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Hydrology and Earth System Sciences Discussions

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

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It is a well written paper on an interesting topic.

The two reviews have been very thorough and have addressed very fundamental issues. After having considered both the two reviews and the author responses and after having studied the manuscript carefully I have decided that the manuscript should be subject to a moderate/major revision before it is accepted. The authors should take the following points into account when revising the manuscript:

(1) Reviewer comments. Please consider all the reviewer comments carefully, as also indicated in the author replies to the two reviewers.

(2) Notion in Eq(8) - including the issue of simulation/data assimilation. I agree with Referee #2 that it appears from the manuscript (Eq 8) as if the measured streamflow is used as input, i.e. that DBM runs in updating/data assimilating mode and not in simulation mode as TOPMODEL. I suppose from the size of the explained variances that it is not updating mode (then it would typically be 99% or more). I have also noted in the author reply that this is not the case, but this needs to be made very clear in the manuscript, so that other readers are not confused.

(3) Model performance in calibration and validation periods As also stated by the authors on pp 3105-3106 a model is only acceptable if it has been successfully conditionally validated. Fundamentally, the model performance should be evaluated from the validation tests and not from the calibration periods. I therefore do not agree when the authors state in the final discussion (p 3124, lines 17-18) that the DBM model 'explains the data a little better' than the TOPMODEL. For the validation periods the explained variance is 75% for both models. That the DBM do a better job than the TOPMODEL for the calibration period is irrelevant, as it mainly illustrates its curvefitting capability in calibration. I think the authors should acknowledge this in their discussion.

(4) Number of figures could be reduced There are 12 figures with hydrographs. And most of them are very much alike. I think that the number could be reduced without loosing much relevant information. One possibility that the authors should consider is to show only hydrographs for the validation period (max one figure from the calibration period), as this in my opinion is the most relevant measure of model performance. Then it would be relevant/interesting to show all model performance figures (explained variances) in a new table, both for calibration and validation. In this way an easier overview of all model runs is provided - now the reader has to leaf through all 12 figures to see/compare the model performances.

(5) Same y-axes on figures The same y-axes should be used for DBM and TOPMODEL simulations for the same period. The figures showing simulated/observed hydrographs for validation periods have axes going to 3 x 10-3 (Fig 4), 7 x 10-3 (Fig 8), 3.5 x 10-3

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(Fig 12). This makes comparison by eye very difficult for the reader.

(6) Flow partitioning I agree with the authors that it is impossible to expect the same flow partitioning of the two models because of the very significant differences in model structures (e.g. p 3124 lines 5-7). But I would go one step further and state that it is impossible to state which of the two are better and that they are artefacts of their respective model structure and do not necessarily represent field conditions. From a theoretical point of view the only thing for which the two models have been conditionally validated is for flow predictions. There is no field evidence at all to support the flow partitioning and therefore I would consider both of them speculative - there is certainly no evidence to support one of the two model' partitioning above the other. From a practical point of view (based on my own experience with many different model structures ranging from black box to distributed physically based), I have learned that you cannot trust a model's representation of flow partitioning when compared to field conditions. Also I do not understand why it is important and relevant - if the only model application is flow simulation (none of them are validated for other purposes and therefore have no documented capability for other types of applications). It may well be that the partitioning in DBM is mathematically objective as stated in the manuscript and in response to one of the reviewers (Ann). However, this does not tell anything about the physical realism of the two resulting DBM flow components, only that it mathematically is optimal, with the given DBM structure, to separate the total flow in this manner. As stated by one of the reviewers (Ann) the authors appear to consider DBM more physically correct than TOPMODEL. I cannot see why - it is not supported by any field evidence and is pure subjective. I would think the other way round (more preference to TOPMODEL), but I accept that this is also subjective. Therefore I strongly recommend that the authors discuss this in a more balanced manner.

(7) Flow uncertainty - GLUE calibration The first TOPMODEL calibration is described as giving rise to 'volatile predictions' (e.g. p 3123, line 22 - but also other places in the manuscript) and therefore the uncertainty of the streamflow data are reduced from

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max 20% to max 10%. This is done without at all discussing what a realistic value would be. I suppose that in a practical case information on actual uncertainty on flow data would be obtained and used instead of this purely hypothetical data manipulation. If you continue along the same line and reduce the flow uncertainty to e.g. max 1% I assume that the TOPMODEL predictions would appear with much less uncertainty, but what is correct? What does these uncertainty ranges actually tell - if you can just change the uncertainty range as you like. I lack a discussion on this issue.

(8) Role of DBM models in hydrology The title indicates that an important aspect of the paper is to elaborate on the rle of DBM models in hydrology. The only text on this in the manuscript appear to be subjective statements on the potential applicability of DBM and TOPMODEL respectively. As the two models have very similar performance (75% versus 75%) I cannot see that these elaborations are supported at all by the results form this case study - and the elaborations on this issue therefore appear a bit isolated from the remaining part of the paper. I recommend that the authors consider to remove (or reduce) the text on this issue.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 3099, 2006.