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Hydrology and Earth System Sciences Discussions

Interactive comment on "Flood trends and variability in the Mekong river" *by* J. M. Delgado et al.

Anonymous Referee #3

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General

The authors present an interesting study of trends in annual maximum (AMAX) discharges in the Mekong river. These maxima are described by general extreme value (GEV) distributions in which the location and scale parameters vary over time. Much attention is given to statistical trend detection. The Mann Kendall (MK) test, ordinary least squares with resampling (OLS), and likelihood ratio tests, indicated as NSGEV, are compared in a Monte Carlo experiment. Corresponding to the trends in their AMAX data, the authors assume that the trend in the GEV location parameter is opposite to that in the scale parameter. This results in a useful addition to the Monte Carlo experiments of Zhang et al. (J. Climate, 17 (2004), 1945-1952).

I had some difficulties with the interpretation of the results. Some parts are difficult to follow for someone who is not familiar with rainfall in southeast Asia. Further, a number of statements needs to be reformulated. Moderate to major revision is required.

Major comments

1. There is a difference between the location parameter and the mean of the GEV distribution (see e.g., Zhang et al. (2004), page 1947). The authors do not clearly make this distinction (e.g., on page 6701, line 20). I wonder whether Figure 6 refers to the exceedances of the location parameter or the mean. Note that it follows from eq. (3) that the location parameter is exceeded with probability $1 – 1/e = 0.63$. The lines in Figure 6 suggest that a larger threshold than the location parameter has been considered. If it is really the mean, then it would be good to give the mathematical expression for the mean.

2. The fact that MK outperforms NSGEV in the case of a constant scale parameter (Page 6701, lines 19-21) needs more attention. It is a very curious result, and partly invalidates the assertion that NSGEV is also by far the best method if only the location parameter is considered (page 6703, line 19). NSGEV assumes that the data come from a GEV distribution and tests for a linear trend in the location parameter. Since the data are generated from such a model, one expects that NSGEV will do better than MK, which seems not to be the case in Figure 4 if the scale parameter is constant. Please check this result carefully. For instance, it is important that the number of detected trends corresponds with the nominal significance level if there is no trend in the location parameter. It may also be worthwhile to repeat the simulation with a somewhat larger number of repetitions.

3. Another curious point in Figure 4 is that the number of detected trends in the location parameter slightly increases if there is a small opposite trend in the scale parameter. Also in that case it would be good to check how the number of detected trends corresponds with the nominal significance level if there is no trend in the location parameter.

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In that case there is still a trend in the scale parameter, which may invalidate the use of the chi-square distribution of the likelihood ratio statistic.

4. In contrast to NSGEV, OLS and MK are primarily designed to test for a change in the mean. A positive trend in the scale parameter partly compensates the negative trend in the location parameter. The trend in the mean will thus be smaller than that in the location parameter. This partly explains the decrease of the power of OLS and MK in the case of an increasing positive trend in the scale parameter. Further, these tests (especially OLS) sometimes falsely detect a positive trend if relatively large values are found near the end of the series because of the positive trend in the scale parameter. The explanations given on page 6702, lines 6-16 and 22-29, and page 6703, lines 1- 4 are difficult to follow and could be omitted. The probability of generating a value greater than the location parameter is, for instance, always 0.63 (see comment 1). It does not depend on the value of the scale parameter!

5. Somewhat more information on the Indian and East Asian monsoon would be useful at the end of section 2 (page 6696). What is the rainfall season? Which one is the Southwest monsoon (page 6704, line 15), or is that another monsoon? The acronyms EAM and IM should already be introduced in section 2. At the end of the paper, there is some speculation about the influence of the North Pacific monsoon (pages 6707, 6708). Why is that monsoon not mentioned in Section 2?

6. The authors do not give the estimated GEV parameters for their AMAX data. In particular, it will be good to know the value of the shape parameter. For instance, the differences between the OLS and MK tests seem larger than in the work of Zhang et al. (2004), which may be due to a more skewed distribution in the present study.

