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Hydrology and Earth System Sciences Discussions

Interactive comment on "Conditioning rainfall-runoff model parameters for ungauged catchments and land management impacts analysis" *by* N. Bulygina et al.

Anonymous Referee #2

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General comments

This article presents very interesting methodological guidelines to constrain the model parameters space for ungauged catchments modelling from physical descriptors. The proposed theoretical framework is very attractive and opens several perspectives for both regionalisation and model calibration issues. The paper is well written and results are presented with good accuracy. Even if I really appreciated the methodology proposed in the paper, I think that the conclusions made by the authors are overly optimistic at this stage of their investigation. Therefore, I think it is premature to use the methodology for land-use change applications. Before that, the method needs to be

tested on other catchments and assessed by comparison with benchmark approaches. In any case, the restrictive hypotheses should be clearly listed and discussed. My main concern is that the authors have gone too far in their interpretation without clearly analyse the results step by step. I have two main comments that I wish the authors discuss:

1.The lack of benchmark approaches to assess objectively the method.

There is a need to assess rigorously what the proposed methodology brings in terms of constraint to the model and in terms of performance improvement. The authors present the method as a way to improve regionalisation method but they should make it clearer. Therefore, I suggest that the authors present model performance for several 'benchmark' situations. There should be at least three approaches to consider:

i.Model efficiency in calibration to give the margin of progress of the proposed method

ii.Model efficiency without constraining parameters from BFIHost to demonstrate in what extent the method improves the original situation

iii.Model efficiency when taking into account prior information on model parameters based on antecedent modelling experience, i.e. by taking into account the distributions of each parameter obtained from other gauged catchments.

2.The choice of the studied catchment/period

There is a problem of balance between the sophistication of the proposed methodology and its evaluation. Results are obtained for only one catchment (including subcatchments) for a 2-month period. It is so few that it is difficult to have a clear idea on the full potential of the method for other catchments/periods. The authors acknowledge this caveat but I think that the methodology is not validated enough to be used as a predicting tools for future land-use scenarios. Concerning the two-month period, why the authors restrict their analysis on those two months? They recognise that the method could suffer from the representation of evaporation processes outside the range of winter months, but it could be interesting to show it! Is the method degraded or simply uninformative for other periods of the year?

Specific comments

I would like the authors discuss/comment the following specific points:

1.Objective

- P.1910 lines 26-28, "The objective of this study is to develop a regionalization scheme which may be applied throughout the UK, and which may provide adequate information about rainfall-runoff responses for a range of applications." With respect to the results presented, I think the authors should be less ambitious when presenting the objective of the study. The paper proposes guidelines to possibly reach this objective but it is definitely not the case within the study presented.

2.Method

- P.1911 lines 20-26. The choice of the procedure used to calculate BFI may induce different estimations, which may affect the final results. I think the authors should discuss this issue.

- It is not very clear to me how did the authors discretise the catchment. I understood that each element is a 100mx100m cell, which is homogeneous in terms of HOST classification. Is it correct? Is the spatial discretisation determined by the spatial resolution of the HOST classification?

- Eq. 7 & 8: The proposed formulations of NS in a probabilistic simulation are interesting but what is the significance of NS values for only two months? Graphical evaluations are to me more relevant with those period lengths.

3.Case Study

- Are the stores of the RR model linear?

- P.1917 line 16: "Following Eq. (1), each parameter set is prescribed a weight based

on the closeness of the simulated BFI to the corresponding BFIHOST value, producing a posterior parameter distribution for each soil type." Could the authors make clearer how the weights are attributed?

- P.1917 lines 22-28. The way the authors use the celerity parameter is very vague. I guess there is a distinction whether the element includes a river network or not? This part of the model description should be more detailed.

4.Results

- P.1919 lines 13-16: "The performances achieved together with Fig. 6 support the view that BFIHOST is an effective response index, and therefore changing the distribution of BFIHOST values (Eqs. 2 and 3) is a viable method of introducing information about land management impacts." To me, this interpretation is very optimistic: 1) the authors do not show the performance of the model before introducing the BFIHost information and 2) a good level of performance in simulating streamflow does not mean that it is relevant for assessing land-use change impacts.

Technical corrections

p.1909, l. 27: Yadav -> Yadav et al. p.1910, l. 14: means than -> means that p.1911, l. 16: BFIHOST -> HOST or soil class p.1911, l. 19-20: remove one of the two "then" p.1916, l. 24: stoage -> storage. p.1920 l.8: "came almost entirely from the BFIHOST" Why almost?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 1907, 2009.