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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.

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We would like to thank Tanja Euser for her constructive comments on the manuscript. In the following her comments are in *italic* and our responses in plain text.

In this paper the authors address the quality of predictions in ungauged basins (PUB). After a decade of new ideas and research initiatives their aim is to compare some of the used techniques to assess the quality is of these new predictions and methods. In this study they compare two regionalisation methods, a process based method (with the HBV rainfall-runoff model) and a statistical method (Top-Kriging). For this comparison they used many Austrian catchments. The comparison of these two methods is based on the prediction of signatures: which method is better in predicting specific signatures. I think that the concept of the paper is very relevant: it is good to look back and assess

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Discussion Paper

the quality and usefulness of previous work. In addition, I fully agree with the authors that a comparison based on signatures is much more informative than a comparison only based on the highest Nash-Sutcliffe Efficiencies.

However, I do not think the the paper is well structured and referenced. I am wondering why the authors did not use the more 'standard' outline for a paper, starting with a proper introduction, which summarises the work of PUB and introduces the techniques compared in this study. Followed by an description of the study area, in which the main differences between the Austrian catchment can be described. In the following methodology section the two regionalisation methods and the signatures can be described more extensively. I think that this will prevent a lot of forward referring and would make the structure of the paper more clear. In addition, I would advice to split the result and discussion section into two sections. The results require a lot of discussion, which is provided by the authors, and this discussion would be easier to follow when it is separated from the results. Finally, I would advice the authors to have another look at their conclusion and move discussion points from the conclusions to the discussion section.

We agree with Tanja Euser. We therefore will rewrite the introduction in order to clarify the paper structure and aims and we will add a discussion section. Also, 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.

Regarding the references used in this paper. In the introduction the authors state that their aim is to assess the performance of methods to predict runoff developed during the PUB decade. If this is the aim, the amount of references regarding studies in the PUB decade is really limited. I think that the introduction should at least contain several examples of the two regionalisation methods. In addition, a lot of people worked with signatures and regionalisation before, adding some of these reference would make the introduction stronger.

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Interactive Comment

In response to this comment, we will change the introduction and give to the PUB issue, which has to do with the three companion papers as a whole, the right relevance. For more details, please see the response to Referee #2. Also, in the revised paper we will add references to papers that use signatures to regionalise, underlying that we do not calibrate on them, but assess the performances of the methods in their ability to get the signatures right.

Finally, there are a lot of small things in the paper which are not completely clear or consequent. Among them the the calibration and exact regionalisation procedure. For example which catchments are used as donor catchments for which catchments and are multiple catchments used as donor catchment? How is determined which catchments could function well as donor catchments for a specific catchment? The authors also note that the used objective function during calibration can influence the results. Can they also comment in which way and to which extent this can influence the final results?

We will add considerations about generalisation of the results in the discussion. We will perform further analyses which may allow us to clarify the questions posed by Tanja Euser. We will extend section 3 in the revised paper in order to provide much more information on the methods and their parametrisation. However we will make clear that the focus of this paper is the assessment rather than the choice/parametrisation of the methods.

To conclude, I think the concept of the paper is good and important; however, I think it requires quite some rewriting before it is publishable.

Minor suggestions/questions: - p450, l5: What is a consistent data set?

We mean that all signatures are calculated on the same dataset. In the literature review of the two companion papers this was not the case.

- p450, l26: Add some references with examples.

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We will add some references to PUB in the revised introduction.

- p451, l5: Add a reference with an example of cross-validation.

Cross-validation is a classic statistical technique extensively used in hydrology. We will add a citation to Efron, B., Gong, G. (1983). A leisurely look at the bootstrap, the jackknife, and cross-validation. The American Statistician, 37(1), 36-48.

- p451, l15: Are the yearly and seasonal runoff not more dependent on rainfall than on actual catchment behaviour?

Yes they are. We are not talking of climate/catchment drivers yet, though.

- p451, l23: The total hydrograph does not seem to be a useful signature too me, because it is too complex. However, in the following of the paper it turns out that again a specific element from the hydrograph is used (the integral scale), it is maybe better to describe earlier in the paper that actually this signature is used.

