

Interactive comment on “Uncertainty, sensitivity analysis and the role of data based mechanistic modeling in hydrology” by M. Ratto et al.

M. Ratto et al.

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We would like to thank the Reviewer for providing interesting points of discussion, that allow us to further clarify and specify aims and methodological choices. All the comments on the DBM and TOPMODEL approaches included in the answer to the first Reviewer also apply in the present case. We would like, however, to further clarify specific elements that relate to the present reviewer's comments.

1. First, a general point. On the basis of her comments, we would speculate that the Reviewer, while very familiar with TOPMODEL (which she has probably used), does not appear very familiar at all, and will not have utilized, DBM modelling. For instance, she says 'It is not clear how the decomposition of flows has been done in

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the DBM model', despite the fact that this has been presented in most of the published papers on DBM modelling over a number of years (see later). While we think it is perfectly understandable that she does not know much about DBM modelling and we will amplify the description in the paper to cover some of the points she raises so that she and others may be better informed, we believe the general tone of her comments displays a rather uninformed and thus unfair bias. We will try to compensate for this bias in our comments below on more specific aspects of her discussion.

We might point out that the authors of the paper include three academics from Lancaster, where the originators of both the TOPMODEL and DBM modelling approaches reside and where the continuing development of both modelling approaches has continued over many years. So we have no reason to consider these models as competitive in any way. The aim of the paper is certainly not a competitive comparison between DBM and TOPMODEL (or mechanistic/physically based modelling in general), it is to provide a unified outlook on hydrological modelling in which the different methodological approaches show (see the Abstract) "... good synergy: combining well to produce a complete modelling approach that has the kinds of checks-and-balances required in practical data-based modelling of rainfall-flow systems. Such a combined approach also produces models that are suitable for different kinds of application. As such, the DBM model can provide an immediate vehicle for flow and flood forecasting; while TOPMODEL, suitably calibrated (and perhaps modified) in the light of the DBM and GSA results, immediately provides a simulation model with a variety of potential applications, in areas such as catchment management and planning."

2. Later on, the Reviewer comments that

"... many efforts are done to get the TOPMODEL producing similar results than the DBM model (uncertainty bounds/partitioning of flows/model performance). A lot of confidence is given towards the DBM model! The other way around would make more sense to me ..."

This exposes the bias of the Reviewer and misrepresents our aims in the paper. There are two major differences between the two types of model: (i) the measures of uncertainty; and (ii) the flow partitioning. In the light of a unified approach to hydrological modelling, this has the following implications:

- As far as the first item is concerned, we think that DBM provides a relatively objective benchmark about the structure of the model and its associated uncertainty. This is because DBM modelling is based on as few *a priori* assumptions as possible and develops the DBM model within the context of a generic class of models widely accepted by hydrologists, namely linear or nonlinear differential equations. As a result, it provides a well identified, statistically sound yet physically meaningful *description of the observed data*. In the mechanistic calibration context, on the other hand, the GLUE approach contains elements of meaningful subjectivity, so allowing the modeller to interact in the modelling process by constraining the model to have a specific form prior to calibration (actually this is also possible after the initial, more objective DBM modelling but we do not discuss this in the paper). This is of course, both a strength and a weakness, and it is achieved by relaxing some elements of full Bayesian estimation. This has been a reason for unfair criticism by overly enthusiastic Bayesian modellers who do not see the limitations of their 'rigorous' approach. One qualifying element of our paper is that we provide an objective benchmark for uncertainty prediction, which, in conjunction with Global Sensitivity Analysis (GSA), allows us to *pre-calibrate priors* in order to eliminate dynamical features that have little effect on the model output and would be rejected by the DBM approach.
- Concerning the second item, the route we follow is mainly dependent on the specific case analysed. Under the case study considered, it seems to us that, from Figures 6-7, the quality of the surface runoff and saturated/groundwater flow generated by TOPMODEL is to be expected because of the nature of the model. However, it has some features that we felt required comment. Specifically, the

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spikes in the saturated/groundwater flow seem to suggest that high frequency components of the surface processes are conveyed in this component. For this reason, we analysed the conditions under which the groundwater flow is affected by these surface processes. It is worth noting that the opposite approach can be used, where the physically-based model (here TOPMODEL) is analysed to determine its underlying dynamic behaviour. For instance, we have used the DBM approach for *Dominant Mode Analysis* (DMA: see Young, 1999). Here, a DBM model is identified and estimated from simulation data obtained from experiments performed on the the physically-based model. This then acts as a surrogate or emulator of the model, allowing the analyst to distinguish the key dynamical features of the mechanistic model. In the present context, however, GSA results, in themselves, seemed adequate to support calibration of the TOPMODEL and DMA was not considered.

3. Sensitivity analysis Here, the Reviewer asserts:

“There is a lot of emphasis on sensitivity analysis (in title, large part of literature review), while it has a minor role in the research that has been presented.”

and later on she also comments that:

“SA is also used out of the context of uncertainty analysis (e.g. to support calibration/to improve understanding in the model behaviour).”

