Reponse to the comments of the reviewer #1, Anonymous

By Nguyen Viet Dung

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 combination 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).

REPLY: We thank the Referee for the constructive and insightful comments. We considered the comments thoroughly and we will completely rework the manuscript. In the new version we will:

- Present the focus of the study more clearly
- Go deeper into details of the statistical methods, justify/explain the selection of the different methods, and add a critical discussion of the results and methods in terms of uncertainty, applicability and limits; we will also discuss the pros and cons of the stationary vs. the non-stationary approach.
- Add a section on sampling uncertainty associated to the bivariate statistics
- Considering the above additions/improvements, we will drop the part of the hazard maps in order to give room for the points above and to keep the manuscript at reasonable length. The hazard maps and their derivation will be described in a separate manuscript, after this manuscript containing the underlying statistics is hopefully accepted.

We hope that with these major revisions the manuscript will meet the comments of both reviewers.

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.

REPLY: To the best of our knowledge, no hazard analysis exists for the Mekong Delta so far. The Mekong River Commission published Flood reports annually, but these do not contain statistical analyses of the floods, but a description of the flood event in terms of extent, severity, damages, peculiarities, etc. The only published statistical analysis known to the author was done by Adamson et al (1999) for Vientiane in Lao PDR. For the Mekong Delta such a study cannot be found in literature. This may be partly explained be the fact that the current flood management practices in Vietnam simply takes the flood event in 2000 as design event. Probabilities are not considered in the current practices to the authors knowledge.

Adamson, P. T., Metcalfe, A. V., and Parmentier, B.: Bivariate extreme value distributions: An application of the gibbs sampler to the analysis of floods, Water Resources Research, 35, 2825-2832, 1999.

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.

REPLY: To our understanding a cluster analysis is a common tool in many scientific studies, thus we did not explain it in detail. However, we can add some explanation to avoid misunderstandings. We acknowledge the inappropriateness of the term “probability of occurrence” in this multi-variate context. We will us “probability of exceedance” of “probability of non-exceedance” in the revised manuscript.

P279L8: technical terms like "Pareto-optimal parametrization" should not be used before having been defined.

REPLY: This will be dropped as it is part of the hazard map derivation.

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 indicated on the map.

REPLY: We will update figure 1 in order to illustrate the geographical setting better (see figure below), as well as the channel system of the Vietnamese part of the Delta will be explained more clearly.

