Hydrol. Earth Syst. Sci. Discuss., 10, C699–C702, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C699/2013/

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.

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Received and published: 7 April 2013

We would like to thank the reviewer for her/his constructive comments on the manuscript. In the following Referee #5's comments are in *italic* and our responses in plain text.

First of all my deepest apologies to the authors for this extremely late review. The manuscript presents a comparison of two regionalisation methods relying on the large and high quality hydrological dataset available in Austria : top-kriging and a regionalised rainfall-runoff model. The manuscript is overall clear, well illustrated and contains undoubtedly useful results that can feed scientific debates in hydrology as already illustrated by the exchanges around this "discussion version". I am personally moderately convinced by the development proposed in the manuscript and the detailed analysis

of the extrapolation results obtained for various so-called "runoff signatures". In fact, the obtained results are perfectly explained by the few figures provided at the very beginning of the manuscript(end of page 458): Top-kriging performs better in reproducing hydrographs at ungauged sites than the selected RR model even when applied to gauged sites. The die is cast! I would have wished that the authors take much more time to comment and explain this surprising result rather than focussing this manuscript on the analysis of the relatively straightforward consequences on the forecasting performances of so-called signatures at ungauged sites. At least, this first result on hydrograph predictions should be recalled in the analyses to support the interpretations. But this is a matter of sensibility.

The figures at the beginning of the paper are illustrative and do not present the assessment of method performances (Figs. 1 and 3 present data and Fig. 2 presents measures of the 6 signatures mapped on the river network). The misunderstanding probably depends on the introduction, which we will rewrite in the revised paper, and on the way Section 2 is organised. Section 2 will be revised as suggested by Referee #3, i.e., the section will describe the regimes and the signatures separate from the regionalization methods in order to give the context of the study and not mixing the two things (description vs. regionalisation). The fact that Topkriging performs better in Austria is not surprising to us (see Merz and Blöschl, 2005, Skøien et al., 2006). The paper's objective is the assessment of the methods in terms of being able to capture different signatures of runoff variability (when the models have not been tuned to do so). With the revised introduction and the added discussion section we will try to make these points more clear (see response to reviewer #1).

Merz, R., and G. Blöschl. "Flood frequency regionalisation-spatial proximity vs. catchment attributes." Journal of Hydrology 302.1 (2005): 283-306.

Skøien, J. O., Merz, R., and Blöschl, G.: Top-kriging – geostatistics on stream networks, Hydrol. Earth Syst. Sci., 10, 277–287, doi:10.5194/hess-10-277-2006, 2006.

Nevertheless, besides this major comment that does not necessarily necessitate a in depth revision of the manuscript, it may be improved through some minor modifications to my opinion (detailed comments are provided in an annotated manuscript attached to this review).

1) The methods and especially the Top-Kriging approach should be presented in more detail (maybe in an appendix). Some important aspects may be unclear for readers who are not familiar with the papers of Skoien, Merz and Bloeschl. For instance, how is an entire hydrograph interpolated at ungauged sites (p 458). Are the various signatures interpolated separately ?

Referee #5 is perfectly right. We will extend section 3 and give many more details on the methodologies and their parametrisation (see also the replies to the other referees).

2) Results obtained for the at site calibrate RR model should also be provided to support the interpretations and to identify what can be attributed to the limits of the RR model and its input data and what is linked to regionalisation of the model.

This is a good point. We can discuss in the discussion section the difference between at site and regional estimates in more detail.

3) The introduction could be shortened as well as the description part of the results to reduce the length of the manuscript and make it easier to read. This result part is much too descriptive and lacking real interpretations. The authors should make the effort to test their interpretation hypotheses or at least some of them against the data (see annotated manuscript).

We will add a discussion section in the revised paper. We will make clear where interpretations of the results are hypothesis or are supported by the analysis.

4) I would suggest to avoid the sometimes too conceptual presentation based on questionable notions (level of linearity, supposed relation between variability and predictability...) and to keep the text accurate and simple (see annotated manuscript).

Yes.

Please also note the supplement to this comment: http://www.hydrol-earth-syst-sci-discuss.net/10/C298/2013/hessd-10-C298-2013-supplement.pdf

We thank Referee #5 for the detailed comments to the manuscript in the supplemented pdf. They will be considered in revising the manuscript.

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