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Hydrology and Earth System Sciences Discussions

Interactive comment on "Estimating strategies for Multiparameter Multivariate Extreme value copulas" *by* G. Salvadori and C. De Michele

Anonymous Referee #2

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It is a well-written paper, devoting to discuss three new parameter estimation approaches for the Multiparameter Multivariate Extreme Values Copulas that the authors introduced earlier in the Water Resources Research (Salvadori and De Michele, 2010). Though this manuscript can be viewed as a supplement to Salvadori and De Michele (2010) given that the only major difference is on how the parameters are estimated, the reviewer does agree that it has sufficiently new contribution to stand alone. My various technical suggestions are listed below:

Major Concerns 1. Although the authors have specified clearly that this manuscript was only for methodological discussion (similar claim in Salvadori and De Michele, 2010) and not for real application, the parameter estimating strategy is however in-

evitably related to the real problem. It is exciting to have 37 sets of quadruples of annual maxima to analyze with. However, as noticed by the authors, many of the maxima of different gauges are not corresponding to a common flood event. Therefore, the dependence between unrelated maxima does not provide useful information from the regional flood frequency perspectives. To my personal point of view, a qualified joint peak flow probabilistic model should be able to reflect the flood consequence from an upstream gauge to downstream ones. Therefore, the appropriate strategy of sample selection is also very important and needs to be realistic (some mathematical scarification may be needed). The current quadruple sample sets used in this study cannot preserve the correlation of the flood movement from gauge to gauge. It deserves for further investigation.

2. On page 7572, line 22 and page 7580, line 27, the authors claimed their techniques to be physically-based, which seemed to be an over-statement. If I understand it correctly, instead of estimating the local parameters using all streamflow gauge observation all together, the authors identified a suitable subset based on the inter-gauge distance. The approach could be justifiable for extreme rainfall analysis, but it may not be appropriate for streamflow. For example, let's consider a gauge G1 located on a tiny stream that is fairly close to the nearby gauge G2 on the main stream. If there is another gauge G3 also on the same main stream, but with a larger distance G2-G3 than G1-G2. It will be very likely that the correlation between G2-G3 will be higher than G1-G2, given the difference in river orders. I would suggest the authors to rephrase their statement.

3. The authors proposed three estimation approaches in this paper, namely the nearest neighbor (1-MEV), cluster (c-MEV), and p-MEV. However, only 1-MEV and p-MEV results are shown in the case study, so there is no way to compare the performance of c-MEV. Being a methodological discussion paper, it seems a requirement that the authors should show some results of c-MEV, even if it is in a very trivial form. Plus, the justification for not showing the c-MEV results is simply "the necessarily small size of

the clusters in a set of only four stations would be of little practical interest", which is not satisfactory. Please bear in mind that the practical purpose has been excluded earlier by the authors. The readers need to know the relative performance among 1-MEV, c-MEV, and p-MEV.

Minor Issues 1. Please check the format of Eq (24).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 7563, 2010.