We realise that our definition of signatures has been misunderstood and we will try to solve in the revised paper. One thing are the signatures (annual flow, flow duration curve, etc.), and another thing are the measures we can use to describe the signatures (e.g., the measures of the signatures used in the assessment are those defined in Section 4). We will make clear the distinction between signatures and measures of them from the beginning in the revised paper. Regarding the hydrograph as a whole as signature, we believe that it is not as complex as the forcing and catchment internal characteristics. It's an integrated signal of the entire system, thus reducing complexity a lot (e.g., it tells about volume and timing of runoff). Therefore we believe that considering the hydrograph as a signature of catchment runoff variability is appropriate.

- p451, l26: This sentence does not seem to be correct.

Thanks to Tanja Euser for pointing this out. The sentence "Each signature is meaningful of a certain class of applications of societal relevance" means that the signatures that we consider all are extracted from the same streamflow record but to reflect vari-

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ability at different timescales. At the same time each signature has a long history of applications in hydrology, for example flood frequency curves are used for design flood estimation, flow duration curves for design and operation of hydroelectric schemes, etc. We will be clearer in the revised manuscript.

- p452, l20: (of 213 catchments in Austria) instead of (Austria)

Ok

- p452, l21: Why are these two methods selected?

Because in Austria they have been developed and used. We will add an extended description on the methods and their parameterisation in section 3, which were performed before and independently from our analysis, and which are published in Parajka et al. (2005-2007), Merz et al. (2011), Skøien et al. (2006) and will be published in Parajka et al. (2013, in prep).

Merz, R., Parajka, J., and Blöschl, G.: Time stability of catchment model parameters: Implications for climate impact analyses, Water Resour. Res., 47, W02531, doi:10.1029/2010WR009505, 2011.

Parajka, J., Merz, R., and Blöschl, G.: A comparison of regionalisation methods for catchment model parameters, Hydrol. Earth Syst. Sci., 9, 157–171, doi:10.5194/hess-9-157-2005, 2005.

Parajka, J., Merz, R., and Blöschl, G.: Uncertainty and multiple objective calibration in regional water balance modelling: case study in 320 Austrian catchments, Hydrol. Process., 21, 435–446, doi:10.1002/hyp.6253, 2007.

Parajka et al. (2013, in prep.) Optimal station density for runoff regionalisation by Topkriging, in preparation, 2013.

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.

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- p452, l23: How well can we predict

We prefer the original “How well do we predict”

- p455, l14: Add a reference for comparative hydrology.

We will add a citation to Falkenmark, Malin, and Tom Chapman. Comparative hydrology: An ecological approach to land and water resources. The Unesco Press, 1989.

- p455, l17: Add a reference for the richness of signatures across the world.

We will add a citation to Falkenmark, Malin, and Tom Chapman. Comparative hydrology: An ecological approach to land and water resources. The Unesco Press, 1989.

- p455, l24: Add a reference for both methods.

The references are (and more will be added) in Sections 3.1 and 3.2.

- p456, l5: I think it should be physically based instead of physics-based.

We prefer “physics-based” for models at laboratory scale.

- p456, l10: Why is the HBV model selected as rainfall-runoff model?

Because in Austria it has been developed and used operationally.

- p456, l16: This sentence does not seem to be correct.

Thanks to Tanja Euser for pointing this out. The sentence “The detailed description of model concept is given, e.g. in the Appendix...” will be changed to “The detailed description of the model concept is given, e.g. in the Appendix...”

- p456, l19: sites

Ok

- p456, l24-27: This sentence does not seem to be correct.

Thanks to Tanja Euser for pointing this out. The sentence “This regionalisation method

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is based on idea to find a donor catchment that is most similar to ungauged site in terms of its catchment attributes" will be changed to "This regionalisation method is based on the idea to find a donor catchment that is most similar to the ungauged site in terms of its catchment attributes"

- p457, l4: How many stations for precipitation and evaporation observations are used?

Details are given in Merz et al. (2011). The data set used in this study includes measurements of daily precipitation and snow depths in 1091 stations and daily air temperatures at 212 climatic stations.

Merz, R., Parajka, J., and Blöschl, G.: Time stability of catchment model parameters: Implications for climate impact analyses, Water Resour. Res., 47, W02531, doi:10.1029/2010WR009505, 2011.

