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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.

A. Viglione et al.

viglione@hydro.tuwien.ac.at

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We would like to thank the reviewer for her/his constructive comments on the manuscript. In the following Referee #3's comments are in *italic* and our responses in plain text.

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?

The remark of Referee #3 is correct. In the original manuscript we have evaluated two

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methods that were performed before and independently from our analysis, and which are published in Parajka et al. (2005-2007), Merz et al. (2011) and will be published in Parajka et al. (2013, in prep). In the revised paper we will perform the regionalisation using Topkriging and the same stations used for the HBV regionalisation. This will provide a more consistent basis for the comparison, as correctly stated by Referee #3.

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.

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.

Yes. We will stress more the focus on assessment, which actually is the point of the paper. Regarding the calibration strategy, we have used what was available for Austria (see Merz and Blöschl, 2004, and Parajka et al., 2005). In section 3 of the revised paper we will add an extended description on the methods and their parameterisation (see also the reply to Referee #1 for details).

Merz, R. and Blöschl, G.: Regionalisation of catchment model parameters, Journal of Hydrology 287, 95–123, doi:10.1016/j.jhydrol.2003.09.028, 2004.

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.

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)

We will cite and discuss these works in the revised paper, even though in our paper we did not use runoff signatures to calibrate the models but rather we assess models' performance in getting the signatures right.

Detailed comments:

- the abstract has no outlook

We believe that an outlook is not needed in the abstract but is needed in the discussion section (which will be added). We will point out that future work will look at changes.

- 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);

This is a very good suggestion for making Section 2 clearer: it is meant to describe the regimes in Austria to give the context of the study. We thank Referee #3 a lot for this suggestion.

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

We will add references to the literature on comparative hydrology.

- 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?

We will perform the regionalisation using Topkriging and the same stations used for the HBV regionalisation. This will provide a more consistent basis for the comparison, as correctly stated by Referee #3. By the way a work not yet published is actually aiming at answering the question on what is the effect of gauges density on the performance of Topkriging (Parajka et al., 2013, in prep).

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

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

This would be interesting to analyse and we will mention it in the discussion. However we have chosen q95 because it is widely used as measure for low flows and in order to be consistent with the companion paper 2.

- 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

Section 3 will be extended in order to give details on the calibration procedure. Also, the sensitivity of Nash-Sutcliffe to the regime will be discussed (citing the paper of

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Schaefli-Gupta which deals with this issue).

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

The new processes are those mentioned in the sentence preceding the statement, i.e., "The landscape characteristics and behaviour and dominant processes change with increasing catchment area. For example, headwater catchments tend to be steep with landslides being dominant, whereas flatland catchments, which are larger, tend to be dominated by groundwater aquifers, wide floodplains and frequent inundations etc., and therefore exhibit very different flow paths. Also, catchment area is a key variable in the aggregation behaviour of rainfall runoff generation processes." At the small catchment scale the runoff generation may be dominated by overland flow mechanism while in larger areas subsurface flow and also routing in the river network could become more and more important (Robinson et al., 1995WRR).

Robinson, Justin S., Murugesu Sivapalan, and John D. Snell. "On the relative roles of hillslope processes, channel routing, and network geomorphology in the hydrologic response of natural catchments." Water Resources Research 31.12 (1995): 3089-3101.

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

The statement is meant to be general. We will consider if to add a quantitative measure on how many catchments are nested in the Austrian case study.

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

We thank Referee #3 for pointing this out. We will make the discussion 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

The sentence has been changed to "As the catchment size increases some of the hydrological variability is averaged out due to an interplay of space-time scale processes,

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thus improving hydrological simulation.”

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

Because runoff variability is more deterministic being temperature driven. When a strong seasonal cycle is present, it is not very hard to predict, i.e., it’s not hard to guess no runoff in winter and snow-melt in spring in Austrian Alps. We will clarify this in the revised manuscript.

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

Yes. The text has been removed.

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

Yes

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

We thank Referee #3 for pointing this out. The sentence “For the rainfall–runoff model the regionalation performance tends to increase with elevation as a result of snow processes in the mountainous catchments, which are easier to predict” refers more to the other 2 companion papers than to this one. In the discussion section that we will add to the revised paper we will make this clear.

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

Probably Referee #3 refers to Table 1 because there is no Nash-Sutcliffe value in Table 2. In Table 1 Nash-Sutcliffe are reported for daily runoff.

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

Ok

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