

Interactive comment on “Hydrologic responses of the Zwalm catchment using the REW model: incorporating uncertainty of soil properties” by A. El Ouazzani Taibi et al.

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General Comments

The paper presents an interesting application of the REW concept to the Zwalm catchment. It also presents an interesting calibration approach, making use of the full series of hydrological time series, while doing the validation through the analysis of individual year performance. I like that approach. Even better would have been to validate the model on some internal state variable or on orthogonal information, but that information is apparently lacking. I am of the opinion that the popular split record test is not a good way of validation because 1) it reduces the length of record for calibration and 2) seldom results in anything more than a confirmation of the calibration, without providing

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more information on the performance of the model.

What I am less enthusiastic about is the "uncertainty analysis" presented. What the authors have done essentially (I stand to be corrected if I am wrong) is do a sensitivity analysis through MC modelling. I do not consider that an uncertainty analysis. A variation of the Ks is not compensated by some other process, which is normally done in calibration and which leads to equifinality and related uncertainty of forecasts. What the authors do is vary an input variable and study the range of output resulting from that. That, although more sophisticated as a result of the MC modelling, is still sensitivity analysis. As a result, this part of the article generates a large number of uninteresting graphs and the following discussion is very lengthy. The authors correctly observe that the rainfall is probably the largest source of uncertainty. I fully agree with that, but there is no follow-up on this.

Specific Comments

I think the paper is interesting as an application of the REW in a new catchment, but I think the focus on the uncertainty in soil properties is not correct. The authors do not really present any info on soil variability. As a result I would accept the paper with a revised focus:

1. Remove the sub-title on incorporating uncertainty of soil properties;
2. Shorten section 6 rename it "sensitivity analysis" and describe the sensitivity to the Ks;
3. Remove the figures 10-15 and the lengthy discussion on the MC simulation. A simple Table can do the job;
4. In general be more concise in the formulation. The paper is very long and wordy. It should be more to the point. The lines 16-20 on page 83 can be removed

Technical corrections

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Finally I have a number of more detailed comments for the authors to take into account.

1. The hydrographs presented in Fig 8 and the like are not readable. This is interesting information for a hydrologist. Make them larger and maybe plot the discharge on a log scale, so that the performance can be better assessed.

2. About the superscripts in equations 2-6. I know that in the original publications by Reggiani et al. it is done that way, but it is not in line with the conventions in HESS. Superscripts are exponents. So turn the superscripts into subscripts. That does not change the readability or meaning in any way.

3. Do not suggest a higher accuracy than is possible or necessary in presenting significant numbers. In the description of the catchment there is no need to give the size of the catchment in more accurate numbers than 114 km² and 178 km

4. Use correct units. Rainfall and evaporation are fluxes, not stocks. On page 77 lines 2-3 write 775 mm/a and 450 mm/a

5. In formulas do not use more than one letter for a symbol. So no PBIAS in Eq.(7) but a single parameter with a subscript; for instance B. Also the multiplication in (7) by 100 is nonsense. Everybody knows that a parameter with a value of 0.60 is equal to 60%. In fact 60% is equal to 0.6 so no multiplication with 100. The name "percent bias" is also nonsense. What you mean is that it is dimensionless, but that is rather obvious. You could call it a Bias ratio, or Bias indicator.

6. In the last paragraph use evaporation for evapotranspiration. The term evapotranspiration is neither necessary nor accurate. When in doubt read: Savenije, Hubert H.G., 2004. The importance of interception and why we should delete the term evapotranspiration from our vocabulary. Hydrological Processes, 18(8):1507-1511.

7. The paper still has many spelling mistakes. Please read the paper carefully and correct. For instance on page 70 line 17 Reggiani is spelled incorrectly

So in summary, I would welcome a revised manuscript for publication in HESS provided

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my comments are addressed adequately.

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