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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 the reviewer for her/his constructive comments on the manuscript. In the following Referee #2's comments are in *italic* and our responses in plain text.

The paper treats an interesting subject, which is important and relevant to the hydrologic community, and appropriate for HESS. The main problem I had while reading the paper is a sense of superficiality, in the way the paper is written, referenced, structured, and also in the methods used to perform the analyses. Essentially the paper compares regionalization based on Topkriging and of the model parameters of the HBV model. The only conclusions that are supported by the analysis are that (i) the regionalization based on Topkriging performs better than the HBV model parameters regionalization,

and (ii) that regionalization performance may depend on some catchment characteristics or signatures. But the paper is written in a way that it seems to want to cover much more ground, calling into question the co-evolution between climate and vegetation, landscape and soils, the catchment organization, the relation between climate and catchment characteristics, etc. These are very interesting aspects, but talking about them, especially out of context, does not help to proof them. This is a paper about something else, and it should focus on what it is about.

We agree with Referee #2 and we will revise the introduction in order to better explain the objectives and match the results. We will add a precise description of the methods (Ch. 3) and a separate discussion section in the revised version of the paper. In the discussion and conclusions we will discuss more in detail some implications of the results of our analysis, such as the ability of rainfall-runoff models to estimate extremes compared to dedicated statistical methods (see reply to Referee #1). We will also discuss the results in this manuscript jointly with those in the two companion papers.

1. The introduction is very self-centered. The authors are talking about huge topics (PUB, the relationships between catchment signatures, landscape and climate, etc.), which have puzzled the hydrological community for decades. The introduction however references 4 papers in total, all from the Authors' previous work, and 2 of them are the companion papers of Salinas and Parajka. The introduction does not exhaustively illustrate what has been done, and does not convincingly show what needs to be done. What this paper adds to previous work is a question that should not remain after reading the introduction.

Our introduction was mentioning PUB and the advancement obtained in regionalisation in the last decade. We will change the introduction and give to the issue the right relevance. Even though the topic of the three papers as a whole has much to do with this, this last paper must be seen as a complement to the other two, with the difference that here two methods are applied on the same region (i.e., these are two methods which were evaluated as the best ones in Austria in previous studies such as Merz and

Blöschl, 2004, and Parajka et al., 2005) and therefore can be more consistently compared. However we will provide more relevant references of the literature on prediction in ungauged basins (e.g., papers of Thorsten Wagner's group on the use of signatures for regionalisation purposes). Regarding the novelty of the paper, the main point is the comparison between signatures, and secondly between classes of methods (in our case, TK vs. HBV). One general message of the paper (along with the two companion ones) is that extremes are harder to estimate with process-based methods than with statistical ones. This is reflected on the fact that all studies considered in Salinas et al. (2013) for regionalisation of extremes use statistical methods. We will discuss this more extensively in the new discussion section.

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.

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.

2. Section 2 seems to me at least out of place. It presents and discusses some results, and it is not described how these results are obtained. Is it summarizing previous work, or is it part of the work that has been done in this paper? In the first case, where are the references. In the second case, where is the methodology. It seems to me that the signatures are defined after being used. . .

The aim of Section 2 is to give the context for the study. This is in line with the suggestion of Referee #3 of having a section that describes the regimes and the signatures separate from the regionalization methods. We will revise Section 2 in order to separate

more clearly the context (hydrological regime in Austria) from the analysis. Figures 1 and 3 are just presenting data (i.e., no methodology is involved). In Figure 2 the signatures were mapped by Top-kriging with the aim of providing information on the spatial variability of the hydrological regime in Austria. We believe that presenting information on the river network is clearer than presenting it associated to gauging stations alone. Figure 2 is not aimed to assess the predictive performance of the method (e.g., through cross-validation). Regarding the definition of signatures there is a misunderstanding that 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. The signatures are defined in Section 2, where some examples are shown. The measures of the signatures used in the assessment are defined in Section 4, which is in our opinion appropriate. However we will make clear the distinction between signatures and measures of them from the beginning of the revised paper.

