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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

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We are very grateful to the anonymous referee #1, who has made a thorough review of our manuscript and raised insightful and critical questions. These comments provide not only guidance for improving the quality of the manuscript but also a contribution to a further discussion on the evaluation of catchment models. We carefully studied the comments and present our response accordingly. We shall revise our manuscript in accordance with the referee's comments in as far as possible. These modifications will be presented in the final version of our manuscript.

General Comments

[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 0.71 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?

Model performance evaluation is to analyse the degree of closeness of the model behaviour matching the behaviour of the real-world system. The evaluation is then subject to the objective function(s) and the performance measure(s). In General, the objective function(s) and the performance measure(s) are chosen depending on the purpose of the model application and issues to be investigated. However, the choices are often subjective with respect to the performance measure(s). Typically, when the GLUE approach (Beven and Binly, 1992) is applied to assess the uncertainty, a threshold value for the likelihood function (e.g. Nash-Sutcliffe efficiency) has to be assigned in order to differentiate the behavioural model (not necessarily a good model) and non-behavioural model, or model parameter sets. The threshold value is usually arbitrarily chosen and it mostly ranges from 0.3 to 0.6 in applications (e.g. Freer et al, 1996; Muleta and Nicklow, 2005; Campling et al, 2002; Beven and Freer, 2001).

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Indeed, given the method applied to the model evaluation in our work, the evaluation of the model performance is also subjective. We applied a threshold value 0.6 as a level to evaluate the model as behavioural.

Certainly, some researchers wouldn't judge a model with Nash-Sutcliffe efficiency of 0.71 or less as a satisfactory model, especially from the operational point of view, or compared with some more fully developed conceptual models. However, in the course of the studies on the REW approach, the approach has shown its promise in hydrological applications although it is still in the infant stage of model development and application. Comparing the results of previous model applications and having seen the improvements, and knowing that the model presented here is still under research, the performance is acceptable in the authors' opinion.

Concerning the length of data for calibration, we agree that it would be better that the time series would be longer enough to cover the whole range of temporal variations of the catchment characteristics (i.e. information content). Franchini and Pacciani (1991), Sorooshian and Gupta (1995), Wagener et al. (2004), among others, have discussed the dependency of data quantity (or the length of the time series) required for parameter estimation on the model parameters to be estimated. There is, however, a general consensus that the information content in the data is more important than the amount of data (e.g. Muleta and Nicklow, 2005). In practice, constrained by the data availability or any other reasons, the length or the number of observations used for model parameter estimation is sometimes shorter than desired. In our case, we found that in the 4-year of data that were available for this study, wet and dry (although it is not as distinctive as that in arid or semi-arid areas) period, high and low flows are covered. Further, there is little documentation reporting on drastic changes in climate and land use, which could lead to changes of catchment rainfall runoff relations. Therefore, we think that the quantity and quality of the data applied in this particular case, given the goal of the paper, is justified.

In this work, the spin-up effect (model initialisation) has been taken care of before calibration. We first run the model using one year of the time series and obtained the final states of this run. Then we use the states as the initial states for the model calibration so that the initial effect was reduced to large extent.

Since the REW approach makes use of spatially averaged quantities (geometric properties, state variables and parameters) over the sub-watershed (REW) scale, the lower the resolution of the catchment sub-division is done (fewer REWs), the more risk that essential catchment heterogeneity (e.g. topography, geometry and soil) disappears. The catchment heterogeneity, however, is a crucial factor determining the flow pathways and hence the responses of the catchment to the forcing. To preserve as much heterogeneity as possible without losing much computational efficiency, we found out, based on our experience, that the spatial discretisation with 73 REWs is reasonable. We have carried out modelling excises with a lower level of spatial discretisation (resulted in 15 REWs). But we didn't observe better results out of those excises with 15 REWs compared with those with 73 REWs.

We will reformulate the relevant parts of the manuscript according the above comments.

[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 stepwise 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.*

We agree with the referee that a step-wise approach to introduce the model complexity is more informative to evaluate the effect of each individual modification. We actually have followed this thought in our work. The improved saturation-excess flow area formation was discussed in the work by Zhang et al (2004a, 2005). The current paper is to present the significance of interception and a rather full picture of the REW application.

All inertia terms of the momentum equations have been ignored ever since the REW approach was coded in the first model version. This is acceptable since hydrological flows are generally slowly varying (low velocities). Even in rivers, the inertia term can generally be ignored leading to kinematic wave approximation.

