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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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The paper gives a fair, open and interesting comparison of two methods for the estimation (prediction/interpolation) of runoff at ungauged locations / catchments. One of them, Top-kriging, relies on spatial correlation of the measured values, and takes into account the differences in support and the nested nature of catchments and subcatchments. The other, PSBI, obtains predictions by applying a kernel smoother in feature space, here the first two principle components obtained from a set of 9 variables related to geomorphology and lithology. The paper is written very well, and deserves publishing.

The paper concludes that the two approaches perform similar, and that gains can be expected when the strenghts of both methods were combined. Could the authors also reflect on how ambitious and complicated this would be? In case such a solution were not feasable for reasons of constrained resources, in which cases would the authors expect Top-kriging to beform better, and when would PSBI be the prefered choice? What about the data requirements of both?

On page 7248, line 20: the authors argue that "Top-Kriging and PSBI are both geostatistical interpolation methods." I would argue against this. In my opinion, explicitly addressing, quantifying, and exploiting autocorrelations in geographic space sets geostatistical methods apart from other statistical methods. PSBI is a kernel smoothing technique that uses a covariance based kernel, and does not explicitly address spatial autocorrelation in observations or residuals.

Using this covariance kernel in the space of two principle components issues a number of new questions. Was stationarity a fair assumption in this space? Were the principle components scaled, and if yes, how? If not, was some sort of anisotropy assumed or fitted? If not, on which ground was isotropy assumed? And further: why was the kernel limited to 2 PC's? Why discard 30% of the information, and why not use a positive definite kernel in all dimensions? (Note that in this case the spherical covariance would not provide a positive definite model, but the exponential or Matern family would.)

When a dimension reduction is needed, why did the authors not choose the linear combination(s) of features that best explained the variability in the target variable, in Q_{355}? Partial least squares but also ridge regression would do this. How did the PSBI compare e.g. to a direct multiple linear regression? This might reveal the benefits (or disadvantages?) of restricting to two dimensions and doing the smoothing in feature space.

A number of relatively minor issues follow:

- On page 723, I find the use of Q_{355} and Q_{95} confusing as the 355 and 95 refer to different properties, number and percentage of days.

- Page 7237, Line 16: I suggest to replace "most of the information" with "most of the variability". Some critical information might show up in lower order PC's, and the hope of course is that this is not the case, but there is no guarantee as PCA can't tell the difference between information and variability.
- 7239, L 24: The authors should mention whether PCA was done on the covariance matrix or the correlation matrix. As they evaluate eigenvalues larger than 1, it seems to be the correlation matrix, otherwise this criterion does not make sense, but the choice needs to be mentioned explicitly.
- 7240, L 9: R package Gstat should be written gstat. R package Rtop is not on CRAN; could the authors indicate where it can be found?
- 7241, L 13: you did not update the PCA, ..., but ... – to me suggests this could have mattered. Maybe you could further stress here that the PCA computation does not involve the Q_{355} data at all, so leaving those out does not affect the PCAs.
- 7258: Figure 3 should number sub-figures as a, b and c, and properly refer to those in the figure caption.

1. Does the paper address relevant scientific questions within the scope of HESS? Yes.
2. Does the paper present novel concepts, ideas, tools, or data? Yes; both methods have been published, but so far not compared side by side.
3. Are substantial conclusions reached? Yes, but I'd invite the authors to try to reach somewhat further.

4. Are the scientific methods and assumptions valid and clearly outlined? Yes, although I disagree with some of the terminology used.
5. Are the results sufficient to support the interpretations and conclusions? Yes.
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes, I believe so; as R was used, making the R scripts available might be a means for the authors to further open up details.
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes, I think they do.
8. Does the title clearly reflect the contents of the paper? Yes.
9. Does the abstract provide a concise and complete summary? Yes.
10. Is the overall presentation well structured and clear? Yes.
11. Is the language fluent and precise? Yes.
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes.
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Only one; see above.
14. Are the number and quality of references appropriate? Yes.
15. Is the amount and quality of supplementary material appropriate? I did not notice any supplementary material.