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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Time-varying copula and design life level-based nonstationary risk analysis of extreme rainfall events" *by* Pengcheng Xu et al.

Anonymous Referee #1

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The Authors present a methodology to estimate a methodology for a design life level-based risk analysis that takes into account uni- e bi-variate non-stationarity. The methodology presented is organized in 3 steps:

- The presence of trends and changing points for uni- e bi-variate datasets is tested using Mann-Kendall and Pettitt tests and their statistical significance assessed. - Independently to the stationarity test's results "According to Porporato and Ridolfi (1998), an insignificant trend should not be ignored because of its effect on the results of hydrological risk analysis. Hence, even if precipitation extremes at a certain station may recommend statistically weak trends, both the nonstationary and stationary models are established for each station in the following section", both stationary and nonstationary distribution functions are used to describe uni- e bi-variate data. - Design life level-

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based risk analysis is applied to bi-variate data through Kendall's joint return period and AND's joint return period. By doing so the Authors the values to be assigned to the hydrological variables to design the infrastructures including bivariate nonstationarity.

The hydrological univariate variables considered are Ps (annual cumulated precipitation above a threshold) and Im (annual maximum daily precipitation) extracted from daily precipitation time series of six meteorological stations located in the Haihe River basin (China).

For univariate variables the GEV distribution is assumed a priori and one stationary and two non-stationary (1: location parameter is time-varying; 2: both location and scale parameters are time-varying) parameters sets are evaluated for each variable. Once the best GEV model (i.e stationary or not) to describe each variable was identified the bi-variate analysis was performed.

For bivariate analysis four Copulas are considered: Student's t, Clayton, Gumbel and Frank with stationary and time-varying parameter. The best copula model is chosen according to AICc criterion results and the nonstationary "Kendall" and "AND" joint return periods computed.

The comments in the manuscript are coherent with the Figures; unfortunately, I was not able to reproduce Figure 4 using Table 3 values (see point 5 in General comments). For this reason, I would like to suggest the Authors to revise Table 3 and/or Figure 4 and the text related in the manuscript. Not being able to reproduce the results of Figure 4 I do not try to reproduce the other results in the manuscript I accept the Authors's analysis but I assume that it is based on data different from those presented in the manuscript.

The research presented is of potential interest for the readers of HESS but there some points need to be revised by the Authors. I suggest to accept the manuscript with major review.

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General comments

There are some points that I would like the Authors to address in the manuscript

1) For the design of which structure are the variables Im and Ps significant? 2) In the Introduction climate change is indicated as one of the motivation to propose a nonstationary design life level-based risk analysis, but there is any attempt to project the results of the manuscript in the future. Shall the Authors provide some indication of what shall we expect in the future? Do the Authors compare their projection of hydrological extremes with the trends that can be derived from climate models?

3) Why do the Authors limits their analysis to six rain-gauges when there are several more in the area (e.g. https://doi.org/10.1002/hyp.9607)?

4) Which are the limits/problems relate to use an upper bounded distribution (i.e. GEV when κ<0) to describe variables that potentially range between [0 $+\infty$).

5) There is a significant difference between quantiles reported in Figure 4 and those computed using equation 2 and parameters reported in bold in Tables 3(a) and 3(b) with t=1960, 1970, 1980, 1990, 2000, 2010, 2020. (see Fig1)

Here I show my attempt to reproduce the panels of Figure 4 using parameters as in the Table below vs screenshots of Figure 4 from the manuscript. Differences are quite evident, I see a similarity only for Im at station 4, for all the other cases it was impossible to reproduce the fan of non stationary GEVs at the different years. I would appreciate if the Authors could spend some of their time to investigate the reasons their and my outputs are so different. (see Fig2-Fig4)

6) In the presence of a statistically significant change point, is it correct to use the same parameter formulation to describe data "before" and "after" the abrupt change point? Moreover, which would be the results of trend analysis if the time series are split as "before" and "after" the change point. Do the Mann-Kendall test' results change

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considering the "before" and "after" segment of the time series separately?

7) As last I would like to suggest the Author to add: a. A section on climate change projection and analysis that can be of interest for future infrastructure design b. one table reporting the basic statistics (min/max/mean/standard deviation) of the Ps and Im variables and the values of the 95-th percentile threshold to help understanding the variability of datasets; c. one figure showing the timeseries with the indication of the change point year of occurrence according to Pettitt test.

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Specific comments

Line 116 The definition given of Ps variable recall me the index R95pTOT used in climate change studies (http://etccdi.pacificclimate.org/list_27_indices.shtml). Is it the same index? In addition, could the Authors specificy the period of observation they used to set the 95-th percentile threshold? According to R95pTOT index the reference period to set the 95-th percentile threshold is 1961-1990.

Lines 290-291 The Authors write "where R_iˆns and R_iˆs are nonstationary risk and stationary risk of a certain hydraulic structure for a design life of i years", but 'i' goes from 1 to n. I would expect that 'n' indicates the design life and 'i' indicates the i-th year from now (i.e. the year the project "starts") to n-th year (end of the project's life).

