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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.

N. Varado et al.

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General comments:
The reviewer questioned the objectives of the study and whether we wanted to test the model using certain knowledge of the catchment behaviour or whether we tried to gain insight into the catchment behaviour using a known model. In reality the answer lies in between. When we started the study, very little was known about the catchment main active processes. The objective was the determination of a spatially distributed water balance in the catchment. Previous studies at larger scale had concluded that there was probably a great deal of water transiting through the groundwater before reaching the river. The principles of the REW model were appealing in the sense that

all the compartments of the water balance were included (and treated with the same mathematical formalism integrated at the sub-catchment or REW scale) and groundwater was considered. Knowing that we wanted to use this model, an experimental design was proposed in order to document the various components of the water cycle in space and time (rainfall, streamflow, groundwater) in a first step and more local measurements in a second step (piezometers, soil moisture along a transect). When we began the study, only the first type of data was available. The poor results obtained for the wells levels showed that some hypotheses were probably wrong in the model. It was confirmed with the second type of data at the local scale, combined with geophysical measurements which modified the perceptual model of this catchment by showing that there was probably not a large connected groundwater system but a lot of small superficial ones. These new data open perspectives for the improvement of the model through the inclusion of perched water table and a better consideration of the soil heterogeneity. Therefore, we think that the study provides a good example of a synergy between modelling and observations, as stated in the second objective of the paper.

Comment 1:
Objectives were reformulated

Comment 2:
The reference to SWAT was removed as the reviewer point of view is correct.

Comment 3:
To my mind, the REW model formulation has three major advantages: namely the resolution of ordinary differential equations (easily solvable), their similar formulations between the REWs zones and local scale equations and the use of REW-average variables. Nevertheless, the assessment of ODE at the REW scale does solve the problem of writing large scale validity equation. Two major problems remain. The first one is the formulation of closure relations, i.e. the flux exchange terms that may vary according to the complexity the modeller wants to insert into the model, to the pedo-climatic context, the dominant processes, etc. And the second but not the least is the

problem of determining parameters at the REW scales because they can not be derive directly from the measure.

Comment 4:
see answer to Comment 1

Comment 5:
Many authors report that the accurate representation of an integrative variable such as the discharge at the outlet is not sufficient to state that the structure of the model is adequate to describe the behaviour of the catchment. This is why most of hydrological modellers try not only to reproduce the discharge at the outlet but also at intermediary station or other internal state variables such a piezometric head, saturation degree in soils, snow height, etc. In my opinion, looking at the intermediary station was a way to evaluate the internal structure of the model. If the model is as accurate on discharge at intermediary stations as at the outlet, it is an important point in the evaluation of the model ability to reproduce discharge across scales.

Comment 6:
The version used was the one developed by Paolo Reggiani during its post-doctoral stay in the LTHE, Grenoble, France. Of course, as many versions as users exist. I made few changes in my version, especially in the infiltration formulation and the relationship between the saturated area fraction and the watertable level. In my opinion these changes did not deserve to be detailed in the article for two reasons. First, I would have to explain every exchange fluxes that were the same as in the previous articles. Second (but linked), the aim of the article was not to present the model but to present a real case study. I did not want to present this work as a model development and, in the objective of a special issue, I knew that some "true" developers would present the details of the model concepts and the fluxes formulation.

Comment 7: (Linked with comment n$^{\circ}$4 and 12)
The question of which discretisation is more accurate for the simulation needs to be

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answered with respect to the objectives of the modelling. The second order for example would be better to simulate the discharge at the outlet of small watersheds. But such a fine discretisation is not necessary if we only want to know the discharge at the outlet of the whole watershed. Knowing which degree of complexity we need to introduce into the models in order to accurately represent our major variables (which are probably site-specific) remains one of the main questions in hydrological modelling. It was one of the objectives of the study, but I agree that it can not be presented as such in the manuscript as the results are not very clear and need more numerical investigation. I made changes in the revised manuscript, developing one main objective and two underlying objectives. The question of which discretisation and which complexity is needed in the model is not completely achieved. Only few elements can be taken from this study.

Comment 8 was taken into account in the revised version of the manuscript by explaining that the boundaries in the saturated zones were considered as permeable. The flux was calculated by the Hardy-Cross algorithm (cf. Reggiani et Rientjes, 2005)

Comment 9:
The Nash-Sutcliffe evaluation criterion is known to give more weight to the large values (Perrin, 2000). To take into account baseflow values, some authors have used logarithm transformation (Ambroise et al., 1995). With zero flow during the dry season we could not use this transformed criteria. Many authors have used root-squared values as for instance Chiew et al. (1993) or Perrin et al. (2003) to derive a criteria sensitive to the whole range of discharges including high and low flow.

