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Discussions

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

L. V. Babel and D. Karssenberg

l.v.babel@uu.nl

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We would like to thank Prof. Beven for reviewing our paper. Please find below our response to the comments (the referee's comments are provided in Italic).

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.

We are of course fully aware of the considerable contributions of Prof. Beven in these fields. In compiling our paper we chose to support our statements with references to scientific literature alone (instead of oral communications with researchers), because in our opinion the written literature provides the most precise and objective information regarding the history of model development and the original impetus for the development of particular models. We did an extensive literature study, which included a considerable range of fields (other science fields, philosophy of science, anthropology, hydrology), and carefully prepared our manuscript. When submitting our paper we were aware that our paper could still contain misinterpretations of statements in papers cited, and we are grateful for the comments provided by the reviewers. We will correct misinterpretations in a revised manuscript (for details see below).

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 referee's comment makes us realize that this part of our manuscript could be interpreted differently than first intended. Let us shortly summarize our line of reasoning. The quality (or credibility, one could say) of a model depends on two aspects: a) how well it represents real-world processes (apart from its practical application) and b) how well it performs in practice when fed with observations or calibrated using observations. Regarding the first aspect, physically-based models can be considered superior to conceptual models, because physically-based models use process equations derived from physical laws. These laws are universal, unlike the model equations and parameters used by conceptual models, which often depend on the study area. This argument strongly supports physically-based models, which is why many hydrologists consider physically-based models potentially (i.e., regarding aspect a.) more valuable. After re-reading Beven (1989, 2001), we are in the opinion that Beven had/has a comparable opinion (regarding a. above). We would like to stress that we refer to the quality regarding aspect a. above on p. 10537, lines 16-27.

We are aware that when confronted with data (i.e., aspect b above), physically-based models have considerable problems particularly those of equifinality and uniqueness, as described for instance in Beven (2001). We did not intend however to refer to this aspect on p. 10537, because this is treated in the remainder of the paper (e.g., p.

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10545).

In a revised version of the paper we will rewrite this part of the text to clarify this point and reconsider the references provided. We will also avoid the use of ‘higher level of detail’ (p.10537, l. 22) to refer to physically-based models because this may refer to the model equations or the discretization used (and is thus confusing). Physically-based models often use a high level of detail regarding the spatio-temporal discretization but this is not always the case, as in for example Reggiani (1998).

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 as a reminder of past discussion of the topics.

We agree that the paper summarizes many ideas that were discussed in the past, and by citing key papers we have tried to give others the credits they deserve. In our opinion, the paper is however more than a reminder of a past discussion. To our knowledge, the paper is innovative because it provides the first account of hydrological models as mediating models, using the definition of Morgan and Morrision (1999), which has been applied to many other disciplines but thus far not to hydrology.

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.

We fear that there might be confusion here about the concept of mediating models, as addressed in this paper. The term “mediating models” was indeed first used by Adam Morton in 1993. However and as presented extensively in section 2, our study is based on the later work of Morgan and Morrison (1999), who considerably enlarged

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the initial terminology in the aim of defining a global philosophical approach of models. In this regard, the "mediating models" definition of Adam Morton should be considered as a sub-component of the broader concept, as explained by Morgan and Morrison (we refer here to p. 36 of their 1999 book). Key concepts where Morgan and Morrison extended Morton's first use of the term are:

1) Morton did not consider observations in their entirety, but only the data provided by the model itself. That is, models are mediating between the underlining theory and the model outputs. The term is broader in Morgan and Morrison's account, where observations are also considered.

2) Morton solely addressed the "world to theory" mediating function of models; however, in no place does his initial article state where this mediating property originates. The three model characteristics addressed by Morton (purpose-relativity, incompatibility and epistemic boundedness), which hydrological articles (most of them by Prof. Beven) indeed extended, are not explicated in relationship to the mediating property.

3) The fact that models are "mediating" appears for Morton to be one characteristic among others, but does not explain the nature of models and their function.

This is where the work of Morgan and Morrison represents a considerable novelty. In collaboration with many authors from as disparate fields as economics, physics and chemistry, Morgan and Morrison attempted to define a complete philosophical account of models – encompassing not only their function, but also, and maybe above all, the very question of their nature. We believe in the relevance of the mediating model account for hydrology. We do not aim at dealing here with precise modelling issues; rather, we would like to take one step back and look at what our models, from a philosophical point of view, really are.