With regard to evaluation of the REW approach (semi-distributed) based model, using data measured at interior flow gauging stations to validate the model (e.g. Senarath et al, 2000) is probably a desirable way. Using groundwater level and soil moisture measurements could also help constrain the parameter space. One should, however, keep in mind that with the REW approach, parameters and state variables are averaged quantities over the sub-watershed scale, thus giving rise to a difficulty of comparing the simulated quantities with point measurements. Even so, the scarcity of such measurements prevails. Particularly for this study, such internal state measurements are not available.

[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.*

Regarding the approach to sensitivity analysis (SA), many techniques are available and they can be grouped into three settings (see e.g. Saltelli, 2000): factor screening, local SA and global SA (often applied as regional sensitivity analysis, RSA, in rainfall-runoff applications). The Fourier amplitude sensitivity test (FAST) and Monte Carlo simulation based methods (e.g. Hornberger and Spear, 1981; Beven and Binly, 1992; Beven and Freer et al, 1996) are among the popular RSA approaches, which found their applications mostly for conceptual rainfall-runoff models. Apparently, local SA approaches don't take into account the effect of parameter interactions. RSA approaches implicitly account for the parameter interaction effect and can explore a higher dimensional parameter space. However, few reports in rainfall-runoff modelling have explicitly discussed the effect of parameter interactions on the parameter indentifiability. In fact, as Bastidas et al (1999) and Wagener et al. (2004) pointed out, most SA approaches are weak in dealing with the issue of parameter interdependency.

Although sensitivity analysis is not the main goal of this paper, we used the one-at-a-time perturbation approach to preliminarily investigate the effect of parameter variation on the model output, which is probably a common process of gaining insight into model behaviour when a model is newly built. This approach may be regarded as an expansion of factor screening that is helpful to filter out most important factors affecting model response. On the other hand, due to the "curse of dimensionality" (Brun et al, 2001) and prohibitively high computational demand, RSA approaches are not

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widely applied yet to large complex models. Instead, local approaches are often applied (e.g. Senarath et al, 2000; Newham et al, 2003). One of the rare attempts using RSA approaches is presented by Christiaens and Feyen (2002) in which the GLUE was applied to MIKE SHE model. In that work, using Latin hypercube sampling, 400 parameter sets were taken (without considering parameter correlations) as input for the hydrological model. But 50% of the simulations failed initialisation or aborted due to a steep decline of the hydraulic conductivity curve. The same authors applied another approach, a linear regression-based sensitivity analysis to tackle the model uncertainty problems for MIKE SHE (Christaens and Feyen, 2002). They provided an interesting alternative of SA for physically based distributed models, however, the drawbacks are also reported, such as neglecting parameter interactions, the inability to transfer the calculated sensitivities back to the original parameters (when the rank transformation technique is used), and the high demand on CPU time. These examples illustrate that a well-accepted approach to SA for large complex models maybe still remains difficult.

The authors fully realise that a comprehensive exploration of the model sensitivity and uncertainty using an appropriate state-of-the-art technique is very important and helpful to model development, however, we would like to leave it to future work. Nevertheless, more discussion will be added to the paper in accordance with the referee's comments with respect to SA issue.

Model calibration (i.e. parameter identification) is a critical procedure determining a successful model application. There is ample literature on hydrological model calibration approaches. Two concepts, uncertainty analysis using the GLUE framework initiated by Beven and Binly (1992) and pareto-optimality solution developed by Gupta et al (1998), are currently mostly adopted in modelling practices. Wagener et al (2003) introduced a new approach, the dynamic identifiability analysis (DYNIA), to complement the traditional calibration methods. Although no established calibration strategy to estimate parameters of distributed models has been well accepted, as stated by Ajami et al (2004), Boyle et al (2000) have proposed an improved calibration strategy that combines the power of multi-objective automatic and manual calibration methods. We adapted the strategy proposed by Boyle et al (2000) although our method is somewhat different from theirs. We shall clarify this in the following with respect to the relevant specific comments.

This paper discussed a simplified application of the REW approach, which is semi-distributed. Although some catchment characteristics (e.g., catchment area, average slope, surface elevation, channel length etc.) are kept distributed, other parameters are lumped. As the referee suggested, indeed, multipliers (as one of the options) could be used to represent parameters' spatial variability, provided that there is some information on the regional differences of parameters even if it is qualitative. Practically, our model is able to allow for such information (spatially varying parameters) to be used in the setting of parameters for different regions. However, due to the lack of knowledge such spatial variability, we think that not much would be gained by bringing in purely subjective information to the model.