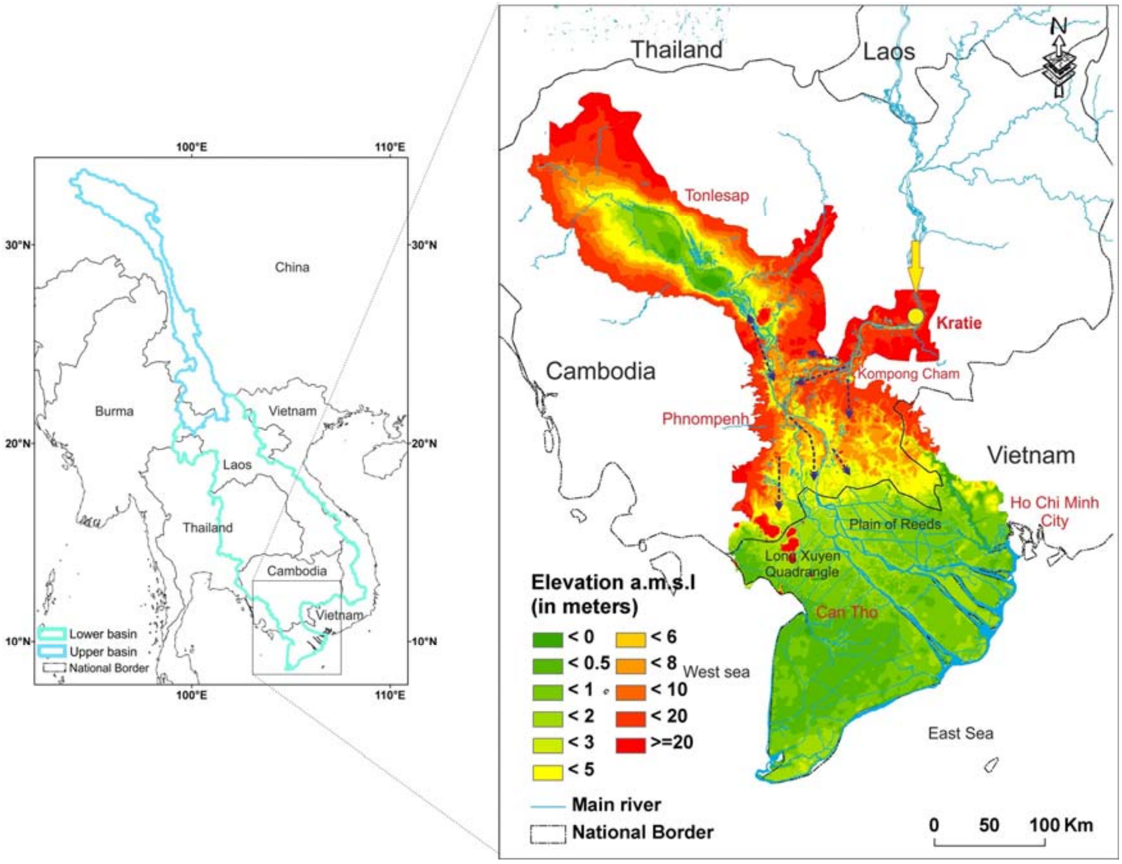

Fig. 1. The Mekong basin (left) and the Mekong Delta (right); the yellow filled circle indicates Kratie, the yellow arrow indicates the main flow input to the Delta at Kratie, Cambodia; the dotted lines with arrows indicate the major flood flow paths.

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).

REPLY: This part will be dropped completely, see general revisions above.

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).

REPLY: Of course, we talk about similar large basins. We do not intend to compare a 800.000 km^2 large catchment with small flash-flood prone headwater catchments. This will be made clear.

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?

REPLY: The unit was used in the frequently cited paper by Yue et al. (1999). Of course, we can use also the common unit like km^3 for the analysis. But we prefer to use this unit because it can be easily calculated from the available daily discharge data. It can also be easily transferred back into synthetic hydrographs with daily discharges for the hazard maps derivation. In other words, we prefer this unit for computational ease.

Yue, S., Ouarda, T. B. M. ., Bobée, B., Legendre, P., & Bruneau, P. (1999). The Gumbel mixed model for flood frequency analysis. Journal of Hydrology, 226(1-2), 88–100. doi:10.1016/S0022-1694(99)00168-7.

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?

REPLY: We thank the Referee for the comment. However we think that the linear correlation by its name hints only on the co-linear evolution of the two random variables. But, in reality, the dependence structure of the two random variables is much more complex (especially for the non-stationary process) than that represented by a simple linear correlation. Serious flood events can, and in fact often do occur when Q and V differ from this general linear correlation. An analysis of the historic disastrous flood events shows just this: annual floods with the same maximum discharge can be disastrous depending on the variability of the volume and vice versa. We will show this in the revised version of the manuscript. As a consequence a bi-variate statistical description is mandatory, a uni-variate statistic of annual maximum discharge and an associated linear regression to volume does not describe the hazard appropriately.

Although the upper tail dependence of the observed data can generally be graphically diagnosed based on a simple Chi-plot (Abberger, 2005) or non-parametric estimators of the upper tail dependence coefficient (Serinaldi, 2008), the question of tail dependency is difficult to answer with the given data set (see also comments of reviewer 2). That is why we limited our study to rather high probabilities of exceedance (>= 0.01). We do not intend to make any statements or analysis of more extreme events. This was also the reason why we chose the Gaussian copula, see answers below.

Abberger, K.: A simple graphical method to explore tail- dependence in stock-return pairs, Appl. Financ. Econom., 15, 43–51, 2005.

Serinaldi, F.: Analysis of inter-gauge dependence by Kendall’s τK, upper tail dependence coefficient, and 2-copulas with applica- tion to rainfall fields, Stoch. Environ. Res. Risk. A, 22, 671–688, 2008.

3. Copulas P286L1: The sentence is impossible to understand for a nonmathematician and of no use for readers of NHESS. Please remain simple (see wikipedia for instance).

REPLY: The text was rewritten to improve the understandability.

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?