- p457, l3/l23: Did the authors first predict the runoff and afterwards calculate the signatures?

Yes.

- p458, l4: How many stations for precipitation and evaporation observations are used?

Details are given in Merz et al. (2011). The data set used in this study includes measurements of daily precipitation and snow depths in 1091 stations and daily air temperatures at 212 climatic stations.

Merz, R., Parajka, J., and Blöschl, G.: Time stability of catchment model parameters: Implications for climate impact analyses, Water Resour. Res., 47, W02531, doi:10.1029/2010WR009505, 2011.

- p458, l23: Table 2 is mentioned earlier in the text than Table 1, it is maybe better to change the numbering of the tables.

Agree. Done

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- p458, l22: These seem to be results, instead of a description of the methods.

These efficiencies refer to the entire time series and are not part of the assessment of the ability in capturing the signatures. This is the reason why we prefer to have them here and not in section 5.

- p460, l15: This criterion is not clear for me, maybe add a reference.

For integral scale we added the reference "(see e.g. Blöschl and Sivapalan, 1995, page 255 and reference therein)."

Blöschl, G., and M. Sivapalan. "Scale issues in hydrological modelling: a review." Hydrological processes 9,3-4 (1995): 251-290.

- p461, Are these performance measures used before, if yes, maybe add a reference.

Normalised error and coefficient of determination are standard measures for method performance.

- p462, l10: How well can we predict

We prefer the original "How well do we predict"

- p462, l17: How many rainfall stations are used and where are they located?

We do not understand this question referred to this part of the paper. We are willing to provide the response if more specifics will be given.

- p464, l5: Did the authors perform a visual inspection or did they weighted the performance measures?

The sentence refers to visual inspection of the figures. Quantitative measures are also reported on the figures and commented though.

- p464, l10: Add reference or formula.

The spearman correlation coefficient is a standard measure of correlation. We added

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the reference to a statistical book (Kottegoda Rosso, 1997, page 281).

Kottegoda, Nathabandu T., and Renzo Rosso. Applied statistics for civil and environmental engineers. Wiley-Blackwell, 2008.

- p464, l16: This sentence does not seem to be correct.

We do not see a problem with the sentence “The highest correlations are obtained with catchment area.”

- p464, l16: Catchment size is probably more clear than catchment area.

For consistency to companion papers we prefer the wording catchment area.

- p465, l7: The authors list a number of arguments why the performance is better for larger catchments. Could it be that larger caore comparable due to averaging and mixing of different processes and that therefore, regionalisations give better results?

Exactly. We will have the sentence “As the catchment size increases some of the hydrological variability is averaged out due to an interplay of space-time scale processes, thus improving hydrological simulation.” later in the paper.

- p468, l9: Add a reference.

Salinas, J. L., Parajka, J., Viglione, A., Rogger, M., Sivapalan, M., and Blöschl, G.: Comparative assessment of predictions in ungauged basins – Part 2: Flood and low flow studies, Hydrol. Earth Syst. Sci. Discuss., 10, 411–447, doi:10.5194/hessd-10-411-2013, 2013.

- p469, l12: One of the reasons the Top-Kriging method works better is, according to the authors, because of the stream gauge density. Why are not the same (amount of) gauges used for both methods?

We will perform the regionalisation using Topkriging on the same stations used for the HBV regionalisation. This will provide a more consistent basis for the comparison, as

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correctly stated also by Referee #3.

- p469, l16: So, if I understand it right, Top-Kriging is most suitable to use when a lot of data is available for the surrounding areas, how does this relate to PUB?

It does. PUB is about unavailability of runoff data in the location where they are needed, not about unavailability of data in the region (or in the same catchment). In response to this comment we will clarify this point more clearly in the introduction of the paper.

- p470, l3: regionalisation instead of regionalation.

Ok, corrected

- p481-484: Although papers are published in color, they are often printed in black and white by readers. Therefore, I would change one of the squares in a circle or diamond, to prevent the use of different colors.

For this type of visualisation we believe that colours are better than symbols because gradual colours can be used. Regarding the two catchments in the example of Fig. 1, we will differentiate them with one square and one diamond as suggested by Tanja Euser.

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