7. The data in Figure 3 come from the same distribution because they were generated from the same model. The figure does not give any useful information and should therefore be omitted.

Minor comments

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Page 6692, line 12. Please change “incurred” into “resulted”.

Page 6692, line 23. Please change “initial absence of detected positive trends” into “absence of positive trends”.

Page 6693, line 20. It is not clear what is meant by “a first approach to variability in the flood regime”.

Page 6693, lines 27, 28. Sentence should be rephrased, e.g., The performance of trend detection tests in the presence of time-varying variability is investigated in a Monte Carlo experiment.

Page 6694, lines 19, 20. Model output and theoretical research also point to a future increase in flood events… It is unclear what kind of models are meant here and also what the authors mean by theoretical research here. The sentence can be omitted (it refers to grey literature).

Page 6695, line 27. Please remove “still”.

Page 6696, line 12. Please change “separate” into “distinguish”.

Page 6697, lines 1-3. In general, the mean of the deviations from the trend line is assumed to be zero. If these deviations come from a normal distribution, then the least squares estimates of the regression coefficients are optimal in terms of mean squared error. They are unbiased and other unbiased estimators will have larger variance. This is the essential point and not that eq. (1) represents a time series. The latter would also be the case if the deviations do not come from a normal distribution.

Page 6697, lines 8, 9. The definition of C suggests that the authors consider ties. However, in that case a more general expression for the test statistic than eq. (2) should be used, see e.g., Douglas et al. (J. Hydrology, 240(2000), 90-105).

Page 6697, line 15. Please change “general” into “generalized”.

Page 6699, lines 7, 8. Equation (9) gives twice the log-likelihood ratio rather than the

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log-likelihood ratio.

Page 6699, lines 10-12. The definition of the significance level differs from that in the statistical literature which is followed in the work by Zhang et al. (2004). One should in fact consider the integral from T to infinity.

Page 6699, lines 13-20. The authors want to show how the probability of exceeding a large threshold changes over time in the case of a statistically significant nonstationary model. This is not clear from the text. Moreover, an exceedance probability of a large threshold cannot be considered as a measure of variability (it depends also on the skewness). Perhaps the authors should remove this paragraph and modify the text in Section 4.3 slightly (page 6706, lines 7-9). Please avoid the term "reference values". The caption of Figure 5 is already quite clear.

Page 6699, lines 25, 26. It is not clear what is meant by "mean rank plotting position". Could it be replaced by "plotting position"?

Page 6700, line 9. Sentence should be rephrased, e.g., "The average variance over the time domain is also obtained by the wavelet".

Page 6700, line 25. "variance" should read "standard deviation".

Page 6702, lines 19,20. It is not correct to say that a model with two covariates has been considered in the case of a simultaneous test on linear trends in the two GEV parameters. Also in this case the year is the only covariate.

Page 6703, line 4. "occurence" should read "occurrence".

Page 6703, line 14. Please change "incurred" into "resulted".

Page 6703, lines 24-30. Local likelihood techniques could also be considered to describe the temporal variation of the scale parameter. See e.g., Ramesh and Davison (J. Hydrology, 256(2002), 106-119) and Butler et al. (Applied Statistics, 56(2007), 395-414).

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Page 6704, line 8. Please change “conducts to higher incidence of type II error” into “leads to a relatively large type II error”.

Page 6704, line 18-20. Sentence should be reformulated, e.g., “Indeed, the trends in the greater flood magnitudes at Thakhek and Pakse become ascendant if the AMAX are modeled by NSGEV with a linear trend in both parameters”.

Page 6704, lines 25,26. It is unclear how an increase in extremely high floods matches the projections from regional and global climate models. This needs some discussion with references.

Page 6705, lines 26, 27. Please omit “of the linear case (two parameters are added when going from GEV to linear NSGEV and from linear NSGEV to quadratic NSGEV)”.

Figure 5. One would expect that the average exceedance probability of the 20-year flood would be 0.05. For Thakhek the average seems to be much smaller than 0.05.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 6691, 2009.