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Hydrology and Earth System Sciences Discussions

Interactive comment on "Mapping snow depth return levels: smooth spatial modeling versus station interpolation" *by* J. Blanchet and M. Lehning

Anonymous Referee #3

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1) Page 3, Section 2, first paragraph. The contents of the extreme value theory is very wide, therefore the authors should be more specific about which topic of the extreme value theory they are focusing on. For instance, the first sentence is too generic. I think they mean that one part of the extreme value theory focuses on the concept of componentwise maxima. Indicate which pages of the Coles book concern this topic so that the readers can easily refer to the relevant pages. In the second sentence I think the authors mean 'the asymptotic behavior of ....' instead of the 'the statistical behavior of '.

2) Page 3, Section 2, second line from the bottom. The GEV distribution is the limiting distribution of the standardized componentwise maxima. The authors should be more precise here about specifying this, without going into too much detail. Moreover, in the last sentence they repeat the explanation of the acronym GEV when it has already been specified in the introduction. There is no need to repeat it again.

3) Page 5, Section 3, from line 125. I think the authors should distinguish the real process (snow depth and annual maxima) from the mathematical one used in order to describe it. This allows them to therefore assume for instance, a smooth spatial process in order to model the annual maxima of snow depth, where the real one would not, and could not be so smooth. This also allows them to be more consistent with the notation in Section 4, lines 136 and 146. Moreover, they should specify the type of underlying process (snow depth) i.e. $Yi(s)>=0$, for all $s \in S$.

4) Page 5, Section 4, second line. In order to avoid confusing the readers the spatial index should be distinguished from the temporal one i.e. if "i" has been used for time then "j" should be used to denote the different sites.

5) Page 6, Section 4, lines 159-160. Are auto -regressive models of order less than 6 still valid for these locations?

6) Page 8, Sections 5.1.2 and 5.1.3. When the linear and Spline-based regression models are described I feel that a few words should be spent on the error assumptions, otherwise the least squares method of estimating the model parameters may not necessarily be the appropriate method. Moreover, when the eta function represents the scale function, then formulas 7, 9 and 10 do not necessary guarantee that the scales are positive which is instead required. I think that the eta functions should be connected to the predictors by a link function and then some comments on suitable choices should be provided.

7) Page 10, Section 5.2, line 253. If I have not missed it before the acronym DEM is used for the first time. Its complete name should be specified.

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8) Page 12, Section 5.3.3. In the penalized spline literature one problem is to determine the number of knots and their positions. For the latter problem, in the univariate case many naive solutions have been proposed showing at least a satisfactory performance in practice, without necessarily being rigorous. In the bivariate or multivariate case, instead the problem is still open. For example, quantile-type solutions suffer the curse of dimensionality. When the authors talk about the best choice I think they are referring to the number of knots. Its this correct? If the function to be estimated is not very smooth then also the position can have an impact on the estimate of the surface. I am not expecting that they provide a solution to this problem but I really would like to understand how they chose the position of the knots. It seems that they completely avoid talking about it.

9) Page 12, Section 5.5. In Table 2 there is something really obscure to me. You have computed the four scores' quantities also for the fitted GEV distributions at each single site. As you have pointed out at in line 314 there isn't any spatial model. Therefore, how is it possible to predict extreme values at the validation sites using the information from the "fitting" sites if there isn't any spatial connection? How do you choose which estimated parameters to use in the prediction from the set of the 84 available for each parameter? According to me, If you have just fitted the GEV distribution also for the validation sites then this does not show how this method is good for predictions.

10) Page 12, Section 5.5, line 318. Could you please explain better what the following sentence means? "This suggests that the fitted models are too unstable". According to me, the exact interpolation methods obviously preform worse in prediction given that they are forced to be equal to the individual estimates, where instead the regression models are not and are therefore more adaptable when considering unobserved data.

11) Page 16, Section 6.2. The authors assume to consider the product of the N univarite marginal densities, which are known, instead of the unknown N-variate density . Therefore in these terms the annual maxima are assumed independent and not approximately independent.

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12) Page 16, Section 6.2. The authors should be aware that this marginal approach has been used also by Smith (1991) in an unpublished manuscript for accounting for spatial dependence in extremal rainfall.

Smith, R., L. (1991) Regional Estimation from Spatially Dependent Data. Unpublished manuscript.

13) Page 17, Section 6.2. I find it improper when the authors state that the TIC has been recently rediscovered by Varin and Vidoni (2005) in the framework of composite-likelihood. Varin and Vidoni (2005) talk about the composite likelihood criterion which is a generalization of the TIC because it differs for different and more general assumptions and so do other details related to the likelihood theory. Even though TIC and CLIC use the same misspecification theory, they are not the same! It is fine if the authors feel more conformable using the TIC instead of CLIC, they are free to use whatever they want but the term 'rediscover' should be removed because it shows that the authors have not read the Varin and Vidoni (2005) carefully and understood it.

14) Insert please the unit of measure in Figure 8.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 6129, 2010.