Review for 'Tailor-made spatial patterns for hydrologic model parameters combining regionalisation methods' by Rouhier et al.

Main Comments

This paper proposes a regionalization approach to identify parameters of a spatially distributed conceptual hydrologic model. The proposed method is compared against standard calibration using four performance measures related to streamflow. Results claim that the proposed regionalization method performs better than the calibration approach by (i) reducing the number of parameters required for calibration, and (ii) by improving performance in most measures. Although the idea of the paper is interesting, in its present form, the paper suffers from several issues which should be addressed before an assessment can be made regarding the reliability of the results. These issues are highlighted below:

1. Writing style: the paper follows a rather confusing writing style. A few issues:
   a. The writing introduces terms such as 'spatialization', 'transposition', 'prescription', etc. for which standard terminology is already available in hydrologic literature. For example, 'prescription' is described as *apriori* parameter estimation (Lines 10, Page 2). It is perhaps better to use the existing term '*apriori* parameter estimation' instead of introducing a new term for the same concept! Similarly, for other terms which may be related to existing terminology, it is better to use that so that readers can easily follow the manuscript. See Blöschl et al. (2013) for terminology related to predictions in ungauged basins and Beven (2001) for standard terminology used in hydrologic modeling.
   b. Grammar and sentence construction can benefit from moderate editing. The paper is a little hard to read right now.
   c. Mixing of results and methods: many results are presented in the methods section. For example Figures 4, 5, 7, and 8 are actually result figures but are presented and discussed in the methods section. A clear separation of methods and results would help organize the manuscript. Another issue is that various sub-plots within Figures 4 and 5 are discussed quite far apart in the manuscript. Generally, figures are presented in order of their appearance in the text. So Fig 4a-d should be discussed prior to Figure 5!
2. Methods: There are differences between how certain methods (such as *apriori* parameter specification) are used in standard literature and how they are interpreted in the study. For example:
   a. Section 4.2.1 implies that 'prescription' is just fixing insensitive parameters to some default value. The introduction, however, states that 'prescription' is *apriori* parameter estimation. The way 'prescription' is implemented in the paper is not *apriori* parameter specification and therefore, either the introduction should be corrected or the method's implementation changed. Section 4.2.2 implements *apriori* parameter estimation consistently with existing literature.
   b. Similarly, the 'constraint' method (Section 4.2.3) is oddly presented. Generally, 'constraining' model parameters implies estimating model parameters as an ungauged site, by using hydro-climatic information (including streamflow) from gauged sites. Constraining typically does not provide a single value for model parameters, rather a range of possible values that reproduce the expected (streamflow) response of the catchment (see Yadav et al. 2007 and Zhang et al. 2008 for more details). However, in the present study, 'constraint' method is applied in an odd manner. It does begin with developing regression relationships between the streamflow predictor (mean annual streamflow or long term mean monthly streamflow) and catchment

charactristics. However, 'in practise' the study just uses observed streamflow data to calibrate relative errors (Lines 19-20, Page 11). Thus, in reality, the study is implementing calibration while labelling it as constraining! To implement constraining, the signature (such as mean annual streamflow) predicted from the regression relationship should be used to reduce the range of (some or all) parameters. Constraining by itself may not identify single parameter sets.

c. Section 4.2.4: It isn't clear why some small validation basins were chosen for application of this fourth approach. Either all validation basins can be parameterized following physio-climatic similarity, or none.

   In addition, certain quantifications, such as the Enhancement Index (EI, equation 1) perhaps have established statistical counterparts. For example, the distance between two CDFs is quantified by the Kolmogorov-Smirnov statistic (KS Statistic). This can be used to quantify whether one CDF is significantly different than the other. The statistic not only provides a measure of distance but also whether the distance is significant given the number of data points used to construct the individual CDFs. Given the longer tails of the Exp1 CDFs (blue lines in Figures 9 and 10) it is expected that the area under the curve would be higher. This can be stated as such and introducing a new term to quantify is not really warranted. At the very least, KS-Statistic can be used to complement the EI.

3. Introduction: The introduction states that parameter estimation schemes for distributed models are not as well organized as those for conceptual models. However, there are several papers that have attempted to do this and can be discussed briefly in the introduction. See for example Gotzinger and Bardossy (2007), Samaniego et al. (2010), Wi et al. (2015), and others cited therein.

4. Results: this section is too short, perhaps also because of presentation of results in the methods section. Some more details can be added. For example, why was only Exp1 chosen to compare Exp2? Rouhier et al. (2017) present various methods of calibrating the distributed hydrologic model using various levels of discretization of hydrologic units and climate data. Some of those experiments performed better than Exp1 of the present study (Fig. 8d for KGE low flow in Rouhier et al. 2017 shows some better performances than shown in Figure 9d of present manuscript). Is it possible to use see how the various experiments in Rouhier et al. (2017) compare with the proposed method in this study?

5. Discussion: the paper does not have any discussion section where the results from this study are compared with those from existing works on calibration of distributed hydrologic models. At the very least, some comparison with Rouhier et al. (2017) can be added.

Minor Comments:

1. Abstract: introduces terms 'transposition', 'prescription', etc. without explaining them first. Consider using standard hydrologic modelling terminology for these.
2. Line 22, Page 1: 'comprises' can be 'is represented by'.
3. Lines 2-3, Page 2: provide citation as all three methods (transposition, prescription, and constraint) exist in literature.
4. Lines 28-31, Page 2: discussion of the constraint method is confusing. Multi-objective calibration is not the same as constraining of hydrologic model parameters using runoff signatures (which is mainly used for ungauged basins). Please review this part of the writeup carefully using the information in Yadav et al. (2007) and Zhang et al. (2008).
5. Line 2, Page 3: Which parameter can be calibrated using land surface temperature?

6. Line 10, Page 3: '50/50 spatial split-sample test' not intuitively clear what this means so a short clarification can help. Or simply the term 'spatial validation' can be used and the more detailed description can follow in the methods section.
7. Line 31, Page 3: 'nival Durance catchment.' Should be 'nival Durance catchment is used.' Please check for grammar and sentence construction elsewhere.
8. Line 32, Page 3: 'A' should be 'Appendix A'.
9. Figures 1 and 2 are the same as those in Rouhier et al. (2017). Please check copyright issues. Please also explain all the terms used in Figure 1, such as P, T, PET, AETbv, etc. in the figure caption.
10. Line 7, Page 4: not clear how long-term mean daily streamflow captures seasonality. It is better to provide equations for each objective function, stating exactly how they are calculated.
11. Please rename section headers for Section 4.1 and 4.2.
12. Section 4.2.1: are sensitivity indices calculated for each calibration station or for the catchment as a whole? Also, is the sensitivity index calculated for all objective functions or just one? If for the entire catchment at once with one objective function, then it is being assumed that sensitivity does not vary across sub-catchments or across objective functions, which may not be true. It is possible that some parameters do not show sensitivity at one site for one objective function but does show sensitivity at another site for another objective function. Please clarify and also provide a table listing sensitivity indices (first-order, interaction effects, and total order) for each parameter so that readers can also assess which parameters are insensitive.

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