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Interactive Comment

Interactive comment on "Flood hazard in the Mekong Delta – a probabilistic, bivariate, and non-stationary analysis with a short-termed future perspective" *by* N. V. Dung et al.

Anonymous Referee #1

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General evaluation:

This manuscript contains potentially numerous interesting and original results but takes up too many topics at the same time leaving too little space for detailed presentation of the basics of the methods used, their justification and discussion of their added value. Moreover, some arbitrary choice need additional justification and discussion (use, suitability and interpretation of the copulas, non-stationnary statistical models). As a result, the manuscript in its present form gives an impression of a complex combi-

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nation of various sophisticated modelling tools (large scale application of an hydraulic model, original calibration method for this model, copulas, non-stationnary statistics...), reflected in its title: a challenging exercise from a technical point of view but which scientific usefulness has to be demonstrated by the authors. This lack of critical analysis in the manuscript in its present form explains my relatively severe rating of its scientific and presentation quality. I suggest that the authors focus the manuscript on some specific aspects of their work and spend more time to present and justify the use of the proposed methods and to conduct a critical analysis of their results. A discussion of the possible limits of the proposed approaches (see detailed comments) is completely missing in the manuscript. Since the hydraulic model has already been the topic of a publication, I would advise the authors to focus this publication on the use of copulas and of non-stationary methods in flood frequency analyses. Some additional work may be necessary to enrich the discussion part (see detailed comments).

Detailed comments:

1. Introduction: I am really surprised that no hazard analysis exists for the Mekong Delta. An international Mekong Commission has been active for years! But the authors should know. P278L25: The method has to be presented in more details in the manuscript. The technical and polysemic term "cluster analysis" can hardly be understood without a context in this introduction. The term "probability of occurrence" is inappropriate and confusing in the context of copulas. What is computed is the probability that both values of the two considered variables are simultaneously not exceeded. Probability of occurrence does only make sense in the univariate case (strictly speaking for discrete random variables moreover). The interpretation of the copulas as return periods is problematic to me. P279L8: technical terms like "Pareto-optimal parametrization" should not be used before having been defined.

2. Data and Methods: P280L6: What is a "multilayer circle channel" ? All the cited locations should be visible on figure 1. The location of the Tonle Sap lake is unclear, Kampong Cham, Reeds are not mentioned. The described flow paths could be indi-

cated on the map. 2.2. The flood inundation model has been presented in a previous publication. It is too shortly presented here to be of any use for the readers. This part could be skipped only mentioning the main features of the model used (type of model, calibration and validation periods, 2 models finally selected based on the impossibility to adjust both at the same time (Nash and flood extent). 2.3. P283L15 : please add "biggest in the world for similar watershed areas". In fact 0.12 m3/s/km2 is not an extreme value for smaller size watersheds (see Gaume et al., 2009, JoH for a recent inventory of extreme discharge values over the world). L24: the selected unit is really strange, why not having chosen to express this volume in millimeters (volume divided by the watershed area), which is a hydrologically more natural unit? L283. If there is a high linear correlation, what is the use of considering both variables in a copula? Moreover, are copulas really suited to represent the pair of variables (flood peak and flood volume)? The correlation will certainly remain if not increase for large events, when it has a tendency to decrease for most of the copulas with the magnitude of the variables... Can the authors comment on this ?

3. Copulas P286L1: The sentence is impossible to understand for a non-mathematician and of no use for readers of NHESS. Please remain simple (see wikipedia for instance). L18: Tail dependence obviously exists between the two considered variables (flood volume and peak). I ask again, is it reasonable to consider copulas without tail dependencies? Is the Gumbel copula not the only possible choice for this study ? P287L10: The use of the AIC criterion should be justified. Moreover, what is the relative weight of the number of parameters in the criterion AIC ? It is very low according to the results presented in table 3. The use of the AIC does not differ from the use of the standard loglikelihood criterion and this must be said. Please correct "likelihoood" in eq. 5. Be also consistent with table 3 : AIC should be positive according to eq. 5. A last comment on this. The idea behind the use of AIC is to limit the uncertainties associated with an increased number of calibration parameters. Bayesian MCMC inference methods help now to tackle this question. They could have been used by the authors with great profit. P288L7:fig6 is nice but does not help to

really judge. "The Gaussian copula is selected" : what a pity according to the previous comments ... P288L16: no details are given about the non-stationary distribution - linear evolution in time of the distribution' mean and variance I suppose - and readers are referred to the PhD of the author. I strongly disagree with that option. This part, which is original and not published until now should be detailed (in an appendix if necessary) or discarded from the paper. Important questions that should be discussed are the following : "are the trends statistically significant?", "do the introduction of additional parameters to account for non-stationarity not increase tremendously the inference uncertainties ?" "How well can these parameters be estimated ?". These are important informations for the discussion of the results. Again the implementation of Bayesian MCMC inference methods could have brought a lot here. L25: the whole procedure used to "remove time" has to be explained for this part to be of any benefit for the readers. 3.2.3. P289L9: The statement "the return period is the reciprocal probability of occurrence" is simply false. The return period is the reciprocal of the probability of exceedance or of non-exceedance. A probability of occurrence does only exist for discrete random variables strictly speaking. Continuous variables have only density functions. Moerover, I totally disagree with the extremely confusing interpretation of copulas in terms of return periods. Equations 6 and 7 are correct : there is a return period for the combined exceedance of the two values (AND case) or the exceedance of one of the two (OR case) of course, but the authors should make up their mind depending on their case study. We see here the limits of the copulas. Ideally, Monte Carlo simulations should be used and generated events ranked according to their forecasted consequences which is not done here. An other important remark: due to the high correlation between the volume and peak, the difference between the computed AND and OR "return periods" should not be so large. Are the results presented on figure 9, not a sign of the inadequacy of the adjusted Gaussian copula that does not sufficiently account for the dependency between the two variables, this being compensated by the selection of the AND combination? The authors should discuss this and not remain on the simple acknowledgement of line 25 that deserves explanations.

5. The title is not well selected : the 100 year hydrograph can not be defined precisely with the selected procedure (see previous comments). The authors should not maintain the confusion. With the proposed method, they do not provide a method for frequency analysis, the linked between computed inundations and return periods is lost. They should acknowledge this.

I stop here the detailed comments, hoping that the previous suggestions and questions will help the authors to focus, enrich and strengthen their manuscript and especially the presentation and discussion of their results.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 275, 2013.

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