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?*

This paper, which is supposed to be a follow-up work of Reggiani (1998, 1999, 2000, 2001), reports the results based on the progressive work carried out by Zhang et al (2003, 2004a, 2004b, 2005). We acknowledge the work by Reggiani and Rientjes (2005), which is cited in our paper. We shall rephrase this sentence.

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

We agree that this statement is not clear enough, as also commented on by referee Erin Zehe. This sentence tries to point out that the means of closing the dependencies of the balance equations in the REW approach is critical. Namely, as Beven (2002)

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discussed about the REW approach, one should recognise that the balance equations alone are indeterminate, and additional functional relationships associated with the simplifying assumptions (i.e., the so called constitutive relationships), for the fluxes within and between REWs, are crucial to the closure of the system. We would like to stress this issue as the closure problem.

- *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 0.71 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.*

Please see the previous discussion within this response.

- *(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.*

Indeed, we took a different way of combining manual and automatic calibration approaches. Boyle et al (2000) first carried out the automatic calibration using MOCOM-UA algorithm to obtain the Pareto solution space and then selected one or more acceptable parameter sets within the Pareto space through manual calibration. The advantage of doing so is obvious, as the referee pointed out, that a group of good parameter sets can be efficiently filtered out by the automatic step. In our case, we have a couple of concerns preventing us exactly following these steps:

1) Our model is not yet an established one, thus an open-eyes manual calibration in the first step would be a desirable way of gaining knowledge of model behaviour; **2)** The model is physically based. We would like, therefore, to calibrate the model within physically reasonable ranges for each of the parameters being adjusted; **3)** Due to the resources available, searching good parameter sets in a large parameter space using multi-objective algorithm with our model is rather difficult at the moment.

- *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)*

We shall do so. In brief, GLOBE (Solomatine, 1995, 1998) is a global optimisation tool consisting of a number of search algorithms: controlled random search (CRS2, CRS4); genetic algorithm (GA); adaptive cluster covering (ACCO), and with local search (ACCOL); multistart methods (e.g. M-simplex) etc.. We choose the M-simplex algorithm for our study, though Sorooshian et al (1993) discussed that the shuffled complex evolution algorithm is more efficient and effective than the M-simplex. Actually, we have tried different algorithms (GA, ACCOL), however, the accuracy gained was very marginal and the computational efficiency was found much lower than that with the M-simplex.

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

Indeed, this statement is qualitative since it is based on the visual inspection of Fig.6. It tries to say that the pattern of the hydrological response of the catchment to the forcing is well represented by the model. Visual inspection on graphs is not a sophisticated, but still widely used way of evaluating model performance in practice.

- *(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.*

The relevant text and graphs will be modified according to this comment.

- *(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?*

In the manual calibration stage, priority was given to fitting low flows as good as possible. In the following automatic calibration, the value of the parameter controlling base flow was kept within a small range so that variations of parameters wouldn't jeopardise the goodness of fit for low flows. In addition, one can see that the differences between high and low flows in the studied catchment are not very large. Therefore, even though the Nash-Sutcliffe efficiency gives more weight to high flows, the matching for low flows is not damaged.

- *(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?*

Based on our experience and knowledge gained in the past work (also from the simple sensitivity analysis), we found out that the subsurface parameters are amongst the mostly influential parameters on the model output. Other research work also showed similar cases, for example, Christiaens and Feyen (2002), and Anderton et al (2002) presented sensitivity analyses focusing on soil hydraulic parameters, respectively for MIKE SHE and SHETRAN; Senarath et al (2000) analysed model sensitivity on 10 parameters for CASA2D (a Hotanian hydrological model), of which 6 are subsurface parameters.

- *(p.663) "Scanning all rainfall E" 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.*

That data point is covered by the flow range used for model calibration and verification as well. Because of this, we think that this data point is probably the result of an inadequate rainfall network (we use only one station). The word "outlier" does not have a statistical meaning. We shall change it and add a short clarification.

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

Please see the previous discussion.

- *(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?*

The groundwater is abstracted from the river basin for the water supply of the city of Liege, Belgium. Due to the abstraction, the groundwater flow regime is different from the natural condition without human abstraction. However, the exact effect on the hydrograph is unknown to the authors. Limited discrete data on the abstraction rate are provided. We synthesised the data as monthly time series, which were used as sink term for the saturated domain of each REW.

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

Please see the previous discussion. We shall reformulate the relevant statements.

Technical Corrections

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

Sorry for the mistake, and it will be corrected.

- *Fig. 5 should be improved in quality.*

We shall improve the quality of Fig. 5.

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