Hydrol. Earth Syst. Sci. Discuss., 2, S1184–S1192, 2005
www.copernicus.org/EGU/hess/hessd/2/S1184/
European Geosciences Union

Hydrology and Earth System Sciences Discussions

Interactive comment on "Multi-criteria assessment of the Representative Elementary Watershed approach on the Donga catchment (Benin) using a downward approach of model complexity" *by* N. Varado et al.

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Received and published: 15 December 2005

General Comments

This interesting paper presents a new application of a model based on the REW approach to a real world catchment, in West Africa. The authors assessed the model performance using not only discharge data set, but also groundwater table and soil moisture data sets collected from the Donga catchment. The authors attempted to test the model with two different spatial discretizations as well. In addition, the effects of the spatial heterogeneity of rainfall on the model efficiency for reproducing discharge at various gauging stations are analyzed. This study fits well within the scope of HESS and contributes to the literature of hydrological research.

However, the authors applied a model that is in a primitive stage of development (largely unknown) to a poorly understood catchment in terms of its hydrological behavior (largely unknown as well). This leaves a huge freedom for speculating on whether you are testing the model (and the model approach) using the data collected and your understanding of the catchment (in this case, assuming that the catchment is known, at least partially), or you are trying to gain insight in the catchment behavior using the model as a tool (assuming that the model is known in this case). In the former case, it does not seem logical that you simply took the model of Reggiani and Rientjes (2005), without any modifications (especially the model closure system), who applied it to a catchment where the climate, physiography, geology and hydrology are very different from the catchment presented in this paper, and where the dominant rainfall-runoff mechanisms may thus be different from the ones in this study catchment. In the latter case, however, I see little discussion on how helpful the model has been to enhance the understanding of the catchment behavior, since the paper is mostly focusing on evaluating model performance.

In the model, which is the same as the one Reggiani and Rientjes (2005) used, the saturation-excess overland flow is the main runoff generating process and largely determines the streamflow. As a result, it is the determinating factor for model efficiency with respect to discharge. In the paper, however, I see no indications whether the saturation overland flow is the main mechanism of the catchment. On the other hand, the authors provide little findings and discussions on this aspect.

It appears that some findings/conclusions of this work are not fully supported by the data presented in the manuscript (see the specific comments). In addition, there are a number of flaws with regard to the English language, which requires improvement. Therefore I suggest that this paper be published after these issues have been addressed and revisions have been made accordingly. Although this may involve quite some work, I encourage the authors to resubmit a revised paper because it is a substantial contribution to the literature on the REW research.

Specific comments

ABSTRACT

1. Page 2351, Line 14: "The first goal is...". Where is the second (and the third, if any) goal, or is (are) there the second (and the third) goal(s)? I see that in Introduction, 3 objectives were mentioned. Is (are) the goal(s) the same as the objectives?

INTRODUCTION

2. Page 2352, Line 16: Is the SWAT model a physically based model? I understand that in SWAT some processes are described using the physically based equations, and it is debatable whether a model is physically based or conceptual depending on different definitions. However, the first sentence of the abstract in Arnold and Foher (2005), which the authors cited, states: "SWAT (Soil and Water Assessment Tool) is a conceptual, continuous time model...".

3. Page 2353, Line 14: "The strength of the approach...". I, personally, see no real problems with model formulations. However, I consider the main challenge to be the derivation of proper closure relations with respect to flux exchange terms considering site-specific hydro(geo)logical conditions. Please comment on this.

When talking about the closure problem, it is recommended that the authors should also refer to earlier publications on this issue (e.g., Reggiani and Schellekens 2003, Lee et al. 2005, Zhang and Savenije, 2005).

4. Page 2354, Line 1-13: The authors stated 3 objectives of their study. However, the main part of the work presented here is on the first objective. The other two are not well elaborated in discussions/conclusion whether they were achieved or not.

5. Page 2354, Line 23-24 (also in Page 2365, Line 5-6): What is "the internal model

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structure” exactly referring to? ”To test the model internal structure”, you used the discharge data measured at a few gauging stations within the catchment. But, I only see the evaluation of the model efficiency (see also Page 2365) based on such measurements. Could you provide some clarity on ”the model internal structure”?

MATERIAL

6. Page 2357, Line 22-23: ”These three recent articles describe well the version of the model used in this study...”. I have observed, however, that there are some differences between the model that was applied by Reggiani and Rientjes (2005) and the other two, although the model concept is the same. Please clarify this.

METHOD

7. Page 2359, Line 2-5: You have attempted to simulate the hydrology of the Donga catchment with 117 and 23 REWs ”to see if one is more accurate...”. Table 5, 6, 7 and 8 listed the results of the simulations with the two discretizations. I have observed that the model produced slightly different performance when applying the 3rd order or 2nd order discretization. However, the differences are not significant to me. I would expect that more discussion on the effect of spatial discretization on the model performance (which is actually one of the study objectives stated in an earlier section of the paper) can be provided.

8. Page 2359, Line 18-22: The description of this part is not clear to me: it is stated that every boundary was permeable but with no flux, on the other hand, it is said: ”The possible flux is calculated by...”. I am not sure if the boundary flux was imposed or not. What does ”the possible flux” mean? Was the mean depth of the bedrock for every REW chosen at the same fixed position?

9. Page 2360, Line 5-7: Do you mean that the variables in Equation (2) are transformed using ”square-root”? If so, I suggest that the transformed equation for model efficiency analysis be presented. Does such transformation give a significantly better evaluation

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of model performance towards the discharge in the dry season? It would be better to provide a detailed explanation and the relevant references on such transformed Nash-Sutcliffe evaluation criteria. On the other hand, it is unclear to me how much you can gain when such a bias was introduced as far as the general model performance is concerned, especially in the case that discharges of the catchment in dry seasons are almost none (see Fig. 6 and Fig. 8).

