

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.

P. Krause (Referee)

p.krause@uni-jena.de

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The paper is interesting and eventually relevant for this journal after some more work. Anyway I have the impression that it was put in a little bit too early. Some more iterations and some more thinking about what and how it is presented would really help to make it a good piece of work. I have a number of comments you might like to consider to improve the quality of the paper. Please check grammar and spelling very carefully – right now the English seems to be a little bit odd – but I am not a native speaker, so I might be wrong. Additionally, all figures should be checked for readability (axis descrip-

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tion, legends) and for consistency with the text. Fig. 3 to 6 are not really reflected in the text so either you might omit them or you have to explain and discuss the information provided and why it is relevant for your model.

Abstract: You mention that the system is able to estimate evapotranspiration (line 6) why don't you use this in your model application. Same is true for snow modeling – not mentioned at all in the application part. Snow must be an important component in your catchment.

Page 945, Line 5: “whole set of hydrological quantities” is not very informative, please be more precise what you mean.

Line 29: Please indicate how you define “an acceptable degree of confidence”

Page 946, line 2, 3: I do not agree with the statement “. . . the topology and the geometry of the river network is more important . . .”

Line 4-6: Something is wrong with the sentence “In addition . . . flow velocity” – at least I don't understand what you try to say.

Line 6: either “prediction is” or “predictions are”

Line 7-11: “To this end . . .” – this sentence is too exclusive in my opinion. Of course RS provides very helpful information for spatial distributed modeling but it is not a “must-must” as you indicate in your text.

Line 17: “modification of model parameterisation” is not an innovative informatics as you indicate in your text. This possible and necessary with/for nearly every model that needs to be transferred and calibrated. The mentioned “modification of its parts” is more innovative but there are several modeling framework systems available (e.g. JAMS, OMS, TIME, MMS etc.) which provide such functionality.

Line 23: The verb “confounded” might not really indicate what you had in mind. Think of a more proper word.

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Page 947, Line 10: From your description I get the impression that you are using nearly exactly the same semi-distributed approach SWAT is using. Maybe you like to refer that in your paper and discuss that a little bit.

Page 949, Eq. 3-6 and the following explanations: Please check very careful that you use the same indices and letters in text and equations. Right now it is a little bit confusing.

Line 23-26: I wonder how you derive d_1 to d_4 . Do you calibrate them?

Page 950, line 16: K_s in Darcy's law is the saturated hydraulic conductivity – do you adapt it due to different saturations in your hillslope? Or do you always assume saturation prior to runoff generation? What about e.g. preferential flow in macro pores? Line 18: As K_s is calibrated I wonder whether you use distributed parameters, i.e. different values on different hillslopes or do you have only one parameter for the entire model (catchment). If yes, how do you deal with different physical soil properties? Some more explanation on this would be interesting.

Page 951, Line 11: How do you determine the residence times for each hillslope? What are the controlling factors? Heading section 3 – use a large P for river Passer

Page 952, line 14: north-east instead of nord-est

Line 15 – 25: It is not explained why you derive and present all this information, describing topographical features. Where is such information used in the model? Please explain this or consider to omit it as irrelevant information.

Page 953, line 1 – 3: Why do use such short periods of only a couple of months? Is that a data problem?

Line 6: Use sensitive or dominant instead of influential.

Line 9: Why didn't you calculate ET a constant ET over a period of several months is not very likely. Here the question arises: Why do you get such a good model performance.

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If I assume that ET is something around 50% (just a guess, please correct me) or more of the precip amount in summer this should be relevant.

Line 15 ff: Something went wrong in the description and the figure legend. Now I am confused what is observed, what is simulated runoff.

Line 19 ff: I cannot really see the underestimation you mention. In the latter part of this paragraph that the underestimation of discharge is caused by an underestimation of rainfall. How do you make the spatial estimates, don't you account for changes in elevation? And what about snow and ice processes could they be relevant for the underestimation as well?

Page 954, line 20 – 21: Indeed you show that the model is able to show the hydrographs. But from fig. 9 I have the impression that the model is not able to calculate them correctly. All of them look awkward with that long recession dominating the entire hydrographs. Looks like a linear storage which starts with a large content and which continuously releases water without being replenished. Some discussion on that would be interesting and absolutely needed.

Page 955, line 3: You should show us the parameters and you should try to explain which parameter is different and why. As you have stated earlier in the paper that the parameters have a physical meaning it is feasible to do so. Are you able to define one parameter set which works for both periods? Otherwise, the single periods might be overcalibrated.

Line 4: You mention "structural model defects". Firstly, I would call them structural model problems not defects. Secondly, please explain what problems you had in mind and maybe present solution how to solve them in the future.

Line 9: What do you mean by the dependence of hydraulic conductivity from temperature? Do you think of frozen soils or what is it. Is this really relevant as you neglect the dependence of hydraulic conductivity from soil water saturation (maybe I got that point

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wrong, but then you should make it clearer in the paper).

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

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