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Interactive comment on "Ordinary kriging as a tool to estimate historical daily streamflow records" *by* W. H. Farmer

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Reviewer Comment 1: *The paper "Ordinary kriging as a tool to estimate historical daily streamflow records" by Farmer W.H. shows a comparative assessment of kriging techniques, exploring the performances obtainable employing Ordinary Kriging, under different model settings, for the prediction of daily streamflow series in ungauged basins. The paper is well written and is rather complete in all its section, the topic is of wide interest in the hydrological field, thus I believe it is suitable for the publication in HESS after some little improvements that in my view the author should consider to take into account.*

Author Response 1: I wish to thank the reviewer for his close attention to the details of this work. I very much appreciate the effort that was clearly put into this review and

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Discussion paper

think it will significantly improve the impact and communication of this work. I look forward to future discussions.

Reviewer Comment 2: *Even if there is a relationship between the covariance function C(x1, x2) between two data points x1, x2 and the variogram, which is, by definition: (h) = 1/2E[(Z(x2)−Z(x1))2] , where h = x2−x1 is the spatial Euclidean difference between the two data points and E is the expected value of the squared increment of Z, relative to the spatial lag h. All the textbooks and papers on geostatistics refer to the variogram, rather than employ the covariance directly, as the major controller of the spatial correlation. C and are two sides of the same coin, because (h) = C(0)−C(h), though the variogram has some more features, which is why it is the main function to look at. For instance, most of the variables that might be referred to as "spatial fields" may (or may not) have a nugget effect, which is a unique feature of the variogram. Moreover, there are some variables that might be "non-stationary". In this case, one can denote non-stationarity as the variogram diverge and never reach the "sill", while non-stationarity might not be seen from the covariance. I think the mathematical notations and equations (1), (2), (3) and (4) (L25 P4) are formally incorrect as they refer to the covariance, rather they should refer to semivariances of the increment z(x + h) − z(x), both theoretical or experimental (see for examples, Cressie, 1993; Journel and Huijbregts, 1978). Although there is a way to employ the covariance matrix too, which derives from the optimization of the prediction variance, the author did not report the correct one. I would recommend the author to rewrite the system of equation (2), (3) and (4) and stick with the variogram. Furthermore the author cites Skøien (2006) as the refernce for solving the kriging system. There are a couple of mistakes with this reference: (1) that paper focuses on solving an "adapted" ordinary kriging linear system to fit with regularized variorgams, so maybe this is not the best choice for someone who wants to discover more about kriging tecniques and (2) that paper never reports covariaces within matrices of the kriging linear system to solve, rather it reports correctly variograms.*

Author Response 2: The reviewer is correct that kriging systems are typically presented in terms of the semivariogram. However, for spatially stationary processes, as is assumed here, the kriging system defined in terms of covariances and semivariances results in identical weights. It is therefore, not formally incorrect to present the system in terms of covariances. The only advantage of considering semivariances explicitly is in the case on nonstationary processes. (The covariance structure still retains the nugget effect, which the reviewer notes as C(0).) Furthermore, while Skøien et al (2006) presents a modified kriging system, their introduction of basic kriging is sufficient; I will add additional references. To ensure consistency with other hydrological applications, the kriging system will be rewritten to be framed in terms of the semivariance.

Reviewer Comment 3: *I think that the comparison with Top-kriging here is not informative as it should and might be even misleading. Firstly, it does not report the best model setting. Even if the author specifies here and there that the comparison with Top-kriging is not definitive, I would strongly recommend to point out that Top-kriging model performances might not be the best obtainable in this study area. Or, in case preliminary analyses have shown that it is instead the best model setting, this should be clearly said throughout the manuscript.*

Author Response 3: I agree with the reviewer that this is not an exhaustive comparison of ordinary and top kriging. With a more targeted exploration and application, it may indeed be possible to improve either model. I will certainly add some verbiage to document the limitations of this comparison.

Reviewer Comment 4: *Secondly, I think the paper, which is intentionally unbalanced towards the two ordinary kriging methods, does not accomplish the assessment Top-kriging deserves. Indeed, the latter is actually an ordinary kriging too, technically it is a "modified" ordinary kriging, where the modification relies just on the variogram. The author instead groups this method together with DAR and QPPQ, whereas it should be grouped with the ordinary kriging methods. Concluding, does this comparison with*

Top-kriging reflect what the title says? At the very end, is this informative? Thus, should the Top-kriging be removed from the comparative assessments with other models?

