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Interactive Comment

Interactive comment on "Uncertainty, sensitivity analysis and the role of data basedmechanistic modeling in hydrology" *by* M. Ratto et al.

M. Ratto et al.

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We would like to thank the anonymous referee for his kind comments and for a stimulating review which raises a number of points that we would like to address below.

General remarks

1. We agree with the referee that the two models considered in this paper are primarily intended to address different applications but we thought we had made this clear at various places in the paper. For instance, in the introduction we say,

"*The results of the exercise show that the two modelling methodologies have good synergy; combining well to produce a complete modelling approach that has the kinds of checks-and-balances required in practical data-based modeling of rainfall-flow sys-*

tems. . . . snip . . . As such, the DBM model provides an immediate vehicle for flow and flood forecasting; while TOPMODEL, . . . snip . . . provides a simulation model with a variety of potential applications, in areas such as catchment management and planning".

2. As regards the referee's other general remarks, we feel that the following statement is rather ambiguous:

" . . . using two different modelling approaches, respectively, a data-based model, and a physically-based model"

This suggests that the *specific* data-based model considered in the paper (namely the DBM model) is a modeling 'approach'. It is certainly true that DBM modeling, *in general*, is an approach to modeling systems (not only hydrological systems) but this DBM approach is much more than the DBM hydrological model considered in our paper. DBM modeling is *not* limited to the synthesis of forecasting models: *it is a generic, objective-orientated approach to modeling stochastic, dynamic systems*, so that it can equally well produce a model for simulation purposes, if this is the desired objective. Indeed, the main difference between the DBM forecasting model, as considered in the paper, and an alternative, parsimonious DBM simulation model, is the nature of the effective rainfall nonlinearity. In the forecasting model, this nonlinearity is identified from the data to be flow-dependent (it is a 'state-dependent parameter' DBM model), with the flow obviously acting as a convenient surrogate measure of catchment storage and so limiting the model to flow forecasting, as noted by the referee. In an alternative *DBM simulation model*, however, this nonlinearity would be dependent on an unobserved or 'latent' storage state obtained, for instance, from some conceptualized cathment storage equation.

Illustrations of these different types of DBM model are given in Young (2001, 2003). In the first of these publications, three DBM models are considered: namely, one with a flow-dependent effective rainfall nonlinearity, as in the present paper; one that is, effectively, a modified version of the well known IHACRES 'hydrid-metric-conceptual' (HMC)

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model (Wheater et al, 1993); and, finally, one that is modification of the Bedford-Ouse model (the HMC progenitor of IHACRES). The first model is designed primarily for stochastic forecasting applications, although it also sheds a lot of light on the ’dominant modes’ of the system: i.e. the most important, physically interpretable, characteristics of the catchment dynamics. The latter two models are stochastic, dynamic simulation models and can be applied in applications that require either single or Monte Carlo-based simulation. Moreover, although all three of these DBM models are identified and estimated on the basis of the same rainfall, flow and temperature data, the DBM modeling approach is not limited to such data and it is possible to conceive of alternative DBM models of an HMC type that utilize other data, such as evapotranspiration information or even DTM data, as in TOPMODEL.

3. It is also important to note that the DBM forecasting model can often provide a useful input into identification and estimation (calibration) of conceptual simulation models. In particular, it provides an estimate of effective rainfall that can be utilized in evaluating alternative conceptual storage equations in HMC models. For instance, the DBM model with a flow-dependent effective rainfall nonlinearity is normally obtained quite quickly and yet it often provides a better explanation of rainfall-flow data than alternative conceptual models (as in the present paper). Consequently, the conceptual storage model can be evaluated by comparing its effective rainfall output with the effective rainfall series produced by the DBM forecasting model. Indeed, initial estimates of the storage model parameters, that would be useful in subsequent calibration of the complete catchment model, could be obtained by optimizing them against the measured rainfall and this DBM model’s estimate of the effective rainfall series.

4. In another statement, the referee says:

"The physically-based model can, however, also be used as a forecast model, but in this case better performance can be obtained by updating the model with recent runoff observations using data assimilation (the same information as used by the data-based model)"

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This is true but, in the paper, we were comparing like-with-like: i.e. both models *considered within the same forecasting context without adaption or data assimilation*. Naturally, *both models* should benefit from the addition of such on-line modifications and, given the results in the present paper, it is likely that the DBM-type model would remain superior in these forecasting terms. An example of a real-time DBM model-based forecasting and data assimilation system that includes adaptive parameter updating algorithms has been described recently by Romanowicz et al (2006) and Young et al (2006).

