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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Parameter regionalization of a monthly water balance model for the conterminous United States" *by* A. R. Bock et al.

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The paper presents a novel approach for regionalisation of parameters of a hydrologic model (the Monthly Water Balance Model) by application to the conterminous United States. The key idea is to define hydrologic similarity between gauged and ungauged basins using parameter sensitivities. The paper is interesting and should be considered for publication. My expertise is on model calibration and sensitivity analysis more than parameter regionalisation, so the focus of my review is on the former aspects. With this respect, I have few main issues that I think should be addressed in a revised version of the manuscript, and a list of minor remarks.

MAIN POINTS

[1] The parameter regionalisation procedure could be explained more effectively.
In the first place, it would be good to have a schematic of the procedure to clearly see what is the role, inputs and outputs of each step (sensitivity analysis, classification of regions, individual calibration, grouped calibration, etc.). The structure of Sec 2-3-4 could be revised to better separate out the methodology from the illustration of results. For instance, I find a bit odd that sensitivity analysis results are presented in Sec. 3.1, before describing how they will be used in the proposed methodology. Another example is the first paragraph of Sec. 4.1, which explains why the individual streamgage calibration is needed, it would fit better in a "methods" section rather than the "results" section.
The way sensitivity estimates are used for regionalisation (described on page 10034, line 7 onwards) needs to be explained more clearly, especially since this is the most novel aspect of the proposed methodology. Specifically:
- What is the connection between the first and second classification? They are independent from each other and then intersected to obtain the actual classification? Please clarify
- Be more specific on how the two classification approaches work. Sentence on lines 7-8 of page 10034 is too generic, does it mean that the parameter ranking is the same in each region?
Description of the second classification (lines 10-11) is also unclear. What are the "unique combinations of parameter sensitivities"? How are they defined? What is their meaning?
- From lines 17-20, I understand that the sensitivity-based classification is further refined using a more 'conventional' approach that looks at proximity and topographic divides. How does this refinement step works? Does it introduce significant changes

in the classification? This is important to know in order to understand the value of the proposed sensitivity-based classification versus proximity or topography-based classification.

[2] Some of the numerical results are a bit surprising and should be double-checked. In particular, in Fig. 5.a the fact that one parameter has sensitivity of exactly 100 and all others of exactly 0 seems odd. Also, the result of Figure 13 is very puzzling. As the authors note on page 10041 (line 27 onwards), the groupNSE values are expected to be lower than the gageNSE values. Figure 13 instead shows many cases were groupNSE is much larger than gageNSE. I really struggle to believe that NSE can be increased so much and so often when using a model calibrated with a different objective function. The only explanation I can think of is that either the calibration algorithm in the gageNSE failed (for instance by getting stuck in a local minimum or being terminated too early) or that the comparison is not fair (for example that NSE refer to different time periods?). This needs clarification.

MINOR REMARKS

p. 10024, lines 1-2 “to transfer ... model uncertainty information”. What type of uncertainty information is transferred and how? This is mentioned here and in the conclusion but it is not clearly discussed throughout the paper.

p. 10026: lines 28: “these methods ignore parameter interaction, and often assume that model algorithms have linear responses to different parameters”. I think this sentence is misleading and I would suggest to delete it. Parameter interactions can be evaluated in local SA by computing second-order derivatives (see for example Norton, 2015). Also, when estimating local sensitivities the linearity assumption finds its rationale in the Taylor series expansion and hence it is quite reasonable.

p. 10028, line 25 to the end of page: this list of parameter names and meaning does not add much to the information provided in the Table, I would probably avoid it.

p. 10030, line 8: the term FOPV is not particularly self-explaining to readers not familiar with GSA. I would explain what it is (“contribution to output variance from ...”)

p. 10030, line 12: “much less information and parameter sets”. What do you mean by “information”? Unclear. As for "parameter sets, it is possibly less ambiguous if you call them parameter samples or even directly model evaluations

p. 10030, lines 14-15: please justify why you do not incorporate the adjustment factors in the FAST analysis

p. 10031, lines 6-7 “parameter ranges were based...” Are these the ranges in Table 1 and already commented on p.10028, line 24? If so, just refer to the Table here.

p. 10032, lines 7-9: What do you mean by “standard application”? Also, I suppose the R package uses the equation $N = 2N_{harm} \max(\omega) + 1$ to determine the minimal number of runs. If so, better cite Cuckier et al (1973), which is where the formula comes from. Also, please mention what is this number in your case, it would help readers to get an idea of how computationally demanding is the proposed approach.

p. 10032, lines 12-13: “The patterns of ...”. Sentence needs rewording

p. 10033, line 25: “While this idea...”. What idea? The one described in the previous

sentence? But then is it really in contrast with the one illustrated on lines 27-28? Please clarify.

p. 10034, lines 3-4. Please justify why using RR.

p. 10034, lines 11-13: citation of Pianosi et al., 2015 does not seem to be appropriate here. That paper introduces a toolbox for Sensitivity Analysis but it does not discuss the issue of setting the threshold for sensitive and non-sensitive parameters. The threshold issue is (partially) discussed in Tang et al. (2007). Pianosi et al. have another paper under review which is more focused on the threshold issue, however it has not been published yet. The authors might cite that paper when it will be published (title is "Global Sensitivity Analysis of environmental models: Convergence and validation", journal is Environmental Modelling and Software).

p. 10037, line 17: Please give a very brief definition of a reference streamgage.

p. 10038, lines 1-4: The definition of the multi-term objective function is unclear. Are the four terms summed up? Why considering both mean monthly runoff and annual runoff (I would imagine that they convey the same information, the former being equal to the latter divided by 12)? I think inserting an equation with the mathematical definition of the objective function would help here.

p. 10038, line 9: "simulated streamflow" should be "simulated variable" (since one of the four is SWE and not runoff)

p. 10038, lines 14-16: Please clarify how the error bounds were taken into account.

Did you modify the definition of the Z score for the SWE?

p. 10038, line 20: Recall here that a parameter is deemed insensitive if sensitivity index is below 5%

p. 10038, lines 21-23: "on a mean monthly based". Unclear. Possibly it might just be dropped, since it was already said that monthly variables are used to compute the Z scores.

caption of Fig. 1: "model parameters used in..." Maybe better: "processes influenced by the model parameters used in..."

Figure 3: maybe not needed. Anyway, if maintained, vertical axis should show units of measurements. Also, it would probably be better to show Drofac and Rfactor in a separate panel.

caption of Fig. 5: Explain what circles, dashed lines, etc. refer to.

REFERENCES

Norton, J., 2015. An introduction to sensitivity assessment of simulation models. Environmental Modelling & Software 69, 166–174.

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