3. Section 3 presents a distinction between statistical and process based regionalization methods. It is not clear if this is the authors own definition (in this case it needs to be better motivated, or applied to the authors own work and not generalized), or if it is common practice to do so (in this case it needs to be referenced).

This is our definition, which is rather obvious. We will change the text so that this will not be ambigous any more (i.e., "we call...").

4. Section 3.1 presents the regionalization based on the HBV model. Is this reusing results from previous studies, or are these results generated within this case study? The paper does not explicitly states this.

In response to this comment we will clarify the methodology applied in the assessment. In the paper we have reused the methodology of Parajka et al. (2005), described in Section 3.1, with the data and calibration approach of Merz et al. (2011).

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.

5. Section 3.2 same here. Are these new analyses, or have these been presented in previous papers? Ideally these questions need to be answered in the introduction, where it should be clearly apparent what is the new contribution of this paper. If Sections 3.1 and 3.2 are presenting results from previous studies, this obviously diminishes the added value of this paper to previous work.

In response to this comment we will revise Section 3.2 providing more information on the Top-kriging approach. We will also add specifics of the methods that have been used in the revised introduction. Sections 3.1 and 3.2 are meant to explain the existing methods that have been used in the literature (Parajka et al., 2005, Merz et al., 2011, Skøien et al., 2006, Laaha et al., 2013). Similarly to the case of the two companion papers, the methods have not been developed/calibrated specifically for this paper. We believe that the added value of this paper (and the two companion ones) is the comparative assessment between signatures and methods, which is not normally done.

Laaha, G., Skøien, J.O. and Blöschl, G.: Spatial prediction on river networks: comparison of top-kriging with regional regression, Hydrological Processes, doi: 10.1002/hyp.9578, 2013.

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.

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.

6. Section 4.1: here it is not clear which catchments have been selected for blind testing and why.

The 213 catchments are a subsample of the catchments used for the regionalisation with the process-based and geostatistical methods. They are those where both methods have been used and exclude catchments with significant anthropogenic effects. For the revised paper, also in reply to Referee #3, we will present the results obtained by using the same dataset (these 213 stations) for both methodologies, which will make the comparison more consistent.

7. Section 4.2: there might be a problem with Equation 3. It does not seem to do what specified in words above, as the numerator and denominator are not averages.

We believe that equation is correct. We will clarify it better in the revised paper. We reason in term of runoff volumes. We changed the text to “i.e. the Pardé coefficient for month i, is defined as the mean monthly runoff volume for the month i divided by the mean annual runoff volume”. The equation is how we compute it (both numerator and denominator should be divided by the number of observed years, which cancels).

8. Equation 6: I guess you are confusing the coefficient of determination with the Nash and Sutcliffe coefficient.

Nash-Sutcliffe is another way to call the coefficient of determination, and it is traditionally used when one deals with time series. Since Eq. 6 deals with spatial values instead, we would prefer to use the standard wording “coefficient of determination”. Note that Eq. (6) is a general definition of the coefficient of determination and would correspond to the squared Pearson correlation coefficient if yi are estimated through linear least squares regression.

9. Section 5 is in fact a results and discussion section, although it would be better to separate them. The absence of a separate discussion section is a drawback of the paper.

We agree with Referee #2 and we will add a separate discussion section to the revised paper.

10. The results section mainly focuses on regressing some measures of errors versus catchment attributes or signatures. This in my opinion tells something about which types of catchments are easier to predict, however it does not seem to me the appropriate way to answer, for example, the question of paragraph 5.2: "In what way do the predictions depend on climate and catchment characteristics?". I think the Authors should find some other ways to extract this type of information from their material.

We mean "dependence" not necessarily as causality but as correlation. That's why we prefer to use this wording for the section heading. In the introduction section we will discuss the causality-correlation issue.

11. Overall, the paper says something about whether the HBV based regionalization is better than topkriging, and on whether prediction errors may depend on some catchment characteristics or signatures. Other conclusions or considerations are speculative, and should be removed.

The conclusion will be rewritten in order to avoid misunderstandings and making sure where interpretations of the results are hypothesis or are supported by the analysis.

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