

Interactive comment on “

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

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Thank you for the opportunity to review the paper titled "Geostatistical 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?

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?

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?

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.

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.

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.

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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?

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

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?

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.

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.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 7231, 2010.

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