Lines 367-368 The Authors write "Except for stations 4 and 5, the best distributions for the other stations were parallel for nonstationarity tests shown in Section 4.1". Is it possible that the mismatch between the nonstationarity test results and the best fitting distribution for Im (station 4 and 5) and Ps (station 5) was to the choice of the Author to ignore the test's results?

Lines 387-390 The Authors write "Contrary to station 5, the nonstationary St copula fitted better than did the stationary model for stations 1 and 6 which was not in accordance with the nonstationarity tests for these two stations (Table 2)." It is true that according to bivariate MK test results station 1 and 6 should stationary, but at station 1 bivariate Pettitt test shows the presence of a change point; the presence of a change point could have influenced the results of LL and AICc ? What will happen if Im and Ps time series are "broken" before and after the change point to LL and AICc estimates?

Line 411 (and Conclusions) The Authors report a value of 355 mm for the 100-year Ps quantile in station 1 under stationary circumstances, but using the parameters reported in Table 3(a) the 100-year Ps quantile in station 1 under stationary circumstances is about 383 mm. It is probably a matter of approximation in the parameters values (355 mm corresponds to a report period of about 62 yr) but I will suggest the Authors to check these values.

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Minor corrections

Around the manuscript there are some typos like "Pettist" instead of "Pettitt"; missing spaces and so on (e.g Lines 226, 228), please check the text.

Lines 172 and 173 Is the limit "(μ-σ)/κ" for lower (upper) boundary of x value correct? According to parameter's estimates in Tables 3(a)-3(b), when κ<0, x can assume only negative values, that is non coherent with the variables Ps and Im that are positively defined.

Lines 249-250 The Authors write "Let JRPs-and and JPRs-ken represent the three types of return period in the stationary case", but the return periods presented are only 2.

Line 260 "JPRs" probably was "JRPs"

Line 343 and Line 426 check the correct location of Figure 3.

Lines 359-365 The Authors write "The best fitted model was selected by performing the minimum DIC criterion combined with the Bayes factor (BF) test", but looking at bold rows in tables 3(a) and 3(b) the criterion of minimum DIC seems not be respected for Im at station 2 where GEVns-1 is in bold instead of GEVns-2 (minimum DIC value).

Line 360-365 Comparing these lines with Table 3(a), for station 1, the variable described as GEVns-2 appears to be Ps and not Im. BF for Im variable in station 1 is >1. Please clarify this point.

Line 387 Please define "MK"

Lines 409-411 Figure 3 illustrates the results of nonstationary tests. Figure 4 reports the extreme rainfall quantiles. Please check the text.

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References

Line 62 Does "Assia et al., 2014" refer to "Aissia, M.A.B., Chebana, F., Ouarda, T.B.M.J., Roy, L., Bruneau, P., and Barbet, M.: Dependence evolution of hydrological characteristics, applied to floods in a climatechange context in Quebec, J. Hydrol., 519, 148–163, https://doi.org/10.1016/j.jhydrol.2014.06.042, 2014" ?

Line 98 Does "(Jakob, 2013)" refer to "Jakob, D., AghaKouchak, A. Easterling, D., Hsu, K., Schubert, S., and Sorooshian, S. (Eds.): Nonstationarity in extremes and engineering design, Springer, New York, 2013." ?

Line 100 "Read and Vogel (2015)" there is no correspondence in the references

Line 126 Does "Nelson (2007)" refer to "Nelsen, R.B.: An introduction to copulas, Springer, New York, 2007."?

Line 212 "Genest et al., 1995" there is no correspondence in the references

Line 213 "Hurvich and Tsai, 1989" there is no correspondence in the references

Line 235 "Fernandez and Salas, 1999" there is no correspondence in the references

"Ghanbari, M., M. Arabi, J. Obeysekera, and Sweet, W.: A coherent statistical model for coastal flood frequency analysis under nonstationary sea level conditions, Earth's Future, 7, 162-177, https://doi.org/10.1029/2018EF001089, 2017." The publication year is 2019

"Zhang, Q. , Gu, X. , Singh, V. P. , and Chen, X.: Evaluation of ecological instream flow using multiple ecological indicators with consideration of hydrological alterations, J. Hydrol., 529, 711-722, https://doi.org/10.1016/j.jhydrol.2015.08.066, 2015." should be moved at the end of the reference list

##

Table (1) I suggest the Authors to change Longitude and Latitude with Longitude E and Latitude N, respectively coherently with the choice of indicating geographical coordinates in degree/minutes format.

Table (2) "Ps" and "Im" should be in italic. For station 3 and multivariate MK test the "*" should be close to the Z-statistic value not to the p-value. I suggest the Authors to add the indication of the year at which the change point is detected for both univariate and bivariate Pettitt test.

