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Hydrology and Earth System Sciences Discussions

Interactive comment on "Bivariate return period based on copulas for hydrologic dam design: comparison of theoretical and empirical approach" *by* A. I. Requena et al.

Anonymous Referee #2

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Bivariate return period based on copulas for hydrologica dam design: comparison of theoretical and empirical approach

General comments: This might potentially be an interesting paper, but in my opinion it needs to be improved in several ways before it can be considered for publication. The current version is difficult to read and it is not always clear what the authors are trying to communicate or how they have conducted their analysis. The authors have clearly studied couplas in great details, but forget that not all interested readers will automatically possess the same degree of knowledge on the topic. In places I think the manuscript could be made more helpful to the reader by more clearly defining the

terminology.

I would have liked to see a better case for the move from single to multivariate FFA. For example, it is not the multivariate nature of flood events themselves that is a problem (several hydrological design problems can be successfully completed using only peak flow). It is the need for flood volume and the inflow design hydrograph that requires more complex methods. The authors could also acknowledge that design flood events and continuous simulation models have been proposed (and used) in engineering hydrology.

The paper lacks identification of a knowledge gap in need of filling followed by a clear scientific hypothesis. The introduction points more towards a more routine application of a set of reasonably well-known models to a case study. Reading further it becomes clear that the authors find it not straight forward to select between candidate models based on the available dataset. They try different goodness-of-fit tests which show little difference. In particular, I like Figure 4 which shows that very different results can be obtained using different model structures that all could have been chosen. Of course, the authors give away their true intentions on page 570 line 13-14, where the spread of simulated values in the upper tail is characterised as 'undesirable'. I would say that this is not a very good argument as, by nature, you will know more about the central body of the distribution (that is where you have the most data) and relatively little about the variability of the floods in the upper tail. Thus, to define the spread as undesirable might be correct from the perspective of application, but from a more scientific viewpoint it points towards the need for new techniques to better inform and constrain the upper part of the distribution. In traditional single variable FFA this would typically be done using regional methods, i.e. attempting to bring in more information from nearby locations with flood data from a distribution reasonably similar to the site of interest. Another possibility would be to use a continuous simulation approach and generate multiple events from a coupled stochastic rainfall – rainfall runoff modelling system and see how these events behave. Of course, this would also have limitations

based on the properties of the modelling system when extrapolating.

Specific comments Section 2.1.1: Please explain what a Chi and a K-plot is. Yes there is a reference, but since you use it in the paper I think this should be explained more clearly here.

Section 2.1.2: Consider introducing the theta parameter in connection with Eq. (1)

Section 2.1.2: What is a maximum pseudo-likelihood method?

Section 2.1.3: What is a 'quasi-inverse' and how is that different from an 'inverse'?

Section 2.1.3, line 13: 'assess' rather than 'prove'

Eq (4): I am not sure how to evaluate the expression in this equation. What is the purpose of the '1' in the summation (I think this must be a typo)?

Section 2.1.3, line 19-20: this makes absolutely no sense to me. Where do you get the estimated coupla from?

Eq. (5): Is this equation correct? It looks like the probability of a probability?

Section 3, line 18-22: I am a little confused. Are the peaks and the volumes extracted from the same events, and selected based on annual maximum peak flow values?

Section 3: Is the Gumbel distribution a reasonable choice? Given the sophistication of the couplas method, this choice appears a bit unjustified.

Section 4.1: The Chi and K-plots have not been sufficiently described for me to find the graphs in the Figures useful or informative.

Section 4.1: The 'undesirable wider spread in the upper tail' is presumably a function of the structure of the coupla when fitted to your data, but the question is not if it is undesirable but rather if it is true?

Section 4.1, line 25-27: I think there is something not right with this sentence? The comparison is between MPL and IKtau, not Sn?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 557, 2013.