Hydrol. Earth Syst. Sci. Discuss., 5, S324–S329, 2008
www.hydrol-earth-syst-sci-discuss.net/5/S324/2008/

Hydrology and Earth System Sciences Discussions

Interactive comment on “Catchment modeling and model transferability in upper Blue Nile Basin, Lake Tana, Ethiopia” *by* A. S. Gragne et al.

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Received and published: 4 May 2008

General Comments

This paper presents a study on the upper Blue Nile Basin. It deals with hydrological modeling of this basin and attempts to discuss the transferability of model parameters from one subcatchment to the other in the study basin. In this paper, the HBV model was used to simulate the two gauged subcatchments. Manual calibration and split-record test were carried out to evaluate the model performance, and sensitivity analysis with Monte Carlo simulations was conducted. The work presented here certainly lies within the scope of HESS and contributes to the body of hydrological literature. However, I still would like to present my critics on the paper, serving as a stimulant for arousing more discussion and, so improving the quality of the manuscript.

The paper does present a hydrological modeling exercise in an area where few scientific hydrological studies have been previously conducted. However, the paper lacks focal point(s) while many issues were touched upon (spatial variability of the basin hydrological characteristics and runoff generation processes, model structure and complexity, model performance, model transferability/regionalization, and model sensitivity) with little in-depth discussion on each of those. In addition, the presentation and organization of the manuscript defintely need refinement to enhance the readership of the paper(please see the specific comments). Therefore, a serious revision for this manuscript is required for a possible publication.

Specific Comments

1. The HBV model code was used for the simulations in this study. The authors should provide the ground, argument or criteria (e.g. software availability, model suitability (i.e. process description), etc.) on which it was selected for the modeling?

2. The authors spent efforts on checking the consistency, stationarity and homogeneity of the data using statistical tests. The tests were performed on the areal monthly data. I am afraid that the statistical properties would have been changed after spatial and temporal averaging, meaning that the test results of the areal monthly data might not reflect or represent the statistical properties of the original point daily observations. The question then, why not test on the original data? The author concluded that the change points and inconsistency appearing before 1993 is due to the poor quality of the data before 1993. How do you support this conclusion? No other causes, e.g. land-use, climate change/variability, would have ever been your considerations?

3. The authors frequently used subjective measure to evaluate the model performance and transferability (e.g. very good, good, satisfactory, and acceptable). Maybe it is advisable to first define a scale of the performance measure before you make a judgment. The authors discussed that the model preformed better for larger time-step (15 days, 30 days) than for daily time-step. This discussion seems trivial since it is so obvious,

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and most (if not every) of the processed-based hydrological models do like that (the larger the time step, the better the performance). It was also stated that from the water resources management perspective the results obtained by transferring parameters of the larger time step model were acceptable. The authors should elaborate this by giving what the common acceptance measure or criteria for water resources management modeling is, based on a proper literature review.

4. The authors made an excuse that the uncertainty analysis is beyond the scope of their study. In my opinion, sensitivity analysis, model calibration for searching optimal parameter set, model complexity assessment etc., are parts of the model uncertainty deal. In fact, the authors did quite large number of Monte Carlo simulation runs for sensitivity analysis. So why not make use of these information to give a in-depth discussion in this regard, rather than just provide a general description of the results and a figure (Figure 6) with little explanation?

5. What is the consideration for taking manual calibration after the Monte Carlo simulations? Why not first manual calibration then MC simulations?

6. Page 814-815, the study area: The land-use type is described as "the dominant land use units are agricultural (65.5%)...agro-sylvicultural (1%) and urban (0.1%)". Commonly, the latter two would not be regarded as "dominant". It would be also good if the total annual rainfall was provided when talking about "rainy season..., in which 70% to 90% of the annual total rainfal..."

7. Page 815-816, materials and methods: It would be advisable to place the "input data" of 3.2.1 (model description and input data) into 3.1 (data screening...) in which the data used in the modeling should be clearly and systematically presented. From the paper, I am not sure which period of data were used for calibration (1994-2000?). In the mean time, it would be better to add some lines in 3.2.1 to explain why the HBV was selected, and more importantly, provide more and clearer description on CRs and the zones. It is not clear what kind of zones (elevation, vegetation) and how they were

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implemented in the modeling (even in Table 4 the information about the zones are not clear).

