

***Interactive comment on “An integrated multi-fingerprint sensitivity-nested approach for regional model parameter estimation and catchment similarity assessment” by Simon Höllering et al.***

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After reading the manuscript, I must admit that “I am lost and confused”! It seems the manuscript is revolving around many but at the same time no clear message. Either I missed or I am not capable to fully understand; I don’t see any conclusion from the presented manuscript!

Starting from the structure; the manuscript is very badly structured and too wordy and long. The literature review is spread over the paper and there are a lot of unnecessary sub-sections and subsub-sections which can be reduced.

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The title looks very sophisticated and broad. Every word need further explanation which the manuscript fails to fully explain. As an example, “integrated” and “sensitivity-nested” seem to have similar meaning. “Multi-fingerprint” can be replaced by much simpler words. Did the authors really carry out a “catchment similarity assessment” as stated in the title?

The abstract is again distance from what the paper is trying to tell. One cannot really understand the final conclusion of the paper from the abstract and only an unclear and again broad sentence such as “The sensitivity approach may be useful. . .” concludes the abstract. I would also remove the word “novel” from the abstract and any other places in the manuscript. It is the readers’ decision to decide about the novelty not the authors’.

The introduction is very wide too with many relevant and irrelevant studies put together next to each other in non-coherent sentences. The general literature review is not enough and missing a significant body of the existing literature on catchment similarities, catchment classification and so on. As an example, one of my own research interests which have been mentioned in section 1.2 can be re-written for topographical landscape unit as follow:

“Using indices based on topography can be one of many ways to delineate a catchment into hydrological response units or as stated by Zehe et al., 2014, functional units. These units can be built based on topographical features as stated by Knudsen et al., 1987, Flugel 1995 and winter 2001 into X1, X2 and X3. Aligned with what have been suggested and with help of a recent topographical index (HAND, Renno et al., 2008) Gharari et al (2011) classified a small (or meso scale) catchment in Luxembourg and then used the mapping for building and constraining a conceptual model (Gharari et al 2014a). This classification have been repeated by Gao et al 2014 for a large scale catchment in China and served as the basis of the modeling exercise.”

The authors can extend this section by elaborating on slope, soil and land cover.

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Please keep in mind that the introduction, as well as each paragraph, should have a funnel shape structure starting from broad and general overview and ending in example of specific implications. This way the reader will be ready for the final and general message of the paper.

“Is the inconsistency of functional unit and physiographic similarities a paradox?” No one really claim that response units should exactly follow physiography. This is just an assumption to give us the ability to make of prediction of what we really don't know (such as prediction in ungauged basin).

I encourage the authors to give a comprehensive literature review in section 1.4 regarding the signatures (or fingerprints). I would also ask the authors to give a comprehensive literature review on the use of signature in the hydrological modeling (such as Euser et al., 2013 and Clark et al., 2011). What remains missing in the introduction given the gist of title and current introduction is the relation between the model, signature and processes. There have been numerous studies looking on this topic, the sensitivity and also uncertainty of the model parameters over different period of time. I remember reviewing a manuscript on this topic for the very same journal HESS, Pfannerstill et al., 2015. Dr. Guse gave a very wide range of publication related to this in his review.

Section 3.4.2 is a mixture of literature review of the signatures, and FAST and selected signature for this study. I would advise the authors to briefly clarify what signatures they used in this study. The literature review should have been presented earlier and FAST is explained later in the manuscript which makes it a bit difficult to follow. I would also suggest to clarify the possible limitation of FAST compare to the other sensitivity analysis, Razavi and Gupta (2015) may help in that regard.

Please clarify the name and possibly the similarities of the headwater catchments in section 4.1 and 4.2. The names of the headwaters pop up every now and then in the manuscript!

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About the parameter constraining: In my point of view what the authors are presenting in this study is not parameter constraining, it is rather parameter selection given various signature and based on sensitivity analysis. We had recent papers looking at constraining (Gharari et al., 2014a and 2014b, Hrachowitz et al., 2014). There are many related existing studies as well, the authors are more than welcome to address in them in their manuscript. Constraining should be different in my point of view from parameter selection based on a multi objective approach (which is similar to breaking the evaluation of the time series into different signatures).

The figures and result part is very hard to follow. I believe one of the main reason is the simultaneous presentation of the results together with the methods. I would have separated the method and result sections. The figures are almost very difficult to follow, as an example figure 5, 6, 7, 8, and 11. Again back to what I mentioned earlier the authors are not constraining the parameter sets but they are selecting different parameter sets based on different criteria (Figure 7). The criteria the authors are using to me seems like a multi-objective approach where the most balanced parameter sets are selected by Euclidian distance. We did this in our study in 2013 as well (Gharari et al, 2013) using Pareto front members and Euclidian distance to pick the behavioral parameter sets. The point of that study was not to constrain the model parameters but to show that the parameter sets which are better performing over time are different from the optimal or behavioral parameter sets.

Conclusion and discussion is vague as well. I would suggest the authors to separate the conclusion and discussion parts. If the paper have a message and strong conclusion the conclusion part should be only few bullet points, pointing at the most general and specific findings of the manuscript. The authors can make use of this bullet point conclusions to form the abstract, introduction and methodology better.

The manuscript is far from the minimum quality for publication, meaning that it should be rejected. To give a chance for the manuscript contribution to be published in HESS I would give major revision. I am looking forward to receiving the revised manuscript. I

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hope my comments help the authors to elevate the level of the current manuscript and present in much better shape.

With kind regards

Shervan Gharari

PS. I thank Fuad Yassin who helped me better understand the manuscript. Our discussion was helpful for writing this review. I guess he was also “lost and confused”!  
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