Comment 10:
A scaling relationship for the hydraulic parameters is certainly an important issue to reach and a great research field. But in the study, it was not possible to do so with only two spatial discretisations. A scale relationship may be achieved with 5 or 6 levels of discretisations, it means probably on larger watersheds. The calibration of K_s and θ_s was made manually. K_s values ranged between 1.10^{-3} m.s^{-1} and 1.10^{-7} m.s^{-1},

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with a step of an order of magnitude, and refining thereafter. θ_s values ranged between 0.6 and 0.01 $m^3.m^{-3}$ with a step of 0.1 and then 0.01 for the small values. In my opinion, the values of θ_s are too small to find a difference between the 2^{nd} Strahler order discretisation and the 3^{rd} order discretisation. One can argue that the optimum is not reach. Probably, but the optimum might be found in 0.001 intervals which does not mean a lot in terms of soil porosity. One of the perspectives of this work is to apply an automatic calibration method, in order to better assess the parameters, their ranges and the uncertainty of the calibration.

Comment 12:
In the opinion of every people working on the catchment, the year 2002 was an especially dry year. On the contrary, the years 1999 and 2000 on which the model was calibrated are considered as normal, even if they are respectively rather dry and rather humid. It is not chocking that the model do not reproduce the discharge as accurately during extreme periods as during normal seasons. For me, it's not a problem of model structure as the processes did not fundamentally vary between two years. The second part of the comment is answered in comment 7.

Comment 13:
The question of initial conditions in the unsaturated zone was faced by considering that the system always reaches the same state at the beginning of the year. This is consistent which the observations reporting the same level of water tables at the end of the dry season and, more recently observed, the same degree of saturation in soils. But these observations remain largely qualitative. The state of saturation in soil can not be measured in every point of the catchment and even more not at every REW scale. So we chose a method that excludes an influence of the initial conditions that remains largely unknown. As explained in the method section, the system was set at a rather dry state at the beginning of January and the simulation was run three times, using final conditions as initial conditions for the next run. The number of three runs was chosen in order to insure the stability of the simulation, in terms of saturation in

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soil and water table level globally returning to their initial state.

Comment 14:
Even during the dry season, the saturated fraction of the surface is not reduced to 0. This is probably why the model still reacts quite rapidly to the first rainfall. (see answer to comment 7.18 from referee 1)

Comment 16 and 17:
In my opinion, the discharge in upstream reaches could not be captured by the model with a third Stralher order discretisation. This discretisation is too coarse to define properly the limits of these catchments. So, Ara and Bokperou station are only parts of a REW, and not a REW as a whole. To identify these catchments, a finer discretisation is required. With a 2^{nd} Stralher order discretisation, these catchments consist in only one REW. Furthermore, these catchments are reported to have a rapid dynamic, with a concentration time of few hours. The input data (rainfall and PET) used in this study was at the daily time step. We know that, in this region, the rainfall duration is rather of few hours. It means that the intensities averaged at the daily time step are not representative of the instantaneous intensities responsible for the hydrological processes, such as surface runoff. So, if we want to capture the discharge in upstream reaches, we would better use a finer spatial discretisation (at least 2^{nd} Stralher order, but 1^{st} would be better) and hourly or event input. When I refer to the 100 km^2, it does not mean that it is an efficiency factor available for all catchments. But in my application, on my 6 stations, I can observe that below 100km^2 of drainage area, the model does not capture the dynamics of the catchment. On the contrary, above 100km^2 the dynamics is quite well reproduced.

Comment 18
I'd like to insist on the fact that I used daily input data. So to my mind, looking at smaller time step outputs is not relevant. That's why I do not refer to hourly efficiency. Most of my figures are drawn with the ten-day average discharges, because it's easier to figure out the model performance, I mean graphically. Nevertheless, I give all the efficiencies

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on daily, 10-day average and monthly discharge in the various tables. So when I draw my conclusions, it's not only from the graphics but also from all the tables: it includes daily, 10-day average and monthly efficiencies.

Comment 19:
Concerning the problem of upscaling soil hydraulic properties to the REW scale, I totally agree with G. Zhang saying that it remains a problem. In my opinion, it's not only a problem for the REW model but a problem for every physically based model that works on "homogeneous" units (HRU, square units, etc.) The problem is the problem of the high variability of the values (e.g. K_s) and the unknown strategy to undertake the assessment of average values from scarce measurements. For sure, the numbers of parameters will increase if we include a more detailed unsaturated zone module. And the accessibility of these parameters will be even more difficult for the underlying horizons. Nevertheless, when underlying horizons have very contrasted properties, as it is the case on the Donga catchment, it seems to me that including such a module will help to simulate piezometric head in the various aquifers. Once again, it depends on what we wish to simulate. More generally concerning the calibration of soil parameters, I agree with G. Zhang about the impossibility to set all the parameters without calibration. I'm quite sure it's not specific to REW-scale models but this problem exists for most physically based models working on homogeneous units. So the argument saying that physically based models do not need calibration is certainly not relevant, as soon as the problem of identifying model parameters at the modelling scale is not solved.

Other comments, concerning spelling and style were taken into account in the revised manuscript.

Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 2349, 2005.