REPLY: We thank the Referee for this comment. Actually, we don't have the time series long enough to discuss the tail dependency (see comments above). Thus we based our selection primarily on the ability of the different copulas to describe the overall dependency structure best. The Gaussian copula performed slightly better than the Gumbel in this context (Table 3). However, as the Gumbel copula was only slightly inferior to the Gaussian and thus the results for the lower quantiles hardly differ, and as it also has the ability to consider tail dependencies, we will use the Gumbel copula in the revised manuscript, although we actually cannot make any statements about the tail dependency. As a side note, the T-copula would also be an appropriate choice, as it resembles the Gaussian copula for lower quantiles, but can also consider tail dependencies.

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.

REPLY: This will be clarified in the revised text. The AIC can be negative according the equ. 5, as the loglikelihood is positive. Yes, in this particular case, AIC does not differ from the use of standard loglikelihood because only one parameter copulas have been considered in this study. This will be mentioned.

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.

REPLY: We thank the Referee #1 for this comment and will consider this in future studies.

P288L7:fig6 is nice but does not help to really judge. "The Gaussian copula is selected" : what a pity according to the previous comments ...

REPLY: We will use the Gumbel copula in the revised version, see answer above.

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.

REPLY: We will largely extend this section in the revised version of the manuscript, see answer to general comments above. The significance of the trends has been proven by Delgado et al. (2010). We build on this study and underline/prove the significance of the trends. As mentioned above we will also discuss the usefulness, limits and uncertainty of the non-stationary approach compared to the stationary approach.

P288L25: 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.

REPLY: This will be added in the revised manuscript.

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.

REPLY: As mentioned earlier, the whole section will be changed and rewritten in a clearer and sound presentation. The “joint return period” concept will not be used in the revised version anymore. As we see that although that concept can be used even in the bivariate context (Salvadori et al. 2005,2010,2011,2013) but may not be suitable (or introducing unnecessary complexity) for the non-stationary case (Sivapalan 2009, Serinaldi, 2013). Instead, we will use the classic concept joint probability of exceedance for describing the scenarios that are critical for the system. Following reviewer 2 we will justify the use of the AND combined probability by characteristics of the Mekong Delta, rather than by statistical reasons.

Salvadori, G., Michele, C. D., Kottegoda, N. T., and Rosso, R.: Extremes in nature: An approach using copulas, Springer, Dordrecht, The Netherlands, 303-303 pp., 2005.

Salvadori, G., & De Michele, C.: Multivariate multiparameter extreme value models and return periods: A copula approach. Water Resources Research, 46(10), W10501. doi:10.1029/2009WR009040, 2010.

Salvadori, G., De Michele, C., & Durante, F.: On the return period and design in a multivariate framework. Hydrology and Earth System Sciences, 15(11), 3293–3305. doi:10.5194/hess-15-3293-2011, 2011.

Salvadori, G., Durante, F., & De Michele, C.: Multivariate return period calculation via survival functions. Water Resources Research, 49, 1-4, doi:10.1002/wrcr.20204, 2013.

Serinaldi, F. : Comments on "Multivariate return periods in hydrology: a critical and practical review focusing on synthetic design hydrograph estimation" by Vandenberghe, S., van den Berg, M. J., Gräler, B., Petroselli, A., Grimaldi, S., De Baets, B., and Verhoest, N. E. C., Hydrol. Earth Syst. Sci. Discuss., 2012

Sivapalan, M.: Transcending limitations of stationarity and the return period : process-based approach to flood estimation and risk assessment, 1675(January), 1671–1675. doi:10.1002/hyp, 2009.

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.

REPLY: The "inadequacy" of the Gaussian copula cannot be inferred from Figure 9. If at all, than adequacy of the different copulas can be inferred graphically from Figure 6 showing the observed flood events along with 20000 randomly drawn Q-V-pairs from the different copulas. Here, the Gaussian and the Gumbel copulas perform best and almost equally well from our point of view. Again, no statement can be made about the tail dependency from the time series data at hand, thus the Gaussian was selected. But as mentioned before, we will use the Gumbel copula in the revised version. Figure 9 will, however, not look different from the current figure, as the plotted quantiles hardly differ using either the Gaussian or the Gumbel copula.

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.

REPLY: As mentioned before, the notion of return periods will be dropped in the revised manuscript, joint probabilities of exceedance will be used instead.