Author Response 4: The reviewer raises an interesting point, suggesting that it might be appropriate to exclude top-kriging from the comparison. The reviewer is correct that top-kriging is a modification of ordinary kriging rather than a completely different method. While I agree with the spirit of the reviewer's comment, I do feel that it is important to contextualize the ordinary-kriging result in the realm of other hydrologic kriging applications, most of which employ top-kriging. Furthermore, the inclusion of a basic comparison with top-kriging shows that modification to the ordinary kriging scheme only produces marginal improvements. I will revise the text to make this last more point more clearly and to point out that top-kriging is more akin to ordinary kriging than any other method considered.

Reviewer Comment 5: *L 24-25 P10. The author conclude that kriging techniques are biased and inaccurate in the tails of the distributions, and prove it with Fig. 4. This is somehow understandable and even quite normal. The kriging techniques are weighted average. Predicting streamflows within a leave-one-out cross-validation, when the lowest or highest streamflow is removed, plus it is perhaps orders of magnitude lower or higher than streamflows from donor sites, it is predictable that the outcome shows upward or downward biases, respectively, in those regimes. I think this thought might be taken into account, or at least pointed out clearly, maybe after those lines or elsewhere in the first sections.*

Author Response 5: The reviewer points out that, in many simulations, especially in cross-validation, bias in the tails is an expected result of the calibration framework. I strongly agree and have tried to make this point in lines 5-14 on page 12. I will revise this discussion in the light of this reviewer's and other reviewers' comments. However, a full exploration of the effects of calibration, validation and application of models on tail behavior, as it is equally concerning even outside of the realm of kriging, is conceptually beyond the scope of this paper. Future research is addressing this issue.

Reviewer Comment 6: *In case the author adopted unconstrained kriging methods, that is when kriging weights are both positives and negatives, predicting low flows may lead to negative estimates. Would be interesting to know, if any, how many negatives are produced.*

Author Response 6: The reviewer is correct that unconstrained kriging methods can produce negative weights. However, because the predictand is a logarithm on streamflow (line 18 of page 5), negative values of streamflow are never produced.

Reviewer Comment 7: *By taking the logs of the standardized streamflows, the author implicitly removes zero flows, if any, from the dataset. Is there any catchment with intermittent regime? If so, would be interesting to see how zero flows are treated.*

Author Response 7: Zeros were not removed, but were replaced with a value of 0.00003 cubic meters per second, as described in lines 10-13 of page 4. Previous work by Farmer (2015) and Farmer et al. (2014) found this value to have only a limited effect on the comparative results. Additional information on the prevalence of intermittent streams in this data set is available from Farmer et al. (2014) and presented in response to a previous reviewer.

Reviewer Comment 8: *L. 33 P9: it is not clear whether or not the author adopted the leave-one-out crossvalidation for the DAR and QPPQ method too. I think so, but I would recommend to rephrase and be a little clearer about the cross-validation procedure used for all the methods reported. Would be even informative to know if any other crossvalidation methods have been used in the past for DAR and QPPQ.*

Author Response 8: As the reviewer assumes, leave-one-out cross-validation was used for all methods. I will revise the text to make this more apparent.

Reviewer Comment 9: *L. 31-32 P6. I think the parentheses might be removed and extending the text for a few lines might improve the reasoning.*

Author Response 9: The parenthesis will be removed. Per recommendations from

another reviewer, this statement will be slightly expanded to explicitly point out the differences between an averaged model and a pooled model.

Reviewer Comment 10: *L. 32 P6, I've noticed the author use to put the punctuation mark before the right hand parenthesis, please correct throughout the manuscript with ")."*

Author Response 10: For complete statements or sentences contained within parenthesis, it is correct to place punctuation within the parenthesis. If a fragmentary statement that cannot stand alone as a sentence is made parenthetically within another sentence, the punctuation should be placed outside of the parentheses. I will closely review the usage throughout this manuscript.

Reviewer Comment 11: *L2 P8. "[. . .] developed form", should be "developed from".*

Author Response 11: Corrected.

Reviewer Comment 12: *L8 P8. "[. . .] between the 5% and 15% non-exceedance probability" should be perhaps "between the 5% and 15% error"?*

Author Response 12: Corrected and clarified per other reviewers' comments.

Reviewer Comment 13: *L30-31 P10. There are two "similarly" adverbs very close one to another. Please, consider to substitute one of them.*

Author Response 13: The second usage of 'similarly', appearing on line 31, should have been removed.

Reviewer Comment 14: *Fig. 4 height might be increased. In general Fig. 4 form factor might be changed to improve the readability of the figure itself.*

Author Response 14: I will work with the editorial team to ensure readability of this figure.

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