5. We hope that the synergy between models that we refer to in the paper is further emphasized by the above, additional comments and that any idea that the models are in some sense competing against each other is dismissed. Since both types of model have been utilized very effectively for many years in our R&D program at Lancaster, we would certainly not wish to give this impression. There is never a single model of any real system and a primary objective of hydrological systems analysis should be to select the model *that best suits the nature of the defined modeling objectives*, be they forecasting, simulation or a combination of both. Restricting our conclusions to just the two models considered in the paper, past experience suggests that the *specific* DBM-type forecasting model considered in the paper has advantages in the context of real-time forecasting; while TOPMODEL has advantages within a simulation context. An evaluation of how alternative DBM-type simulation models may compare with TOPMODEL in such a simulation context will be the subject of future research.

Specific comments

1. The reviewer asks to comment on the validation results. We agree with the reviewer that the length of the calibration period of the rainfall-flow models should include widest possible range of inflows and usually, 3 months are not long enough when the robust model for the flow predictions is sought. However, for the purpose of this paper (i.e. presentation of two complementary approaches to rainfall-flow modeling) these lengths of period for the calibration and validation of the models were sufficient, in particular as

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hourly measurements were used. As the reviewer rightly noticed, the validation results were not very good, but we did not aim for the comparison of best model results. In this example we used only one raingauge station, which is not fully representative for the rain amounts contributing to the flow at Hodder Place (Young, 2003).

2. Concerning the elementary effect method of sensitivity analysis, we apply a development and refinement of the Morris (1991) method. We depart from Morris' original idea in two ways (Campolongo et al., 2006):

- the sampling strategy of Morris is improved as follows: once the number of trajectories r has been chosen, we generate a number $r^* > r$ of trajectories applying 'standard' Morris (1991) design and retaining for model evaluation the subset r that provides the best exploration of the input space, i.e. to avoid oversampling of some levels and undersampling of others that often occurs in the standard Morris procedure;

- the standard 'Morris' procedure computes the mean (μ) and standard deviation (σ) of elementary effects, while we only compute the mean of the absolute values of the elementary effects, that we call μ^*. For screening purposes, this is actually the only measure needed, since this alone is able to provide negligible input factors ($\mu^* \approx 0$) while, in the standard Morris approach, one has to look at the bi-dimensional plot in the (μ, σ) plane and selectively screen the parameters lying towards the origin. Of course, μ and σ can be used to make some guess about non-linearities or interaction effects, but this goes beyond the scopes of screening applied in the current paper. Moreover, it can been seen that is the best proxi to Sobol's total indices (Campolongo et al., 2006).

3. The sample size requirements of GLUE applications are maily linked to the convere-gence of the cumulative distribution functions (Pappenberger et al., 2003 and Saltelli

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et al, 2004, pages 153-155) and thus to the ‘success rate’, i.e. the percentage of samples that provides good fit. Given the pre-calibration approach and the small number of parameters, the sample size was sufficient for the current application. This also shows that careful sensitivity and pre-calibration analysis allows for a reduction of the computational costs of GLUE exercises.

4. We are very grateful to the referee for noting a number of errors and omissions in the paper. These arose because the deadline, coupled with the multi-authorship, led us to rush the final checking of the manuscript and so miss the errors and omissions:

- The simulation results of the TOPMODEL-GLUE analysis, as shown in all figures Fig. 5, are derived from the posterior mean and 95% uncertainty bounds.
- The referee is perfectly right with respect to the curves shown in Fig.1: the dash-dot line is the parametric estimate and the full line is the non-parametric estimate.
- The operator z^{-1} in Eq. (8) is not defined: this is the backward shift operator: i.e. $z^{-r}y_t = y_{t-r}$. This should not be confused with z used in Eq. (10).
- The referee rightly noticed inconsistency in the value of the coefficient of determination for the data-based model. It is 92.3% and it is well be corrected in all three pages.
- The referee is correct on page 3113: two sentences are more or less repetitions: (i) “During the summer months water abstractions from the reservoir strongly influence the flow”, and (ii) ”During summer time, the flow is affected by abstractions from the reservoir situated in the catchment”.
- We agree that the figure referencing is confusing. Fig. 5 and Fig. 8 are referred to before the first reference to Fig. 4, and later Figs. 6-7 are referred the first time (page 3118).

- we are very grateful to the referee for all technical comments. These will be corrected in the revised version of the paper.

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