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Hydrology and Earth System Sciences Discussions

Interactive comment on "Joint return periods in hydrology: a critical and practical review focusing on synthetic design hydrograph estimation" *by* S. Vandenberghe et al.

Anonymous Referee #2

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While reading the paper I found very interesting the different definitions of return period possible when dealing with multivariate extremes. I must say that I was confused by these definitions and the way they are compared. The fact that also other reviewers were confused implies that additional explanations need to be given in the revised version of the paper. My 2 main comments follow:

1 - From a practitioner point of view, it would be useful to discuss the implications of the choice of each definition for the joint return period in Section 3. For example the regression approach in Section 3.1 is, to me, like caring for the 100yr peak (if

X is peak) and providing an additional information of what is the expected value for the volume (if Y is volume) associated to that peak, assuming a linear relationship between the two applies (which does not seem the case in Figure 5). The definition in Section 3.2 is similar but does not assume this linear relationship. More interesting, but harder to figure out, are the following definitions (section 3.3 and 3.4). Both identify couples of peak-volumes associated to a single return period, which I guess defines the probability of the peak OR the volume to be exceeded (and the selected volumes are those associated to the maximum annual peaks only, if I'm correct). These are events (joint peak-volume) which are more or less likely to happen, thus the one with largest joint probability is chosen in Section 3.3. What does this choice mean from a practical point of view? And what is the difference with the "super-critical" event definition?

2 - Another concern is about the procedure used in the paper. The copulas are fitted to 500 years of simulated data, the goodness of fit is discussed and the events corresponding to the same return period based on the different definitions are compared. By reading the paper it is not clear why this exercise is performed. Is the objective to find out marginal distributions and copulas that fit well the rainfall and rainfall-runoff models? If so, the authors should state and discuss it clearly at the beginning of the paper. It would be also interesting to see the sensitivity of the method to the record length. The ideal would be to simulate many more years (100000) and then using subsamples of different length. I realise this is a lot of work and goes beyond the scope of the paper, but maybe a short discussion on the sample length issue would be appropriate in the paper.

Other reviewers have raised the same points, and they have done it more clearly than myself. Nevertheless I have stressed them again because, especially for point 1, their clarification is necessary for this paper to be a practical review, as stated in the title.

Detailed comments:

Page 6787, lines 22-25: "different choices of the conditioning variable lead to different results", this is true in practise but not in theory, isn't it?

Page 6790, equations: what does the double-sided arrow mean?

Page 6791, line 7: why should one select the point with largest joint probability?

Page 6791, line 19: the concept of super-critical events is key because separates the multimentional space into two regions and allows the use of a univariate return period. More details should be given about how super-critical events are distinguished from an hydrological point of view rather than from a statistical one. Why should a practitioner use this definition of return period?

Page 6793: Eq. 11: Kc(t) = f(*) but * does not contain t. Is it correct?

Page 6793: again the concept of most likely design event. Why should one select the point with largest joint probability?

Page 6799, line 10: it is said that the joint distribution of Qp, Vp and D need to be estimated. It is not clear to me why, which should be explained more at the beginning of the paper. Why are 500 years simulated and then a statistical model is fitted? Is it because the authors want to identify the statistical model most similar to the rainfall-runoff model? Why not simulating more years (100000) and not fitting a model but assuming that the empirical frequency is given by the simulations?

Figure 2: lognormal or gumbel (or other) reduced variates should be used in order to see the behaviour of the tails.

Figure 5: the linear regression seems not a good choice in this graph, particularly for high magnitudes of peak and volume.

Figures 6, 7 and 8: the y-axis should be selected in order to see the high quantiles.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 6781, 2012.