims (perhaps somewhat naïvely with hindsight). My PhD thesis, using a finite element solution of the Darcy-Richards equation as the basis for a hillslope model had the same aim, with the parameters being measured in the field (it failed miserably – see HESS 2001). The problem is that there are no real suggestions in this paper as to how to achieve the aims of having more physically realistic models that rely less on calibration.

Yes, by specifying physically reasonable values of parameters the author's experience

suggest that reasonable results might be obtained (but does that imply for the right process reasons that will give correct predictions in future as well as for the past?). I did something somewhat similar using Topmodel in JH 1984 using measured parameters and also declared some success. But there is also plenty of evidence that there may be many such parameter sets (the equifinality problem), all of which will give different future predictions. And increasing the physical basis of a model will actually make this more likely, since the perfect model would have a semi-infinite number of parameters which still cannot be specified a priori, if only because of the problem of uniqueness of place (e.g. Beven, HESS 2000). So the question is whether a form of model conceptualization can be found for which effective model parameters (those which allow the model to provide good simulations at the scale of application) can be estimated for application in many different catchments with varying characteristics (or actually in catchments with similar characteristics). There is little evidence to support this possibility as yet. It therefore seems more of a simple hope than a realistic ambition.

In practice, of course, we have model limitations AND data limitations – the data issue is not mentioned at all in the paper but again even the perfect model is going to be limited by the inconsistencies in calibration and prediction data (e.g. Beven and Smith, ASCE JHE, 2015) – so the success of a model run with a priori parameter estimates might depend more on the (unknown) errors in the data than on whether the model is a realistic representation of the processes.

So it would seem as if this piece needs some more thought and reflection on how the author's suggestion might actually be made to work? What would a new model formulation look like (I offered some thoughts on this in HESS 2006,see also Martyn Clark et al. in WRR 2015 and Weiler and Beven, WRR, 2015)? What would parameter estimation algorithms look like (pedo-transfer functions are not a good example to follow, I am sure the associated uncertainties and incommensurabilities are not what the author had in mind)? How are different model formulations to be properly tested as hypotheses given uncertainties in model evaluation data (there are already disagreements about this – eg. Clark et al WRR 2011; and comment by Beven et al. 2012)?

The author seems to consider GLUE as another model calibration method but in fact it was originally designed to deal with the problems relevant here, since I first started discussing these issues in my JH 1989 paper. It is, however, a framework that rejects the idea of optimization (for reasons discussed in Beven and Binley, 1992, and many papers since – see particularly JH, 2006, and CRAS Geosciences, 2012). But it can incorporate many different ways of evaluating models, even qualitative evaluations and, in its recent limits of acceptability formulations can take account of expectations about the effects of data limitations. As such it already seems to be the type of framework that the author is calling for, especially since it also allows prior information about parameters to be specified where that information is available. But experience suggests that this will in no way eliminate the expectation of equifinality unless some very strong assumptions are made (and in that respect the LISFLOOD example included by the author is actually a simple example of poor practice, why did the prior distributions assumed not specify that flood plain roughness must be greater than channel roughness?).

I see that I am listed as having provided comments on this paper. That is not actually correct. I did provide some brief comments on a quite different, albeit related, paper comment. Discussions are continuing with the author on this one!

Keith Beven

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 12377, 2015.