10. Page 2361, Line 24-27: The scale issue is prominent throughout hydrology science. The REW approach, like other approaches, cannot avoid scale problems. To my understanding, in the REW approach, parameters are representative ones for REWs, similar to what is so-called "effective" parameter with the other models. Once you have a different delineation of the catchment, resulting in different number of sub-watersheds (REWs), then the representativity of the parameters for the REWs changes. Therefore, it is logical that parameter values do not stay identical (e.g. Ks) for the different discretizations of the REW-approach-based models. Other than trying to find out if the parameter values change or not, it would be more interesting to look for a scaling relationship. In this paper, only two parameters Ks and θ_s (saturated moisture content) were reported to be calibrated. It is discussed by the authors, however, that θ_s was found to be identical for both cases with order 2 and order 3. I suspect that the values are not really the "true" values found by the manual calibration since the "optimum" may have not been achieved. Further more, it should be taken into account that parameter interactions may play a role in model calibration.

11. Page 2362, Line 6-10: I am wondering why the model efficiency with decadal time step for 3rd order case (0.53) is lower than that with daily time step (0.57), which is quite unusual?

12. Page 2362, Line 15-19: You speculated that the less efficient model performance for the year 2002 was probably due to the fact that this year is the driest year. Have you examined the model structure (in the current form) itself whether it is able to represent the catchment behavior? Although there are no figures to show the results of this year

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(at the catchment outlet), I have looked up Fig. 11 in which simulations for a few other stations for the year 2002 were presented. Apparently, the model failed to reproduce the runoff behavior except for a few big events. Please elaborate more on this model behavior.

You also suggested, based on Table 5, that the 2nd order discretization is better for the model to simulate the discharge at the catchment outlet than the 3rd order discretization. However, if one look at the efficiency indices for the daily discharge, there are no significant differences between the two spatial discretizations. Again, when your model calibration didn't reach the "optimum" (see also Comment 10), such a conclusion should be made with caution.

13. Page 2363, Line 1-12: The authors stated that adjustment of the soil parameters (specifically which?) helped to improve the simulation soil moisture state (Fig. 9). However, I also see that the initial moisture content for the simulation with "surface parameter" differs from the one with "adjusted parameter". How do you evaluate the effect of the initial conditions on the soil moisture simulation? In other words, the effect of one fact on the model performance can only be evaluated when the effects of other factors are (or can be) ruled out.

Please provide references to the statement "...the first meters of soil are probably the most contributive to the streamflow...".

14. Page 2363, Line 18-22: Indeed, the pattern of the groundwater level dynamics and the range of the groundwater level fluctuation for REW No.1 were nicely simulated (Fig. 10). However, my concern is: If the average groundwater levels for REWs fluctuate with a range of about 8 m, the saturated surface area (variable sources area) of REWs would become zero during the dry periods. Then how were those peaks in the dry periods (e.g. one can see from Fig. 11) generated? Another question, if you have 8 m of average groundwater level fluctuation within less than 100 days, how much recharge flux (or rainfall input) would be needed to induce such fluctuation, considering the soil

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porosity of 0.03?

15. Page 2365, Line 10: I would suggest avoiding using the word ”accurately”, which appears quite often in the paper.

16. Page 2365, Line 16-18: Please clarify what the problem of ”spatial discretization and temporal scale” is. On what ground can it lead to the statement ”a finer discretization (1st order) may help to better reproduce the processes on these catchments”?

17. Page 2365, Line 22-25: Based on the data provided by this paper (e.g. Table 6, 8 and Fig. 11), it is hard, in my opinion, to draw the conclusion with sufficient statistical significance that ”the model was able to ACCURATELY simulate...as soon as the drainage area was above 100 km^2”. Moreover, drainage area is not the only one factor to determine the model efficiency. For example, I think that topographical characteristics of the catchment (or sub-catchments, i.e. REWs) is also important.

DISCUSSION AND CONCLUSION

18. Page 2366, Line 26 - Page 2367, Line 3: The-three-month-delay behavior of the catchment was correctly simulated after the calibration, according to the authors. I think, if I understand it correctly, that this is based on the results of the simulated decadal discharge volumes. How about if you look at the results with daily discharges, or hourly data? Due to the averaging effect, data with large temporal scale (e.g., decade, month) generally don't help much to evaluate a rainfall-runoff model, from my point of view.

Even if information on soil horizons is available, upscaling to a representative value for the REW level remains a problem. Even if a detailed unsaturated zone module is introduced to the model that is able to deal with such soil layering structure, the detailed information on the other parameters won't be always available. Therefore, I am afraid that calibration for a REW-based model will always be needed in a foreseeable time period. Please comment on this.

Technical Corrections

Table 4: BC and IC in the category column probably mean boundary condition and initial condition. To be clearer, make notes in or under the table.

Fig. 1: It would be better to label a couple of the neighboring countries' name in the figure giving readers a better orientation reference.

Fig. 4: In the box Zone O, "overflow" should be "saturation overland flow"; in the box Zone C: "concentrated flow" should be "concentrated overland flow". The box of Zone R is crossing the two REWs (i and j), which is not appropriate. If it is wanted to show the river connections of REWi and REWj, I suggest making two Zone R boxes separately connected by an arrow, similar to Zone S boxes.

Fig. 9: It would be better to adjust the position of the legend so that it looks nicer.

Fig. 9 and Fig. 10: Please use English date for the time axis.

Fig. 11: The year indicated in the figure (2000) doesn't correspond to the year indicated in the figure caption (2002).

References

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