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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Hydrological models are mediating models" *by* L. V. Babel and D. Karssenberg

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Review of Hydrological Models are Mediating Models by Balbis and Karssenberg

Given my past involvement in Topmodel, SHE and the philosophy of environmental modelling, then it is a little surprising that the authors did not check their interpretation of the history with me before submitting this paper. They did not, and in places their interpretation is not correct. It is also somewhat galling to see some of my papers cited with reference to positions that I was criticising in those papers (e.g. P10537 Lines 23, 25).

The positive aspect to the paper is that it could be quite a useful summary of thinking on models as mediating structures between theory and observations, and could serve

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as a reminder of past discussion of the topics. However, the authors several times say that this is new, but I had picked up on the concept of mediating models from Morton's paper (and communicated with him) and already discussed the idea in my 2001 philosophy paper (which they cite without mentioning that, even though he commented on and is acknowledged in that paper). Indeed they suggest that the idea went unnoticed which surely is not the case on the basis of the papers they cite!!!!, and in fact this role of models, particularly in relation to calibration, is effectively mentioned in earlier papers even if without the use of the term. The 1989 Changing Ideas paper, which they also cite, discusses the physical basis of SHE and already (nearly 25 years ago!!) suggests that it is essentially a conceptual model. I therefore disagree with their suggestion that the discussion in this paper is fundamental (para. 2 of the Introduction) and consequently with some of the subsequent discussion.

They miss the discussion of this in relation to the SHE model in the Refsgaard and Abbott book (1996) and there are other relevant papers which they do not cite (Beven, WatSciTech 2005; HESS 2006; HESS 2007). They do not directly consider the role of the perceptual model in shaping different approaches to modelling (they do mention missing processes but they do not mention my suggestion in 1989 and in Rainfall-Runoff Modelling that all models should be considered as conceptual models; they do not note the direct empirical background of all physical theory in hydrology (Manning, Darcy etc) to the extent that we have recently suggested that the term physically-based should actually be avoided (Beven and Young, WRR, 2013).

Their interpretation of the history of both Topmodel and SHE is wrong in some aspects, but they are right about the conditioning on available computer time. Mike Abbott's whole idea in driving the SHE initiative was exactly to move towards a Hydrogeosphere type model. To say that this could not be conceived at the time is entirely wrong – but what was possible was conditioned on computer power. In the early days of SHE there was a lot of discussion about how best to simplify the discretisation of the model to allow catchment scale applications (I did not agree with the choices made at the time since

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I knew it would lead to numerical problems in coupling the saturated and unsaturated zones from my thesis work – something solved later in the 3D versions of SHETRAN and MIKE-SHE when computer time was available but not mentioned here). The later totally non-physical applications with grid scales up to 4 km was similarly conditioned on computer time, but without discussion of the prejudice to the physical basis of the model at the time. It is also wrong to say that SHE was not conceived as applicable to unguaged basins (P10550) – that was exactly one of its driving motivations (in the same way as for land use change such as the deforestation mentioned).

Topmodel is presented as a purely conceptual forecasting model, but it is not actually either. It might have been better to go back to one of the ESMA type conceptual models, such as the original Stanford Watershed/HPSF model as an example of a purely conceptual model as used here. Topmodel was based on very specific subsurface theoretical approximations (as discussed in the Beven, 1997 and Kirkby, 1997, papers cited; and similar in fact to assumptions made by Horton in 1938). The authors suggest that this "immediately limited" the application of the model – which is true in the sense that the assumptions have to be valid (certainly not the case everywhere for global land surface parameterisations but perhaps not too bad in some catchments) – but is this not the case for all "physically-based" models?

Similarly, the surface and subsurface components of Topmodel were originally structured to allow parameters to be determined by inversion from measurement (e.g. Beven et al., JH1984 - NOT by calibration against discharges as with a purely conceptual model, though this is certainly how it is often applied) but this is also true of all "physically-based" model parameters since most of the parameters are not themselves theoretically based, except by inversion (itself an interesting facet of the mediation process).

But this was quite unlike the lumped conceptual models available in the 1970s when Topmodel was developed, particularly in that the distributed outputs from the model could be checked against observations as in the original 1979 paper and others since.

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The 1979 paper also gives an example of how the theoretical concepts of the model were compromised by model calibration. It is also not a forecasting model (it could be, but it is not the approach I would take for forecasting) but has always used as a simulation model (again see Beven and Young, WRR 2013 for the precise use of terms).

In this paper there is only one mention of model calibration (the hope that SHE might require less calibration) and not a single mention of the word uncertainty. But surely both are absolutely critical to the role of hydrological models as mediating models (and hypothesis testing etc etc). This, of course, brings in a whole other body of literature (e.g. recent discussion of the Clark et al WRR 2011 opinion piece; the authors might also look at Young WRR 2013 for a different view of the problem).

Some specific comments.

P10541 – how could one indeed possibly gain any form of novel knowledge not yet encompassed in textbooks from their manipulation? How could we attempt to test the applicability of theories, if models were nothing else than theory? And similarly, how could we aim at representing the world, if models really were the world?

Good philosophical questions- but here we are concerned with hydrological modelling practice and the authors do not explain the relevance to modelling practice here.

P10549 laid at should be lay at

P10550 hence called for a fundamentally different model structure than SHE

Not really – both models try to predict surface and subsurface flow components and both can be used to predict impacts of major land use changes (for Topmodel see, for example, Buytaert and Beven, WRR2009). They just do so in different ways. The original SHE model had no transport components while forms of Topmodel have been used to make water quality predictions (Robson et al. HP 1992; Page et al. HP 2007).

P10553 Virtual realities

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Are not new (only the name is recent and even then it appears before the references cited – see Bashford et al. HP 2002, but I am not sure we were the first). I first published hypothetical simulations based on a "physically-based" finite element model in Earth Surface Processes in 1977. See also Binley et al WRR 1989 and many studies from other authors. I also do not know of a virtual reality study that is a model of the entire system as the authors suggest. They all have simplifying assumptions of one form or another.

P10554 Obtaining measurements was one of the objectives of Topmodel.

Not really sure what this means. Yes parameter values can be derived from measurements but the vast majority of Topmodel applications have not done this but have calibrated the model using calibration. Perhaps read the Death of Topmodel paper and the discussion about calibrating against spatial water tables in Rainfall-Runoff Modelling a little more carefully.

P10556 Conceptual models are designed from the beginning to match particular local conditions.

This might be the case for data-based models (ANN, SVP, DBM) based entirely on input-output observations at a particular site but not for most conceptual models (SWAT for example) that are usually presented as having some generally applicable structure but which require calibration for local conditions (but then so do physically-based models). There is a form of conceptual modelling for which this might be true – the FLEX or FUSE approach, but then local calibration and uncertainties will be important again in the choice of a local model structure over any others.

P10557 This approach of modelling renders it clear that the "final", or "perfect" model will never be reached (Beven, 2001a):

This again appears to be a mis-citation. In that paper the concept of the perfect model is raised in the context that if it requires calibration then it will be subject to equifinality

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– no suggestion is made that such a model might exist.

It seems increasingly clear that the authors have not done any hydrological modelling. While that does not preclude them from making some valuable comments on the existing literature, it does not seem that they really understand enough about the modelling process, and modelling history, use of particular models and important effects of uncertainty on the mediating process to really make a significant original contribution. I think it is particularly unfortunate that, if this paper is really concerned with hydrological models, that the authors have only chosen to illustrate their discussion with the non-hydrological example of fig. 1

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