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Hydrology and Earth System Sciences Discussions

Interactive comment on “A generic system dynamics model for simulating and evaluating the hydrological performance of reconstructed watersheds” *by* N. Keshta et al.

Anonymous Referee #1

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The topic of the paper is very interesting, and it is appropriate to HESS. However in my opinion the paper is not acceptable at the present form. Main concerns are on model, data and results. It may be acceptable after major revision.

Main concern The aim of the paper is to develop a new watershed hydrologic model, but in my opinion the paper does not provide new contributions on that. The model is a lumped conceptual model, not really innovative because it seems similar to old multi-layer models that are present in hydrology text books. For instance the Stanford model (1966) of Crawford and Linsley. Moreover the difference between this model version and a previous model version published by the authors in 2005 and 2007 is not clear. I can’t read those papers, but the authors explained that the only new thing of this model version is the canopy storage module. The authors proposed two approaches. However it is not clear if they used both the approaches, they didn’t provide a comparison of the two methods, and more important they do not have data for testing the approaches. There are so many parameters in the hydrologic model that you cannot say if the canopy interception module is important for the case studies. You need experimental data to test it. I was very surprised by the discussion section, where the authors wrote that the soil moisture is well simulated, and "It has to be noted that many previous efforts of simulating similar sites resulted in agreement between the observed records and the simulated values in trend only, not in magnitude". There is an old literature on soil moisture models from 80’, a lot of these models are available and they can simulate very well the soil moisture in magnitude and for a lot of experimental sites. Finally the model at watershed scale is not really tested, because in the case study the runoff is absent (see Fig. 8).

Comments:

1) Introduction section. It must include a literature review on hydrologic models for soil moisture prediction, and more important for runoff prediction at watershed scale. The authors must explain what is the new contribution of their model and why they are not using existing models. At least they should compare their model with existing models. 2) Pag. 1447 and 1448. cut these two pages. It is enough the scheme of Figure 2. They are just describing a typical hydrological model. 3) Pag. 1449 and 1450: The canopy interception model seems too complex. It compute the evaporation rate from the canopy as the sum of both the trunk and the canopy evaporation. You don’t have data to test the model, and it seems to me that the contribution of the trunk evaporation is not really important. You are just increasing parameterization. Again I can’t see in the paper any data for testing this canopy module and I cannot find any comparison between different canopy models. 4) Soil moisture is usually computed

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as volumetric soil water content (no dimensional). 5) Pag. 1451: SW is the surface water storage. Can it saturate? I cannot see any saturation parameter. Why? 6) Pag. 1453, row 4. wilting point is not the residual moisture content. They are two completely different concepts. The first is a vegetation related parameter, the second is a soil parameter only. For instance, definitions in Kutilek and Nielsen, Soil Hydrology, Catena Verlag, 1994. 7) Pag. 1454, equation 11. it needs more explanations. Is it derived mathematically? How did you estimate it? 8) Pag. 1455. equation 12. again, explain the equation derivation. 9) Pag. 1458. it is not clear the size of the natural watershed in the area of BOREAS. There is written that the area is 1000 km x 1000 km, which is huge. I do not think that you are simulating the whole area with the lumped model. Furthermore details on runoff measurements are not provided. What is the instrument? Finally details on basin characteristics are not provided. What is the basin area? What is the average altitude? Etc. 10) Pag. 1458. cut equations 13, 14 and 15 (definition of rmse, mean absolute relative error and correlation coefficient). they are redundant and obvious. 11) Table 1 lists the model parameters, but I can’t see soil parameters. Saturated hydraulic conductivity? Saturation soil moisture? Residual soil moisture? Why? Are you not using soil parameters in the model? 12) Table 1: again, I ca not see parameters of the Penman equation for evapotraspiration. Why? 13) Pag. 1460, row 4. the authors wrote that the case studies are in arid and semi-arid regions. Please, can you provide more details on climate for demonstrating it? 14) Pag. 1460, row 21: the authors wrote that "RMSE of the GSDW model is lower than the values obtained from the site-specific SDW model". But I cannot see results of the SDW model in this paper. 15) More explanation on the systematic errors in soil moisture predictions during DOY 90-160 for all the case studies. The error is large. If the problem is the snow melt, the snow module is not working. 16) I suggest to the authors to use others case studies for testing the overland flow. Indeed they used a case study without overland flow (see Fig. 8). In conclusion the watershed hydrologic model is not really tested.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 1441, 2008.