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Interactive comment on "A geostatistical framework for estimating flow indices by exploiting short records and long-term spatial averages – Application to annual and monthly runoff" *by* Thea Roksvåg et al.

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General comments:

This paper presents a method for estimating low flow indices based on short (one year) records. The novel method uses a Bayesian approach that treats annual flow index series of multiple gauges as Gaussian random field (GRF), that is linearly decomposed into one GRF representing the long-term average pattern, and one GRF representing the annual residual (deviation from the long-term average) pattern. Two approaches to

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Discussion paper

localize catchments in a geostatistical framework are presented, one called areal model and one called centroid model. The models are cross-validated and compared with the Top-kriging method for a larger Norwegian dataset, with some of the catchments nested, but more of them not nested. The evaluation is performed for the single-year prediction case, equivalent to in-filling single-year gaps in annual index series. The paper concludes that the proposed method is well suited for exploiting the information stored in short records of runoff data, which is seen a main benefit compared to Top-Kriging.

The paper addresses the problem how to perform optimal predictions of streamflow indices combining information of long and short records in the gauging network, which is an important science question within the scope of HESS. The objective is presenting and evaluating a novel estimation method. The paper is generally well written and easy to follow, but has some potential shortcomings that need to be amended before the paper can be considered for publication.

1. Scope of the paper

There is some inconsistency concerning the actual scope of the novel estimation method and how it is evaluated. The title suggest the method should be able to predict flow indices exploiting short and long records, what should include two cases: (A) estimating long-term average indices and interpolating annual values, and (B) models to fill in missing years in annual flow index series. However, in the paper only case B is evaluated. Filling in gaps in annual flow index series is indeed an important question, but long-term average indices may be more relevant, as they are basic requirements in a number of water management tasks. I would therefore see a greater value in assessing both cases, rather than restricting the approach to a method to fill-in missing values. For evaluating within the larger scope, the Top-kriging approach should be adapted by introducing observation weights representing the length of the record as proposed by Skoian (2006).

2. Geostatistical methods

The methods are generally well described and easy to follow for the reader. This is a general strength of this paper. The linear decomposition of space-time patterns and runoff into a long-term average and an annual residual pattern is a promising idea and the proposed Bayesian approach provides an elegant solution to perform joint estimation of both components. Most methods appear sound, but I have concerns about the actual value of the areal model. It is claimed to “ensure that the water-balance is close to preserved for any point in the landscape” and is presented as similar or equivalent to the Top-kriging approach with this respect. However, defining average runoff as the average point runoff in a catchment is not sufficient to ensure water balance in any sub-catchment. This requires that the “right” point runoff patterns are summed up and averaged, which cannot be observed and need to be estimated. But I cannot find in the methods how the disaggregation of point-runoff, underlying the hierarchical geostatistical model, is actually performed. In Geostatistics, this relates to the change-of-support problem which is resolved by using regularized variograms for various area, which also constitutes the core of the Top-kriging approach. No such approach is mentioned to be underlying the areal method.

A second concern is about the proposed extension to “monthly runoff”. I think this term is misleading, as monthly runoff this is usually understood as a series of monthly values. In this paper, the focus is still on an annual runoff index series, but with a different index than the annual mean. One could rephrase this point, from an extension to monthly runoff, to an evaluation of the novel method for individual seasons. As processes change, the correlation length changes as well, and this may explain the different performance of methods.

3. Evaluation method

The evaluation so far is only focussing on the global, “overall” performance. It would be interesting now to add more specific assessments, that give insight how the methods perform in which situation, and why.

Firstly, the underlying approach is a GRF decomposition by a linear model (Eq. 4). It would be interesting to see the importance (magnitude) of each effect. This will be informative about whether the yearly deviation from the annual pattern is rather constant, or has a spatial structure. This can be summarized in a table and in an additional plot of maps showing the spatial variability of the annual residual (range of xj(u)) as compared to the average spatial pattern c(u).

Secondly, it would be interesting to see how the proposed model performs in different estimation settings along the stream network, that define the river network estimation problem, such as small headwater catchments, interpolation between gauges, and catchments which are not nested (for example see my own attempts in (Laaha et al., 2013, 2014). This will enable the authors to show how well the areal model is able to incorporate the water-balance constraint in a useful way as stated in the introduction, and how far it is equivalent to Top-kriging in this respect. And more general, this would give an evidence which models perform well in which situation. The demonstration can be based on summary statistics stratified for the three estimation settings, and on regional examples (region with highest nestedness, and a region with very low nestedness).

Section 5.5 which gives a demonstration of an annual runoff map for southern Norway (also introducing a simpler centroid model) seems to deviate from the direct scope of the paper, appears rather uninformative and can be deleted. It would be more informative to see the components of the full model instead (see my previous comment).

4. Discussion

The Discussion section is merely a repetition of the results and findings stated before. This redundancy needs to be avoided. This can be partly obtained by a clearer separation of results and discussion section, where some parts of the results section may be shifted to the discussion (e.g. comparison with other studies, Section 5.4)

Additionally, the section should address how far the findings depend on the particular Norwegian setting and how far they can be generalized. One particularity stated in the introduction is that Norway has a very specific meteorological situation, with a pronounced east-west precipitation pattern related to orographic enhancement in front of the mountain range, that is common and prominent for each year of the data set. This suggests that taking an average pattern and shifting it, either by a constant or by a spatially corrected constant as used in this paper, will have much potential. There will likely be a lower value in other situations, when precipitation processes dominate that occur on a smaller space-time scale, such as convective events. Another particularity is the rather low gaging density, giving rise to a low number of sub-catchments along the river network. How far does this effect the performances of models should be discussed.

5. Conclusions

The conclusions (also in the Abstract) remain a bit general, and should be sharpened around the actual performance of the proposed method.

Specific comments:

Study area: "This leaves 195 catchments for testing with areas ranging from 7.5 km^2 to 18934 km^2." (p4 line 16). How many of them are nested?

Section 3.1.4: Please make clear whether such regression methods have been used for estimating annual discharge.

Section 3.3.1: monthly rainfall is not the scope of this paper, why not using annual runoff as an example?

Section 3.2.1: "Likewise is c(u)a spatial effect that models the long-term spatial average of runoff, or the spatial variability caused by climatic conditions in Norway..." (p10, line 30) - I think this interpretation is not sound, it is the combined effect of climate and catchment characteristics that lead to spatial variability of runoff. (This interpretation occurs several times throughout the MS). "... while xj(u) is a year specific spatial effect that models the spatial variability due to annual discrepancy from the climate." (p10, line31f): This could also be formulated in a clearer, more meaningful way.

P11, line 19: Centroid model: "This alternative does not require preservation of water-balance and can be used for any environmental variable". Think the model "does not allow" for preservation of the water balance and is therefore not well suited for runoff and runoff-related variables, but can be applied for other environmental variables.

P14, line 16: "spatial variability" . . . is it rather space-time variability?

P14, line 30ff: Second property – preserving the water balance: Here the authors state what the model should be able to do. They need to show it is capable to do it. In contrast the example uses a constant point-runoff within the sub-catchments, which is not reasonable.

Several times: Hydrological stability is rather an abstract term that can be interpreted in different ways. Consider using low inter-annual variability instead.

Gregor Laaha, 11 October 2019

References

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