The referee's comment is very useful in that it highlights two weaknesses of our article. First, we should underline the evolution of the "mediating models" concept of Morgan and Morrison in regard to Morton's first use of this term, as the latter author might be more familiar to hydrologists. Second, we will acknowledge Beven's development on Morton's work, which relates to the early stage of the later, enlarged concept and should therefore be included.

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.

The value of our contribution lies in the fact that we evaluate hydrological models against the 'mediating models' concept, as described by Morrison and Morgan (1999), which is a philosophical account of models that has so far not been dealt with to its full extend in the hydrological literature. Unlike the model of Newton's pendulum and the economic barometer discussed in the first part of our article, hydrological models are mostly not located at "extremes" (i.e. end members) of apparent physicality or data-dependency. It is therefore not uncommon to see physically-based hydrological models be coined at times as "conceptual", and conceptual models defined elsewhere as physically-based. In our opinion, this permanent confusion supports our main argumentation: models of both classes are too similar in their nature and construction to be completely separated. By transcending all types of models, the "mediating models" account overcomes this confusion, and is therefore relevant to the hydrological community.

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;

We will cite these papers in a revised version of the manuscript.

We use the term 'physical intuition', introduced by the philosopher Winsberg (2010) to refer to the role of perceptions of individuals in model construction. This is discussed on p.10550, l. 11 – 19. We would like to thank the reviewer for his references related to the same aspect (Beven, 1989, 2000, 2004) and will include this in a revised version of the manuscript.

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).

The notion of "theory" is considered in a broad sense in the "models as mediators" concept. Theory should be understood primarily in its opposition to observations, and hence encompasses also laws, theoretical notions and concepts. In this regard, the empirical background of hydrological theories does not enter in contradiction with the account presented in this article. The article of Beven and Young (2013) was published after this manuscript was submitted and we will cite it in a revision of the manuscript.

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.

Our choice of words is unfortunate in this sentence, and we welcome this remark. By "conceived", we did not mean that a similar type of models could not have been imagined, but rather that it could not have been developed or run (at the required resolution, for sufficiently large areas and time periods, including calibration and uncertainty analysis). The initial term will be modified.

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).

We did not state that SHE was not conceived as applicable to ungauged basins, but it is true that we did not mention this application explicitly. This will be corrected.

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?

We would like to stress that our aim is explicitly not to enter any polemic about a classification of hydrological models as "conceptual" or "physically-based", since we show that any distinction between both classes is somewhat artificial, and that a global account of hydrological models, such as offered by the "mediating models" concept, is required. We are aware of the theoretical approximations on which Topmodel is based, which is why we state (p. 10547, lines 21-24) that it could obviously not be considered as an empirical extreme of hydrological modelling.

We fully agree with the referee that "physically-based" models encounter similar limitations as "conceptual" models upon application. This is what we discuss p. 10547 (l. 19-20) and more extensively p. 10556 (l.12-22). We conclude that both terms refer at best to the timing at which theory (or observations) are steering model construction.

Similarly, the surface and subsurface components of Topmodel were originally structured to allow parameters to be determined by inversion from measurement (e.g.

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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.

At no point in the article did we write that the parameters of Topmodel were determined by calibration against discharge. We explicitly stated (p. 10547, l. 4-8) that one aim of the model was precisely to guarantee for the physical significance of the parameters. As emphasized above, we would like to insist once more on the fact that we do not aim at labelling Topmodel (or any other model mentioned in this article) as purely "conceptual" or "physically-based". We are showing the similarities between both traditional "classes" of models (i.e. their mediating properties) in order to transcend this distinction.

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).

The authors are aware of the important role of probabilistic modelling in hydrology, and have applied various techniques themselves. In this article, we chose to leave this out, because it does not essentially change the position of hydrological models regarding the mediating models concept. The use of probability distributions for components of a model (be it the model structure, parameters, state variables, or boundary conditions) or observational data has critical effects on how models and observations are compared or combined. It does not change the position of hydrological models however as mediators between theory and the world (i.e., observational data). We would like to thank the referee for suggesting the references, particularly Young (2013) provides an interesting account of models, placing conceptual models and physically-based models (both as defined in our manuscript) in the same group of deductive models (see also Beven and Young, 2013). We will include these ideas in a revised manuscript, particularly because it allows us to define conceptual models (and data based models; see our comment below) more precisely. It does however not change our account of hydrological models as mediating models.

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.

This quotation should be put back into its context – the end of section 1, which provides a review of the history of philosophical accounts of models. No remark on hydrology is made at this point. The questions formulated here – which are purely rhetorical - underline the drawbacks of the syntactic and semantic views discussed earlier in the same section. We now understand that their rhetoric nature is not obvious and will make it more explicit in a revised manuscript.

