Response to the comments on the manuscript (HESSD-2019-415) "A geostatistical framework for estimating flow indices by exploiting short records and long-term spatial averages - Application to annual and monthly runoff"

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This is the authors' answer to referee Dr. Jon Olav Skøien. We would like to thank him for several insightful comments and constructive suggestions.

His review suggests, as the other reviews, that we include an analysis of the mean annual runoff. We suggest to do this as described in our response to Anonymous referee # 2. We think that performing this analysis (and adding some more discussion) will resolve most of the referees' concerns regarding the case study and choice of record length in the case study (record length 1). The cross-validation will be a case study to illustrate the model properties of our two field model, while the assessment of mean annual runoff will be a test of the method on a more realistic hydrological dataset.

The rest of the comments of Dr. Jon Olav Skøien are mainly specific comments that can be resolved by adding some more discussion/rephrasing sentences. We comment some of them here. The remaining comments will be taken into account by adding more discussion/rephrasing.

1 Specific comments

"P25 – description of Figs 11-12. It is mentioned that UG has problems predicting large values of runoff, but I'd say it is just as difficult with small values. Additionally, the negative values should be mentioned here, not only in the conclusions. It seems there are no negative values for annual runoff for UG? I think this result is partly related to the fact that the data don't follow the assumption of being normally distributed, which should be discussed. A transformation method such as logtransform could avoid this problem, although log-transformed data on the other hand don't go so well with the linear aggregation assumption, see also Clark (1998)."
As the referee suggests we did not transform the data because it doesn't go so well with the linear aggregation assumption in the areal model. However, it could be done for the centroid model. This can be mentioned.

"Figure 1b is a bit difficult to understand, including the reference to 0-4 catchments. These types of figures are generally difficult to make nice, so I'm only asking the authors to test if other visualizations could work better. The lines of 4 catchments is difficult to see. Maybe it would be possible to add river network on top, to understand the catchment order?"
We will search for a better way of visualizing.

"P27L10 I do not think it is correct that observations from nearby catchments have a larger impact on the target catchment for the centroid model when rangec > rangea. It should be the opposite, large range means that stations further away will also get a weight in the interpolation."
Yes, this is correct and the sentence should be rephrased. The point here was that the range ρ_c for the areal model is very low, lower than for the centroid model. That means that the predicted runoff for an ungauged catchments that is located far away from gauged catchments, goes towards the mean value β_c instead of being influenced by neighboring observations. The low range ρ_c for the areal method might be connected to our relatively strong constraint on the measurement uncertainty for the areal observations. The tendency of providing a low range might also be a contributing

reason for why the areal model performs poorer than Top-Kriging for the ungauged case. This can be mentioned in the discussion.

"Then also the next sentence (L12-15) seems somewhat incorrect. If the values in Table 3 are divided by the similar values in Table 1, maybe the differences could easier be interpreted in light of the range?" We agree. Again, the point was that a low range means that the predicted runoff for the ungauged catchments goes towards the mean β_c. We will look through this part and rephrase.