These comments seem rather strange. We thought we had been quite clear about the major role of GSA in the context of the paper. Moreover, the use of GSA for calibration purposes is explicitly declared throughout the paper. For example, we say in the Abstract:

“The bottom-up approach is developed using the TOPMODEL, whose structure is evaluated by global sensitivity analysis (GSA) in order to specify the most sensitive and important parameters; and the subsequent exercises in calibration and validation are

carried out in the light of this sensitivity analysis. GSA helps to improve the calibration of hydrological models, making their properties more transparent and highlighting mis-specification problems.”

Later in the Introduction we say:

“In the calibration (model identification and estimation) framework, the understanding ... of the influence of different uncertainties on the modelling outcome, becomes a fundamental question. ... Sensitivity analysis (SA) can play an important role in this framework: it can help in better understanding the model structure, the main sources of model output uncertainty and the identification issues (Ratto et al., 2001). For instance, Pappenberger *et al.* (2006a,b) and Hall et al. (2005) have recently presented cases that achieve such an understanding for flood inundation models, using sensitivity analysis to support their analysis.”

Again, in the Section on GSA methods, we explicitly link methodologies to calibration on page 3110:

“ Both the FP and the FF settings are extremely important in the calibration context: FP matches the need of highlighting the key input factors driving the uncertainty of the model predictions and possibly reducing them; while FF matches the need to identify irrelevant compartments of the model that, subsequently, can be simplified. ... ”

And further on, *Regionalised Sensitivity Analysis* (RSA) is also clearly linked to calibration.

Concerning the definition of sensitivity analysis on page 3107, we are convinced this is fairly general, even for calibration purposes (and in fact all GSA methods are linked to calibration). For example, if variations of the output cannot be attributed to a subset of input factors, the latter have no use for calibration.

The Reviewer also finds some mismatch between the GSA methodologies described and applied. We do not agree with her: we have actually applied all the methodologies

described, from variance based (main effects), to Elementary Effects and to RSA. As far as explaining why GSA was chosen, we thought that the clear links between GSA and calibration discussed throughout the paper were a sufficient motivation for applying it.

Finally, the Reviewer also asks why GSA was not applied for DBM. This again exposes the Reviewer's unfamiliarity with DBM modelling and we are grateful of the opportunity of explaining this point. The reason is simply that the DBM is a statistically-based modelling approach that contains aspects of sensitivity analysis inherently within the statistical processing of the data. In particular, the process of model structure identification ensures that the model is identifiable and minimal: i.e. it is the simplest model, within the generic model class, that is able to adequately explain the data. As such, it provides a parametrically efficient (parsimonious) description of the data in which all the parameters, by definition, are important in sensitivity terms, thus negating the need for sensitivity analysis.

4. Uncertainty analysis Here, the reviewer would have liked to see more attention to uncertainty analysis in the literature review. This is partially deliberate: given the subject of the current special issue and the subject of the other contributions, we thought that our key contribution concerned the use of DBM and GSA for uncertainty issues. But, given the Reviewer's comments, we will now include more information about the uncertainty implications of DBM.

The reviewer's 's sentence:

"The uncertainty analysis results are compared to each other while they are not comparable: ..."

is rather ambiguous. We do not simply compare uncertainties but, due to subjectiveness in the GLUE approach (mentioned also by the Reviewer), DBM is used to provide an objective quantification of the 'noise'. This is in fact one of the key role of DBM

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for uncertainty analysis. In response to the Reviewer's question about noise, the 'noise' identified in DBM modelling represents that part of the data not explained by the model. It can be quantified in various ways (e.g. simply by the variance of this noise, or its variance in relation to that of the model output, by a coefficient of determination; by a stochastic model, if such a model is applicable (see later); by its probability distribution; by its spectral properties etc.). Of course, it can be due to multivariate reasons: measurement inaccuracies; limitations in the model as a representation of the data; the effects of unmeasured inputs; etc. Sometimes, if it has rational spectral density, this noise can be modelled stochastically (e.g. by an AutoRegressive, Moving Average (ARMA) process) but this is not always applicable when dealing with real data. Consequently, the 'instrumental variable' identification and estimation methods used for DBM modelling are robust in the face of noise that does not satisfy such assumptions (see e.g. Young, 1984).

5. Model performance We agree with the comment on model performance, above all in validation. The very same considerations mentioned in the answer to Reviewer Number 1 apply here.

6. Flow partitioning As mentioned earlier, this topic has been discussed in most of the published papers on DBM modelling. The decomposition and its associated partitioning are a natural and totally objective mathematical decomposition of the estimated model which has a nice interpretation in both dynamic systems and in hydrological terms (see e.g. Young, 2005). It is a function of two mathematical properties of model: the eigenvalues, which define the residence times of the 'stores' in the parallel pathways; and the steady state gains of these stores, that define how much flow passes through each store. In fact, two such decompositions are possible (identifiable): a parallel decomposition, as reported in the paper; and a feedback decomposition. However, in line with the tenets of DBM modelling, this latter decomposition can be rejected because it has no clear physical interpretation. It is clear, therefore, that the partitioning

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of the DBM model, as identified in this manner, is inferred from the data by the DBM modelling methodology and it is not imposed on the model in any way by the modeller, as in other approaches to rainfall-flow modelling (e.g. compare the HyMOD model of Moradkhani *et al.* (2005) of the Leaf River, where the model structure is assumed by the modellers, with the DBM model of Young (2006), which is based on the same data set but where a similar but subtly different structure is inferred from the data)

7. We will clarify the notation in equation (8).

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