P10549 laid at should be lay at Will be corrected.

P10550 hence called for a fundamentally different model structure than SHE Not really

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– 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).

In section 3.1 and 3.2, we solely address the construction of models. We hence restrict ourselves to their intended application – as presented in the scientific articles accompanying their initial description – which motivated their structure and construction. Once the model is developed, the range of applications varies considerably over time, and modifications of the model structure can be made to match new functions. Only the initial impetus for model development is however decisive for the initial model structure, on which our focus lies. On the basis of the original literature (Beven and Kirkby, 1979; Abott et al., 1986), we identify different intended functions for both models (p.10549-10550), which necessarily called for different modelling approaches and model components. The fact that both models were later used for similar applications is not in contradiction with this analysis.

We have chosen to construct this article using three well-known hydrological models and analyzed each point regarding these three models. The impact of the original objectives of the model on its construction could however be exemplified in a more extreme way if two models would be considered that were build with totally different objectives in mind. For example, a modeller aiming at forecasting soil moisture using a distributed model assimilated with remotely sensed soil moisture, will use a detailed description of unsaturated zone processes, maybe neglecting (or simplifying) other processes such as ground water flow, as these are considered less relevant and only slow down the assimilation scheme. On the other hand, somebody aiming at gaining fundamental understanding of the interaction between groundwater and the unsaturated zone water, will model both zones and interactions with great detail, accepting longer model run times. This point is briefly discussed on p. 10549, l.26-29.

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P10553 Virtual realities 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.

We would like to thank the referee for the additional references provided, which will be included in a revised manuscript. We did not write that virtual laboratories were new, and merely attempted to provide the reader with recent examples by citing the papers of Weiler and McDonnell (2004) and Sivapalan (2005). By writing that virtual laboratories were "models of the entire system under study", we meant that these models attempt to include as many processes as allowed by the constraints detailed in section 3.2. From the entire development of section 3.1 and 3.2, it should be clear that we do not consider any hydrological model as entirely deduced from theory. We write explicitly that simplifications are inevitable (p. 10545, l. 25).

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.

The confusion appears here to arise from the word "measurements". We use this term in section 3.3 in relationship with the "theory to world" mediating direction of models. When used for simulation, models provide additional "data" about the current, past or fictive world (e.g. discharge values for a hypothetical rain event). The term "measurements" can hence be equaled to "model outputs". This terminology goes in line with our aim to characterize models as instruments of scientific inquiry. However, the referee's

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comment makes us realize that the word can be misinterpreted; we will define it more clearly in a revised manuscript.

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.

To avoid any confusion on what follows, we would like to note that our definition of conceptual models (p. 10537, l. 20-21) englobes both models that appear to be steered principally by our perceptions of the system (generally coined as “conceptual models”), and models based mostly on empirical data (which the referee refers to here as “data-based models”). This classification goes in line with the general focus of the “mediating models” account, which lies on the influence of “theory” and “the world” on the nature of models and their construction. Indeed, perceptions and measurements can both be considered to be derived from the domain identified here as “the world”, as opposite to “theory”.

The referee’s comment underlines the difficulties arising from the traditional distinction between conceptual and physically-based models. The degree of theory and observations encompassed by hydrological models indeed follows a continuous gradient, along which no separation appears possible. The meaning of the word “local” evolves along this gradient too. For data-based models, the valid range of application will typically be very limited in space (because processes vary between catchments), time (because processes may change, e.g. due to land use change), and research question (because data-based models typically include only variables required for a subset of applications only). As the dependency on theory increases, the valid range of application will progressively be extended. At the extreme of physicality (such as the Newton's pendulum presented in section 3.1), the term "local conditions" will ultimately refer to the entire globe, any time period, and any research question, one could say.

The line of reasoning on p.10556, l. 10-22 is that models derived to a greater extent from data (such as the data-based models mentioned by the referee) will not necessitate much "weakening" of the theoretical assumptions upon application, as these are such that they match the available observations (and their spatio-temporal resolution). That is, the degree of physicality (or dependency on data) will stay constant when the model is confronted to the real world. However, when the model relies more extensively on theory (i.e. more physically-based), the available observations will often not match the requirements of the model (particularly regarding the large number of attributes, mostly at a high resolution, required by the model). As a result, interpolation, up- or downscaling of data, or sometimes even modifications of the model structure are necessary in order to apply the model to the real world, hence leading to a decrease of physicality upon application.

