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Hydrology and Earth System Sciences Discussions

Interactive comment on "Comparative assessment of predictions in ungauged basins – Part 3: Runoff signatures in Austria" *by* A. Viglione et al.

Anonymous Referee #3

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This paper presents an interesting example of good practice for the evaluation of a regionalisation study but I am not convinced by the added value of the comparison of the two methods. As far as I understand, the two methods are not trained with the same amount of data, meaning that one makes predictions based on more information than the other. How is a fair comparison possible in this setting?

I suggest reworking the text to give it a clearer focus on how runoff signatures can be used to assess the performance of prediction methods across a range of catchments (the current focus seems to be how good "state-of-the-art" regionalisation methods are). It could certainly become much clearer if the assessment method was illustrated in detail for one of the models and only summarized for the 2nd model. And It should be better explained, for each of the models, how the performance for the prediction

of the runoff signatures for the training data sets depends on the chosen calibration strategy.

Furthermore, there should be a more detailed review of existing studies that use runoff signatures to calibrate models or for model performance assessment (for example the work of Thorsten Wagener)

Detailed comments:

- the abstract has no outlook

- section 2: the description of the different hydrological regimes is important for the paper but currently buried in the somewhat vague text about climate-vegetation-landscape co-evolution. I suggest having a section that describes the regimes and the signatures separate from the regionalization methods (the two sets of methods are actually independent);

- section: since comparative hydrology is mentioned, I would expect some additional references

- Method: I did not understand how the different set of gauges were selected and why for the statistical method, there were much more gauges; this should be clearer in the text. Does TopKriging a better job simply because it had far more training data?

- should the low flow signature not distinguish between the season of occurrence? or are there no catchments that have winter and summer low flow?

- p. 458: I would say that a median Nash value decrease of 0.11 is a quite important decrease but this depends on the underlying hydrological regime. The numbers are definitively very hard to interpret, especially if averaged over different regimes - details about the model calibration should be given

- p. 464, line 23: very vague statement that "with increasing catchment area new processes take over", an example would help

- p. 465: it should be mentioned much earlier how many catchments are nested

- p. 465: repetition of the discussion of aggregation effect , this could be more concise

- p. 465, line 19: a bit ill-formulated, not the interplay of processes improves hydrological simulation but the reduced variability is easier to model

-p. 466, first line: why is the prediction performance improved if there is snow?

p. 466, line 14: the exception of mean annual runoff is mentioned just before

p. 468, line 21: should it read "these flows" instead of floods?

p. 470:line 3 - 5: this is mentioned for the first time in the conclusion; there is no outlook

- Table 2: since low flow is evaluated, Nash-log values would be a useful information

- Fig. 6: I suggest adding "see text for further details".

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 449, 2013.