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Discussions

Interactive comment on "The JGrass-NewAge system for forecasting and managing the hydrological budgets at the basin scale: the models of flow generation, propagation, and aggregation" *by* G. Formetta et al.

S. Samanta (Referee)

sudeep@uwalumni.com

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In this paper, the authors present a hydrological model (JGrass-NewAge) that runs within an easily accessible GIS framework. The model is designed using a modular structure with individual model components accounting for various hydrologically significant processes, such as, stream flow, evapotranspiration, snowmelt, description of intakes, out-takes, and reservoirs. The model is potentially useful to the hydrologic modeling community. However, in my opinion, the descriptions in the manuscript are not very clear and precise, so the specific advantages of the model component that

is described and tested (the "Adige" component for flow generation, propagation, and aggregation) are not clearly conveyed. The mathematical terms should be defined more consistently, making clear distinction among calibrated or otherwise specified parameters, observed variables, and other variables such as those used internally for calculations. The goals and methodology of the analysis (data, model configurations used, etc.) should be more clearly described at the outset. The results should also be discussed more thoroughly. Addressing the above issues will help to clarify the advantage of this model with respect to other comparable modeling approaches that might be used for similar purposes. These general comments above are further elaborated in the more specific comments below.

1. p. 946, ll. 12-19 – many modern hydrological models are able to simulate stream flow as a time series (i.e., not restricted to modeling floods or droughts) and are able to provide estimates at various locations. Many process-oriented hydrologic models also employ component structures due to its various advantages and also use the concept of hydrological response unit. Therefore, these capabilities of the JGrass-NewAge model do not appear to be new or different. The authors should clearly convey the specific advantages of their model in comparison to models that have similar capabilities, instead of why these capabilities are important in the context of hydrological modeling in general, as currently done in the preceding paragraphs. Moreover, the ability to provide "statistics revealing the internal (spatio-temporal) variability of some of the quantities analyzed" is very imprecise, while the details of the "innovative informatics" are not readily accessible as the Antonello et al. (2011) is indicated to be in preparation stage. These should be explained in greater detail.

2. p. 949, eq. 3-6 – Explaining the underlying physical concepts in more detail here (in terms of water balance etc. and linking eq. 1 and 2 concepts to these equations) would be useful for understanding the physical basis of the model.

3. p. 949, ll. 10-22 – The superscript 0 of S is not defined. Are S1 and S2 the same as S01 and S02?

4. p. 950, eq. 9 – Is this equation actually used in the model? Its relation to the other equations does not seem clear.

5. p. 951, eq. 10, 11, 12 – Please analytically explain or justify the use of this simplification based on the use of mean and variance of residence times and how the mean and variance values are obtained. Are they simply calibrated values? If k and n are calibrated parameters, as indicated below eq. 10, then what is the significance of eq, 11 and 12?

6. p. 951, eq. 13 – How the terms of the right hand side are calculated and related to the terms in the preceding equations is not clearly shown. It might be better to give the expression used for calculating Qi(t) in addition to its derivative shown in eq. 13. Overall, the underlying mathematics used for integration or scaling from unit area of hillslope to HRU and ultimately stream discharge is not entirely clear to me as I seem to miss some calculation steps. Is it possible to describe the links between stream discharge and processes at the unit area and HRU levels more clearly? Consistent reuse of mathematical terms through the sequence of the equations would help in this regard.

7. p. 952, eq. 14 – The definition of Chezy coefficient seems to differ between eq. 13 and 14. Qsp is not defined.

8. p. 952, l. 16 – How the hypsographic curve is used in the model is not clearly explained.

9. p. 952, ll. 19-20 - Please provide the details about the Passer river basin that is used in this analysis.

10. p. 952, ll. 21-25 – Include an explanation of how the area perimeter relationship, results of the linear regression (the relationship $P \sim A0.489$ is not linear), the mean and the variance mentioned here are used in the context of the model equations described before. The mean and variance mentioned in eq. 11 and 12 relate to distributions of

residence times. If these two sets of mean and variance values are considered related, please include a justification for that.

11. p. 953, ll. 5-15 – Please mention which parameters were calibrated and which were kept constant along with the values used for the summer and winter simulations, so that their significance may be clearly understood by the readers. As the model structure is process oriented, the calibrated parameter values and their differences between seasons may be interesting and informative about the system. In addition, many modern automatic calibration methods provide some advantages (e.g., uncertainty estimates) over manual calibration methods. Is there any specific reason behind choosing a manual method?

12. Deficiencies in data (p. 953, ll. 19-24) and model structure (p.955, ll. 4-5) are cited as reasons for some systematic errors. Such errors are usually more informative for making modeling improvements compared to simple goodness of fit measures. I would suggest showing the systematic deviations more clearly, perhaps using a residual plot, and including a more thorough discussion of such deviations.

13. p. 954, ll. 23-25 – The idea of component based model structure is not new and its use for this model is not really demonstrated in this paper. Also, the details of the "informatics" structure are not described in the paper. Therefore, these may not be considered as conclusions from this study itself. Moreover, good fit to a specific data set may not be considered as sufficient validation of a model, in my opinion. How model predictions relate to the observations should be discussed in more detail prior to the conclusions.

14. p. 955, ll. 1-11 - The conclusions regarding structural "defects" and significance of the differences in calibrated parameter values should be discussed in detail along with the results. The fact that evapotranspiration and glacier outflow were kept constant should be mentioned before reporting the results along with the values they were held at.

15. p. 955, ll. 12-16 – I am not sure that the authors clearly demonstrate that these “statistics of simulation” are different or more reliable than other models or methods for doing so.

16. The captions for the figures should be more descriptive and understandable on their own, as far as possible.

Interactive comment on Geosci. Model Dev. Discuss., 4, 943, 2011.