We realize that the continuous gradient associated with the valid range of applications along the data-theory axis has not been presented into sufficient detail in the article. We will include it in a revised manuscript.

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

We were not sufficiently precise with placing the reference at the correct position in the sentence. The sentence should have read: 'This approach of modelling renders it clear that the "final", or "perfect" model (as described in Beven, 2001a) will never be reached: a model stays..'. We refer here to the definition (or concept) of the "perfect"

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model provided in Beven (2001, p. 5, 'The problem of uniqueness'), because it exactly describes what we intend at this place in our manuscript with the term perfect model. The referee is correct that he does not explicitly state in Beven (2001) that such a model might exist, but it was also not our intention with the reference to suggest this. In a revised manuscript, we will leave out the reference.

It seems increasingly clear that the authors have not done any hydrological modelling.

This is not correct. The second author has experience in the development of model construction tools, hydrological model construction, upscaling, error propagation modelling and model – data integration techniques. A simple search in a bibliographic database would have revealed this. The first author holds an MSc in hydrology and has a background in philosophy and anthropology.

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.

Our article represents the first attempt to transpose the "mediating models" account developed by Morgan and Morrison (1999) to hydrological modelling. While this account has previously been applied to very disparate scientific fields, such as physics, economics and chemistry, its suitability to hydrology had to our knowledge not been evaluated before. The innovation of our paper does however not limit itself to presenting a new philosophical account of scientific models to the hydrological community. It additionally offers an original approach to overcome the limitations of the traditional distinction between "conceptual" and "physically-based" models, which the referee's comments underlined strongly.

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

The three hydrological models considered in this study are widely known in the hydrological community and do not need further illustration using a figure, in our opinion. However, we do not expect hydrologists to be fully familiar with the economic barometer first developed in 1919. In particular, the term "barometer" could induce some confusion about the real nature of the economic barometer – a mere graphics representing the evolution of economical indexes over time. It is to support the understanding of section 3.1 that fig. 1 was inserted, and not to illustrate our discussion (which fig. 2 does).

References

Abott, M. B., Bathurst, J. C., Cunge, J. A., O'Connell, P. E., and Rasmussen, J.: An introduction to the European Hydrological System – Système Hydrologique Européen, "SHE" 1: history and philosophy of a physically-based, distributed modelling system, J. Hydrol., 87, 45–59, 1986

Beven, K. J.: Changing ideas in hydrology – The case of physically-based models, J. Hydrol., 105, 157-172, 1989.

Beven, K.J.: Uniqueness of place and process representations in hydrological modelling, Hydrol. Earth Syst. Sci, 4, 203-213, 2000.

Beven, K.J.: How far can we go in distributed hydrological modelling?, Hydrol. Earth Syst. Sci.,5, 1–12, doi:10.5194/hess-5-1-2001, 2001.

Beven, K.J.: Robert E. Horton's perceptual model of infiltration processes, Hydrol. Process., 18, 3447-3460, 2004.

Beven, K. J. and Kirkby, M. J.: A physically based, variable contributing area model of basin hydrology, Hydrolog. Sci. J., 24, 43–69, 1979.

Beven, K.J. and Young, P.: A guide to good practice in modelling semantics for authors and referees, Water Resour. Res., accepted article, 2013.

Morgan, M.S. and Morrison, M.: Models as Mediators: Perspectives on Natural and Social Science, Cambridge University Press, Cambridge, UK, 1999.

Morton, A.: Mathematical models: questions of trustworthiness, Brit. J. Philos. Sci.,

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44, 659– 674, 1993.
Reggiani P., Sivapalan M., Hassanizadeh S.M.: A unifying framework for watershed thermodynamics: balance equations for mass, momentum, energy and entropy, and the second law of thermodynamics, Adv Water Resour. 22(4), 367–98, 1998.
Sivapalan, M.: Pattern, process and function: elements of a unified theory of hydrology at the catchment scale, in: Encyclopedia of Hydrological Science, edited by: Anderson, M. G., Wiley, Chichester, UK, 193–219, 2005.
Weiler, M. and McDonnell, J.: Virtual experiments: a new approach for improving process conceptualization in hillslope hydrology, J. Hydrol., 285, 3–18, 2004.
Winsberg, E.: Science in the age of computer simulation, University of Chicago Press, Chicago, USA, 2010.
Young, P.C.: Hypothetico-inductive data-based mechanistic modeling of hydrological systems, Wat. Res. Res., 49, 915-935, 2013.

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