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Hydrology and Earth System Sciences Discussions

Interactive comment on "

Geostatistical regionalization of low-flow indices: PSBI and Top-Kriging" *by* S. Castiglioni et al.

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Referee#3 raised relevant and enlightening points. We are persuaded that addressing them will improve significantly the clarity of the presentation and the practical usefulness of our study.

Our reply is structured as follows, we report all referee's comments (indicated by RC) together with our reply (denoted by AR, Authors' Reply).

RC:

Thank you for the opportunity to review the paper titled "Geostatisical Regionalization

of Low-Flow Indices: PBSI and Top-Kriging". I very much enjoyed reviewing this paper and I found to the paper to be interesting, relevant, and generally well-written and organized.

I have several specific comments enumerated below as well as some technical corrections, which suggest a few places in the manuscript where a small amount of additional detail would help clarify some of the methods.

Specific comments:

1) How sensitive are the principal components to the characteristics used? In other words, how robust are these results, particularly if less or more catchment characteristics were available than used in this study. Would the physiographic space, and thus the performance of the method, change much? For example, on p. 7245, lines 10-15:

It might be worthwhile to comment on how important permeable area is in the PCA results, with reference to figure 2, given the difficulties of quantifying this attribute. What effect would removing permeable area from the analysis have?

AC:

This is a very sensible comment, particularly relevant for the percentage of permeable area P, which might be harder to retrieve than the other catchment descriptors utilized in the study (see e.g., Brath et al., HSJ, 2001). In order to address it, we will reapply the PSBI approach by dropping the information on the permeable area. Table 2 of the revised manuscript will report performance indices for PSBI applied with or without the information on P, and the differences in terms of performance will be discussed in the text.

RC:

2) I think it is difficult for the reader to see the distinction between PBSI and Top-Kriging methods as described early in the text. While both methods are remarkably well-explained in a very short amount of words, I think it would be valuable to contrast

a few of the important differences in section 4. Where are the methods identical and where do they diverge?

AC:

Good point! An introductory comment will be included at the beginning of section 4 (Structure of the analysis and results) that will briefly remark the main similarities and dissimilarities between methodologies and recall the different geographical scales of the comparison (regional and catchment scales), together with its main aims.

RC:

3) I was left to wonder how zero-flow values were handled and modeled by the methods.

Is it possible that the presence of zero values could be biasing the results in some way?

AC:

The study area does not include catchments for which empirical Q355 is equal to 0. Nevertheless, the prediction of low-flow indices in ephemeral catchments is an important topic (e.g., Croker et al., HSJ, 2003) and a comment will be included in the manuscript. In particular, both techniques predict the streamflow indices of interest through a weighted average of empirical values of the index itself. Therefore, the application of the approaches in ungauged basins may be biased for catchments characterized by extremely high streamflow values (underestimation may tend to prevail) and for extremely low values (overestimation may tend to prevail), zero flows included. In this context though, the term “extremely” has to be intended relative to the vast majority of available empirical data. Concerning PSBI, if a significant portion of the physiographic space contains primarily catchments for which the empirical values of the low-flow index of interest are equal to zero, PSBI predictions may not necessarily be positively biased. The same holds for Top-kriging, positive bias may be limited if significant portions of the study stream network are characterized by zero flows.

RC:

4) I believe that omitting the sites that performed poorly with Top-Kriging could be misleading. This performance reveals a serious limitation to Top-Kriging that could have important implications for application of the method to flow estimation in ungaged basins. I think the methods did not perform competitively with one another when these sites are included.

AC:

We do not share the same opinion with Referee#3 on this point. The performance of Top-kriging is presented with or without three peculiar catchments. The reasons behind the limited performance of Top-kriging in cross-validation for these peculiar sites is thoroughly analyzed and interpreted (see from P.7241, L. 26 to P.7242, L. 16). Therefore, we believe that the reader is provided with a complete, fair and objective comparison of the methodologies. Moreover, this result is important for highlighting and interpreting the complementarity of the two methodologies. Developing an approach that combines the advantages of both is the objective of future analyses, but this point will be commented also in the discussion section of the revised manuscript, as requested by Referee#1.

RC:

5) Both methods have the advantages of mapping the variance of the estimates. I wonder if one method had lower variances than the other. Perhaps this topic might be better placed in another paper but I think understanding the variance of the estimates is also a very important piece to this type of comparison.

AC:

We agree with Referee#3, mapping the variance of the estimates is an important feature of both methods. We do also agree on the fact that this topic should be addressed in another paper. The main focus of this study was to compare the methodologies in

the context of PUB, and therefore on the basis of the accuracy of their predictions in ungauged sites. Nevertheless, a reference to this point will be made in the revised discussion section.

RC:

6) I felt that the discussions and conclusions were well-reasoned and nicely written, with a nice contrast between the results and methods on p. 7248, lines 20-25 and p. 7248-7249, lines 27-2.

Technical corrections:

p. 7235, lines 3-6: Please add a sentence about how the Q355 is computed. Is this computed from the median annual duration curve?

AC:

Empirical values of Q355 was retrieved from the period of record flow-duration curves. The interest herein was on long-term low-flow indices, rather than low-flow indices for a hydrologically typical year. An additional sentence will be provided in the Introduction (around L. 6 of 7235).

RC:

p. 7239, line 4: Please specify that you mean air temperature. Also, I think 'regime' needs to be pluralized.

AC:

The suggested changes will be incorporated.

RC:

p. 7239, line 17: Please add a phrase or sentence about why universal kriging was used.

p. 7239, line 20: Please use more specific terms rather than "kriging interpolator" and

"deterministic interpolators." What exactly do those terms mean in the context of this study?

AC:

The first paragraph of subsection 4.1.1 will deeply be revised (as requested also by Referee#1) in order to provide a clearer presentation of the main outcomes of Castiglioni et al. (2009), explaining why universal kriging was selected in this study among all interpolation techniques tested by Castiglioni et al. (2009) (e.g., ordinary and universal kriging for kriging interpolators, inverse distance weighted -IDW- and Thiessen polygons among the deterministic interpolators).

RC:

p.7240, line 19: Please add what variable the empirical variogram is modeling.

p. 7240, line 20: Replace the word "data" with the specific data that is used in this study.

AC:

The sentence "A modified exponential variogram is adopted to model the empirical variograms; the model is fitted to sample data through a WLS method implemented in the R-package Rtop (Skøien, 2009)." will be modified as:

"A modified exponential variogram is adopted as theoretical model (see e.g., Skøien, 2009); the model is fitted to the empirical variogram (see e.g., Figure 3) through a WLS method implemented in the R-package Rtop (Skøien, 2009)."

RC:

Table 1: Please add a header or footnote describing the abbreviations in the table.

Consider adding the median value so that the reader can have some insight into the distribution of the values.

AC:

A description of all terms will be added in the caption.

25, 50 and 75th percentiles will be also included to have a more detailed description of the distribution of the values.

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