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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.

Dr Bulygina

n.bulygina@imperial.ac.uk

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General comments 1) 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.

We thank the reviewer for saying that 'the proposed theoretical framework is very attractive and opens several perspectives for both regionalisation and model calibration issues'. We also appreciate the comments and proposed technical corrections. The

paper has been significantly revised to include a new period of simulation, and a new extended discussion, plus various minor improvements as detailed below.

2) 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.

The following was added to the conclusions: '...it must be noted that the land use impacts analysis presented in this paper is largely theoretical (i.e. HOST class change and interception loss modelling) and built mainly on literature review. Validation studies are required. This may be done using paired catchment or manipulation plot experimental data (Brown et al., 2005; Marshall et al., 2009), and will be one of our future work directions. "

3) 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.

This would significantly lengthen and complicate the paper, which we prefer not to do as it has been praised by the reviewers as well-written, and is long enough and already contains significant new ideas.

There should be at least three approaches to consider: i. Model efficiency in calibration to give the margin of progress of the proposed method

Although the scope of this paper is not a specification of how well the BFI-based approach performs when compared to the traditional calibration(calibration is not going to be an option for an ungauged catchment), we compared our results to the Pontbren modelling study undertaken by Jackson et al. (2009) and Wheater et al. (2008). Their work is based on a field-wise representation of Pontbren, so that each field model is calibrated using small-and catchment –scale measurements, as well as physics based model simulations, so that it involved a large quantity of data and model development efforts. We compare the NSE performance of their model (gauges 2, 5, 6, and 7) in the table (see the supplement). It can be seen that overall the BFI-based approach is better in general but not always so, according to the NSE measure. We prefer not to complicate the paper by describing in full this comparison; however we summarise this analysis in the revised paper on page 5, immediately after introducing Table 5.

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

The BFI-based regionalisation efficiency is demonstrated in the paper on Figure 6. The streamflow prediction bounds using unrestricted (uniform distribution) parameters are shown in light grey, and the restricted predictions are shown in medium and darker grey. The proposed parameter restriction significantly narrows the prediction bounds while following the observation pattern.

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.

It could be an interesting exercise to compare the proposed method to existing regionalisation techniques which use parameters from other gauged catchments. Unfortunately, to the best of our knowledge, there are no parameter regionalisation data available for the scale we are interested in – field spatial scale, and 15-minute temporal resolution. However, the issue is now covered more extensively in the discussion

section.

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.

The method was tested on a 3 month period: January-March, 2007.

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 following was added to the conclusions section: 'It should be acknowledged though, that due to the model specifics: actual evapotranspiration evaluation is not suitable for dry periods, and fixed –ratio effective precipitation separation between slow and fast stores (may not work for dry months), the model was applied to a wet catchment with relatively impermeable subsoil during the winter period. And therefore it is possible that the chosen model is not suitable for other period, as well as the conditioning is not sufficient for other basins resulting in wide prediction bounds. ' Even though the model is not designed for dry periods simulations, we run it for July-August, 2007 (5 months spin-up period), and the results are shown on new Figure 7. And we added the following to the results section: "Predictions for a summer period (end of June 2007 to beginning of August 2007) are shown in Figure 7. While the good performance is maintained in general, the model significantly over-estimates stormflow following the rainfall events during the relatively dry periods in the middle of July. Difficulty in simulating wetting-up periods is typical of this PDM model and similar conceptual models. In particular, the simple evapotranspiration calculation and the inability of the model to maintain percolation while turning off stormflow generation (i.e. the assumption of constant ïĄą) are thought to be the main causes of this error. Both Figure 6 and Figure 7 show that the model is least successful at Gauge 9 in terms of explaining the base flow observations. This may be because the influence of the lake in sustaining low flows is more complex than represented in model; or may be due to low flow gauging errors."

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.

See general comment 2).

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?

See the comment above.

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.

The sentence was changed to: 'The objective of this study is to propose 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.'

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.

The BFI index was calculated in the same way as it was done for the BFI estimation in HOST, so that it is possible to compare the indices.

- 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?

As mentioned in the introduction, we want 'to explore impacts of local land management changes to catchment properties' – the local changes are assumed to take place 'field' – wise, motivating field scale (100 m x 100 m) resolution. Although each element is homogeneous in terms of HOST classification (i.e. the predominant HOST class in each element is assumed to apply uniformly over the element), the method in general is not limited to this size or kind of elements. The following was added to the method description section: 'For simplicity, we assume each element to be homogeneous in terms of HOST classification (although if the heterogeneity was significant, the method could easily be extended to consider the distribution of BFI values within each element)".

- 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.

The three months 15-minute observation data consist of approximately 4x24x30x3 = 8,640 data points, and we feel that it is useful to have not just graphical, but also quantitative characteristics of the model performance.

3) Case Study - Are the stores of the RR model linear?

In the model description there is a following 'A probability distributed soil moisture model together with two parallel linear routing stores...'

- 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?

Following equation (1), any parameter set θ sampled from the prior parameter distribution p0 should be weighted by model estimated BFIθ closeness to the expected BFI from the HOST using described in the text normal distribution.

- 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.

The description was extended 'Each element is connected to the stream network. In each element the channel is assumed to be a single straight length of channel connecting the upstream and downstream nodes of that element. The runoff from each element is assumed to be uniformly distributed along the associated stream length. Element runoff is routed down using a constant celerity approach, i.e. the water moves with constant velocity...'

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

See general comments 3.1.ii.

and 2) a good level of performance in simulating streamflow does not mean that it is relevant for assessing land-use change impacts.

Thank you for this point – the sentence was changed to 'The performances achieved together with Figure 6 support the view that BFIHOST is an effective response index.'

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.

Thank you for the technical corrections – corresponding changes were made in the text.

p.1920 l.8: "came almost entirely from the BFIHOST". Why almost?

As described in chapter 3.3, peak flow timing was used to estimate celerity.

Please also note the Supplement to this comment.

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