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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.

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General comments 1) This paper illustrates an interesting approach of calibrating a conceptual rainfall-runoff model to regionalised hydrological statistics, in this case the baseflow index estimate available in the HOST database. The paper is well written and presents results clearly. I wish to make a couple of general comments before a series of specific comments and clarifications.

We would like to thank the reviewer for calling our paper 'an interesting approach of calibrating a conceptual rainfall-runoff model'. We acknowledge a thorough review of the work, as well as useful regionalisation strategy suggestions. 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) First, I feel the results are limited to a significant degree by the choice of study catchment. The model application is to a wet catchment with "relatively impermeable subsoil" during the winter period and thus there should be limited challenge in predicting the overall runoff volume. Hence the challenge reduces towards simulating the timing, which the information used should inform (at least in terms of volume of slow versus fast response and timing of high flow). While not invalidating the results, this does limit their generality, which to be fair, the authors acknowledge implicitly, though this should be spelt out in more detail. The key concern is how well will the approach perform in the many environments where estimating the water balance is more challenging?

The study introduces an index that may be helpful in parameter restriction, a new method of using it for model conditioning, and demonstration of applicability to the Pontbren catchment. The paper notes that it is likely that for some basins this particular index will not be sufficient resulting in wide prediction bounds. Therefore, while in principle we could explore the applicability of the index and the conditioning method to different types of catchment, that is not within the objectives of the paper. However, we have extended the analysis to include a dry period, which partly addresses the reviewer's concern (see new Figure 7 in the paper). 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 alpha) 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."

Additionally, the following was added to the conclusions section: "...the conditioned model was shown to simulate observed flows to an impressive level of accuracy in a wet winter period. Due to the simplicity of the evapotranspiration model and also due to the fixed split of effective precipitation between slow and fast stores, the method did not perform so well in a drier period, and is not expected to perform so well on relatively permeable soils."

3) In relation to how easily the catchment can be modelled, it would at least be good to see how well a traditionally calibrated model would perform on this catchment.

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 have compared our results to Pontbren modelling study undertaken by Jackson et al. (2008) 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 their model (gauges 2, 5, 6, and 7) performance in the Table A (see supplement). It can be seen that gauge 2 and 7 streamflows are modelled better using the BFI – based regionalisation approach, while gauges 5 and 6 streamflow are modelled better by the other modelling approach. 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.

4) Second, there is no testing of the approach advanced for predicting change due to afforestation or grazing intensification. This needs to be spelt out very clearly. There is a clear opportunity to test this approach using paired catchment experimental data

(see review by (Brown et al., 2005)).

The following was added to the conclusions: 'Finally, 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.'

Specific comments 1) 1911 line 19-20. For a general model, simulated BFI will depend on the model forcing. In general that forcing will be spatially variable over any particular soil type. However the method as presented develops one distribution of parameter sets based on the response from one model unit. Is the method generalisable to say larger scales where spatially variable forcing becomes important?

The variability of response within one soil type is implicit to the method, by consideration of the variance in BFIHOST. A note on this has been added when introducing BFIHOST on page 2. "The variance of BFIHOST is specified for each class, representing spatial and temporal variabilities within classes"

2) 1913 line 1-3. (Brown et al., 2005) discuss the evidence for changes in baseflow following forest cover change arising from paired catchment experiments around the globe.

The following was added to the text (in italic): 'There is also evidence that base flow proportion increases under forest both in Pontbren (Wheater et al., 2008) and in reviews of paired catchment studies around the world (Brown et al., 2005)...'

3) 1916-1917. The model description is pretty short. There is no mention of how actual evapotranspiration is calculated for example or how this might link to soil moisture, or how any ET parameters were set. Some additional detail would be useful.

The following was added to the model description section: 'An element's generated

runoff directly goes to a stream network that connects all elements, so that there is no model element interaction... Actual evapotranspiration is equal to potential evapotranspiration multiplied by the relative moisture content of the soil.' A more detailed description is available in the cited literature, i.e. Calver et al., 2005; Lamb and Kay, 2004; Lee et al., 2006; Orellana et al., 2008; Wagener et al., 2004.

4) 1918, line 14. Top-down models such as (Zhang et al., 2001) could also a useful source of conditioning information.

This could, potentially, be used for ET-related parameter restrictions, but in this study we used interception capacity and air resistance from the literature. The following was added to the text: 'Alternatively, canopy interception capacity and wet canopy enhanced evapotranspiration rate could be conditioned using the top-down model for forest actual evapotranspiration rate proposed by Zhang et al. (2001)'.

5) 1918, lines 15-19. It should be explicitly stated that the method was uninformative for the remaining parameters (as would be expected from the model structure and the information used). It would also be useful to show the prior distributions on Figure 5.

The following was added to the text: 'After one-, two-, and tree- at-a-time restricted parameter plot inspection, there seem to be no other BFI-induced restriction on the parameters'. The prior distributions were added to Figure 5.

Technical corrections 1) 1910 line 8. I assume there is not interaction between model elements?

We assume that this comment refers to page 1917. See comment 3).

2) 1914 Equation 4, define q.

The following was added:'... q is a variable for integration'

3) 1916 line 5. Can you clarify whether there was only one rain gauge used, even though there are multiple stream gauges installed throughout the catchment.

The following was added to the text: '...from one of the on-site raingauges'.

4) 1919 lines 7-10 discus flow rates for which the model was calibrated. This is slightly confusing to the reader as the model wasn't calibrated to flows in the usual sense. Some re-wording would improve this.

The lines refer to 'the range of flows within which the streamflow gauge was calibrated and considered accurate', not 'flow rates for which the model was calibrated'. We think it accurately describes what we meant to say.

References Brown, A.E., Zhang, L., McMahon, T.A., Western, A.W. and Vertessy, R.A., 2005. A review of paired catchment studies with reference to seasonal flows and climatic variability. Journal of Hydrology, 310(1-4): 28-61. Zhang, L., Dawes, W.R. and Walker, G.R., 2001. Response of mean annual evapotranspiration to vegetation changes at catchment scale. Water Resources Research, 37(3): 701-708. Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 1907, 2009. C215

Please also note the Supplement to this comment.

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