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Hydrology and Earth System Sciences Discussions

Interactive comment on "Rainfall-runoff modelling in a catchment with a complex groundwater flow system: application of the Representative Elementary Watershed (REW) approach" *by* G. P. Zhang and H. H. G. Savenije

Anonymous Referee #1

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Review of hessd-2005-0017 by Zhang and Savenije

General Comments

This is an interesting paper in which the REW approach is applied to a watershed in Belgium. It is another attempt to apply the REW concept to a real world watershed. The authors made modifications to the model structure and contribute to the ongoing discussion regarding what level of complexity is useful/possible in representing watersheds. The paper is generally well written and provides a valuable contribution to the literature, however, in its current form; it leaves at least 3 questions open that the authors should address. These are in general:

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Interactive Discussion

Discussion Paper

[1] What is a level of performance which would lead to the conclusion that the model is a satisfactory representation of the watershed system? Or, in other words, what is a threshold for a model to be 'behavioral'?

- The authors use a lot of qualitative statements to express their satisfaction with the model performance. These are purely subjective and the authors should state explicitly how these statements translate into quantitative statements (e.g. what Nash Sutcliffe value is needed for an accurate hydrograph representation in the author's opinion?). Not everybody would judge an NSE value of .71 or less as satisfactory.

- Only 4 years of streamflow data are used, which is very short for the type of analysis shown. Particularly if a split sample test is performed. Some researchers concluded that longer time-series are needed for model calibration. Particularly, since there must be some spin-up time that the model requires. The authors should discuss this.

- The authors apply the model using 73 REWs, though (input and output) time-series data is only available at the watershed outlet and they all have the same parameter values. How does the model perform with less REWs? Is the level of spatial detail justified given the available input data?

[2] How can (or should) a (semi-)distributed model structure be evaluated when only observations of streamflow at the watershed outlet are available? And what length of time-series is necessary for this purpose?

- The authors make multiple modifications to the REW scheme: [1] the addition of interception, [2] an improved transpiration scheme, and [3] improved saturation-excess flow area formulation. In addition, they simplified the momentum balance equations by ignoring inertia terms.

- It would be more informative if the authors would introduce these changes in a step-wise manner, instead of all at once, so that the impact of individual modifications becomes apparent.

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- The authors should further think about how these changes can be assessed in a way that shows that they lead to an improved representation of the underlying system. I think that purely an improved NSE value is insufficient in this regard. It is difficult to separate the impact of simply having more parameters, and thus a more flexible model, to actually improving the model structure. Figure 10 and the related discussion is a good start, but more than a single plot would be very valuable. The flow duration curves are of limited value in this context either, as discussed below.

[3] What evaluation (sensitivity analysis) and calibration tools are appropriate for complex and non-linear hydrological models?

- The authors use a first order perturbation analysis to test parameter sensitivity. This approach ignores parameter interactions and its limitations should be stated.

- While the authors only calibrate 6 parameters, these are assumed the same for 73 REWs. The author's should discuss the justification for this particular approach. I understand the need for computational efficiency, but other approaches are possible, e.g. multipliers on the parameters to simply maintain spatial consistency. A physically based approach should allow for some information to be used in the setting of parameters for different regions, even if it is only expert opinion based.

RECOMMENDATION: The authors should revise their paper by including major revisions to the analysis performed, a more detailed justification of approaches and methods chosen, and by quantifying their statements regarding the model's performance assessment.

Specific Comments

- The authors state that this paper is the first full REW application to a natural watershed (p.640). What about the paper by Reggiani and Rientjes?

- "However, it has been realizedĚ" (p.642). The authors might want to expand this statement and explain what this means. It is not clear to me from this sentence.

- The authors state that previous REW applications showed unconvincing results (p.643), at least with respect to full applications. I'd like to know how the authors define a convincing result! Many hydrologists would say that Nash Sutcliffe Efficiencies of .71 or below are unconvincing as a justification that a model represents a watershed's response well (more about this below).

- (p.658) The authors apply a simple perturbation analysis to test model output sensitivity to parameter variation. This method has clear limitations and ignores parameter interaction. The authors should discuss these limitations and state why they have chosen this approach in contrast to more general global sampling approaches like Regional Sensitivity Analysis.

- (p.657-660) The authors use a step-wise approach to parameter calibration in which they first use a manual and then an automatic stage. This is different from other approaches, e.g. the one by Boyle et al. that the authors mention, in the sense that the order of manual and automatic steps is reversed. The reason for having first an automatic and then a manual step is that the automatic procedure can sample that large parameter space much more efficiently for good parameters, while the second step reduces the difficulty in defining an objective function that accurately captures the fit that the modeler is trying to achieve. It is not clear to me what the benefit of a reversed order would be! If the authors simply mean that they manually adjust the feasible parameter ranges, then this step should not be called calibration in my opinion.

- The authors should provide a few sentences on how GLOBE works so that it becomes possible to judge the applicability of the chosen method and the reliability of the results. (p.659-660)

- (p.661) "Clearly, Ě well captured" & "Ěquite accurately reproducedĚ". These are qualitative statements that require from the authors to define what they mean in a quantitative sense.

- (p.661) The authors use a flow duration curve on normal scale to conclude that low flows are better reproduced than medium flows (Figs. 7, 9, 12). I do not believe that this type of plot allows this conclusion. If the difference of errors in different flow ranges is of interest that the percentage error would be preferable. Absolute differences are likely to show better low flow behavior. Also, a log scale would be more helpful in evaluating low flow performance.

- (p.661) In addition, the authors use an objective function that favors parameter sets that reproduce high flows correctly, yet the conclusion is that low flows are represented best?

- (p.661) Why does the analysis focus primarily on subsurface parameters? Are the conclusions regarding parameter sensitivity reliable if a local sensitivity analysis approach is used and parameter interaction is ignored?

- (p.663) "Scanning all rainfall Ě" What do the authors mean by the statement that this point is an outlier and cannot be used? Unless this value is occurring due to a measurement error than it should be included in the analysis. An exceptional event is particularly interesting in testing whether the model represents the watershed hydrology appropriately. Unless this data point is an error, the authors should discuss why the model does not reproduce it, e.g. due to limitations in the flow range used for model calibration.

- (p.664) How do the authors define "convincing results"?

- (p.664) The authors state that the watershed is affected by pumping and artificial drainage. What is the affect of this human interference with the watershed and on the hydrograph, and why is it not considered in the model formulation?

- (p.664) "Judging by the Nash-Sutcliffe efficiency Ě reasonably accurate." Again, what is the threshold for this conclusion?

Technical Corrections

- P. 659: Douglas et al. should be Boyle et al.

- Fig. 5 should be improved in quality.

Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 639, 2005.

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