8. Page 820, 5.1 model calibration and validation: The results showed that the model performed better in validation period than in calibration period. The author speculated that this is caused by the better data quality in the validation period. Have you ever considered that it could be due to the fact that the calibration (especially manual calibration) exercise might not yield an optimal parameter set? One cannot always curse the data (quality) if the other influential factors could not be ruled out. The authors stated that the model efficiencies were generally very good (Reff>0.83) for all three CRs, even though the model overestimated the observed discharge by about 52 mm/a. How significant is this 52 mm/a comparing to the total annual rainfall? Water balance error is also important for the modeling as is one of the objective functions, I assume.

9. Page 821, discussion of discharge modeling: Again, the authors claimed that the poor modeling results can be attributed to data quality. Why were the appropriateness of the selected model (structure) and its compatibility to the hydrological characteristics of the study basin not subject to examination? The authors discussed the 3 factors affecting the ability of the model to simulate the daily runoff. The 1st one is "the spatio-temporal variability of the rainfall could not be observed with the given network (cf. Fig. 1) and errors in areal rainfall estimations translate more directly in poor runoff predictions in the smaller KSC than in the larger UGASC." I am wondering why the imperfection of the given gauging network and errors in areal rainfall estimation would give rise to poorer predictions for the KSC while the two sub-catchments fall in the same gauging network. Form the figure 1, one can see that the elevation the southeastern part of KSC are distinctively much higher than other parts of the basin. Could the topographic effect on rainfall distribution be the one of the actual reasons for that?

10. Page 822-823, "The HBV model with the given model structure could not deal with such a complexity of hydrological processes. A more distributed and process-based model structure (e.g. Uhlenbrook et al., 2004; Wissmeier and Uhlenbrook, 2007) would

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be needed." This discussion is rather general, and one should realize that more complex model structure does not warrant better results, which was partly demonstrated by one of the conclusions made by the authors themselves: "...investigated model structures have only a minor relevance for the goodness of predictions of the catchment discharge"(end of this page). In page 823, "As the increased degree of freedom through more model parameters for the more complex model structures (CR I<CR II<CR III) did not result in better model performance, one can conclude that (i.e. CR I)." How could the authors, by such a simple reasoning, jump to the conclusion "information content available in the input and output data is already utilized in the simplest model structure"? What is the logical link between the information content use and the complexity of model structure supporting the conclusion? In fact, the differences between CR I, II and III are merely the distribution of the spatial hydrological entity to be modeled (i.e. spatial discretization), rather than the model structure for process description, which is all the same for the CR I, II, III. A further elaboration for this part is appreciated.

11. Page 825, 5.2.5 Parameter sensitivity: 1 million of MC runs were conducted, resulting in also large number of "good-performance" runs (i.e. good parameter sets) for the subcatchments. What more could be inferred from this in addition to obtaining an indication of the sensitive parameters? I would suggest that authors take a closer look into and more thinking about the sensitivity results. It may help obtain more understanding of / insight into the model behavior, and ultimately the basin hydrology. "...note a parameter, for which good model simulations were possible for a wide range of parameter values, can still be a significant parameter in a certain parameter set. In other words, changing the value of such a parameter and keeping the other parameter values constant can have an impact on the model performance." Is this implying the parameter interdependency? Please put it more clearly.

12. Page 827, conclusions: the authors complained that the dissimilarities between the two sub-catchments have hampered transferability of model parameters between UGASC and KSC, and hence ultimately regionalization of the model parameters. I

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would say, probably it is a common sense that any catchment on the earth is dissimilar to another. Maybe it is good to look at this problem from a different angle, otherwise prediction of ungauged basins (PUB) would be an easy job.

13. Table 1: please indicate data type (e.g. daily discharge, daily rainfall). The last column: annual mean, 19997 m^3/a and 1975 m^3/a for Gilgel Abay and Koga respectively, are these values or units correct? If the annual mean discharge for Gilgel Abay is 19997 m^{-3}/a, it equals 0.000634 m^{-3}/a, am I wrong?

14. Table 4: zone 1, 2, 3 for each subscatchment, it would be better to have an illustration showing the locations of the zones and provide clearer descriptions of the zones. Why for zone 1 and 3 the parameters are fewer than for zone 2?

15. Figures: the readability of all figures should be improved. Figure 1: provide a reference for the basin, I assume that not every body knows where it is exactly located though the river is well-known; the font of the legend is too small to read. In the text of the paper (page 815) it is stated the there are 3 discharge station while in the Fig. 1 only two appear, typo? The authors should follow the HESS' author instructions to prepare the figures. When printing this manuscript on an A4 and in black-white, one can not distinguish the lines. Figure 4: improve the legend; Figure 6: improve the axis labels and explain (in the text) how the standardization was done.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 811, 2008.