

Interactive comment on “Spatial analysis of precipitation in a high-mountain region: exploring methods with multi-scale topographic predictors and circulation types” by D. Masson and C. Frei

Anonymous Referee #2

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General Comments:

The topic of this manuscript is relevant for mapping precipitation fields in high-mountain regions, and is publishable in HESS after, probably, a moderate revision by addressing or at least discussing the following two issues:

1) In the abstract, it is stated first that ‘Again, the stratification by circulation types and the wind-aligned gradient predictor do not improve over the single predictor KED model. Similarly for daily precipitation, information from circulation types is no improving interpolation accuracy.’ Then it says that ‘...they support the common practice of

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using climatological background fields in the interpolation of daily precipitation.’ Is ‘climatological background fields’ referred to the same as ‘circulation types’? If so, the authors need to rethink what information they want to deliver, because it seems that results conflict with conclusions. If not, the authors need to do further work to reword this part so as to avoid potential confusion.

2) In Section 3.1, it is stated that ‘In all our applications, the semi-variogram is assumed to be exponential with a nugget, sill and range as parameters. The semi-variogram is assumed to be isotropic.’ The authors should at least discuss why it is reasonable to assume an exponential model rather than, for example, spherical, and why it is reasonable to assume an isotropic covariance function. It seems that the zonal patterns of the observed precipitation field as shown in Figures 1, 2 & 4, do not support such an isotropic assumption.

Other specific comments:

In addition, there are other minor points. For example,

1) some of the presentations can be optimized. I don’t think in the present study the ‘Box-Cox transformation’ is more important relative to the ‘likelihood-based estimation procedure’ because, as emphasized by the authors, ‘the utilization of a likelihood-based estimation procedure is central in our application.’ I believe, most readers, like me, may be more willing to see what the ‘likelihood-based estimation procedure’ is and why it plays a central role in the current application.

2) Following the authors, box-cox transformation with a power parameter of 0.5 will transform all wet days to be -2 (though I am not sure whether a fixed value of 0.5 is the best choice or not). This means that after transformation daily precipitation becomes -2-inflated from zero-inflated. I am wondering how these ‘-2s’ are treated when interpolating daily precipitation because on one hand this is critical for the representation of the intermittent nature of daily precipitation field, and on the other hand daily precipitation time series contains a great amount of zeroes.

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