Table 3(a) e 3(b) please specify the meaning of bold row, I guess that bold indicates the "best" fitting model, but in this case why for Im variable at station 2 the best model is GEVns-1 if GEVns-2 shows the minimum DIC?

Table 3(b) refers to (Station 4-6) not to (Station 2-6) and the '-' symbol is missing for BF values of stationary GEV in station 5

Table 4(a) and 4(b) reports the meaning of bold and underlined text. Infinity symbol cited in caption does not appear in the table, probably substituted by "NaN".

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Figure (1) Step S2 is omitted.

Figure (2) I would like to suggest the Authors to add the Haine river to the map.

Figure (3) In the caption there is a typo “Mann-Kendalld” instead of “Mann-Kendall”. Please check the legend, the description of the last item (purple backward arrow) is equal to the one of the third one (green upward arrow). The “+” symbol is redundant with the test that already specify if the trend/change point is statistically significant.

Figure (4) the “star” symbol is not defined.

Figures (4), (6), (7) I would like to suggest the Authors to improve the quality of these figures. They seems to be a collection of screenshots with different size and background colour. Figure 4, in particular, seems to lack of organization in the sub-figures arrangement.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-358/hess-2019-358-RC1-supplement.zip

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-358, 2019.

Station	Attribute	Model	μ	σ	κ
1	Ps	GEVs	144.37	45.32	0.058
		GEVns-2	151.85-0.00042t	exp(3.92-0.000038t)	0.019
	Im	GEVs	50.64	18.13	0.012
		GEVs	50.64	18.13	0.012
2	Ps	GEVs	106.25	28.84	-0.12
		GEVns-2	99.92+0.0028t	exp(3.25+0.000057t)	-0.13
	Im	GEVs	40.71	14.01	-0.053
		GEVns-1	42.02+0.00026t	14.30	-0.099
3	Ps	GEVs	86.92	26.92	-0.077
		GEVns-2	85.78+0.0014t	exp(3.57-0.00014t)	-0.091
	Im	GEVs	34.31	11.00	0.13
		GEVns-1	37.51-0.00067t	11.38	0.10
4	Ps	GEVs	141.69	55.87	0.19
		GEVs	141.69	55.87	0.19
	Im	GEVs	56.36	22.29	0.34
		GEVns-1	65.73-0.0035t	23.17	0.31
5	Ps	GEVs	160.34	56.07	-0.21
		GEVns-2	150.25+0.0011t	exp(4.07-0.000038t)	-0.19
	Im	GEVs	67.46	26.37	0.072
		GEVns-1	77.72-0.0041t	26.88	0.047
6	Ps	GEVs	137.56	57.44	-0.24
		GEVs	137.56	57.44	-0.24
	Im	GEVs	63.02	29.43	0.033
		GEVns-2	57.05+0.00083t	exp(3.45-0.000071t)	0.072

Fig. 1.

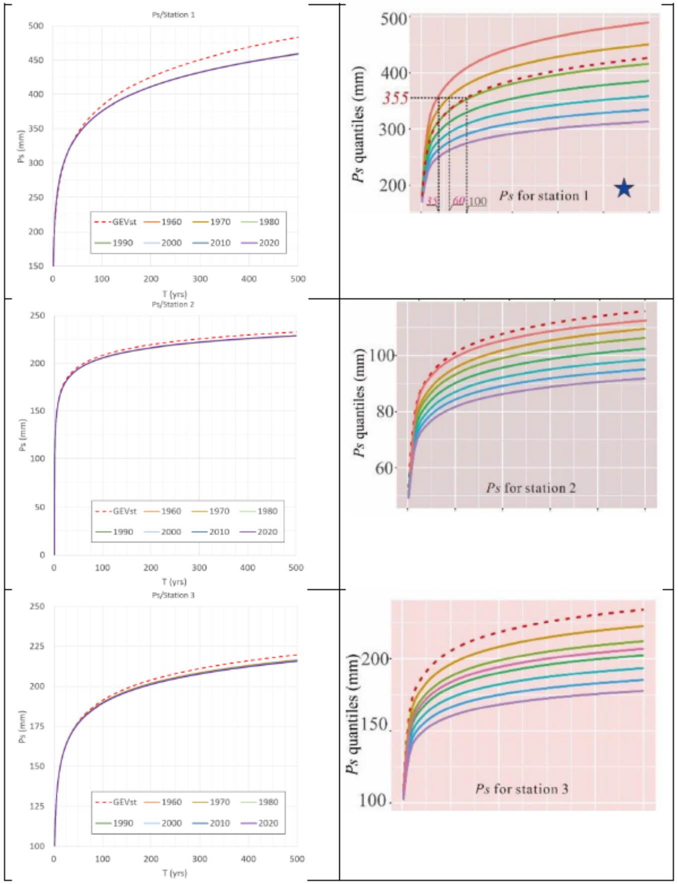

Fig. 2.

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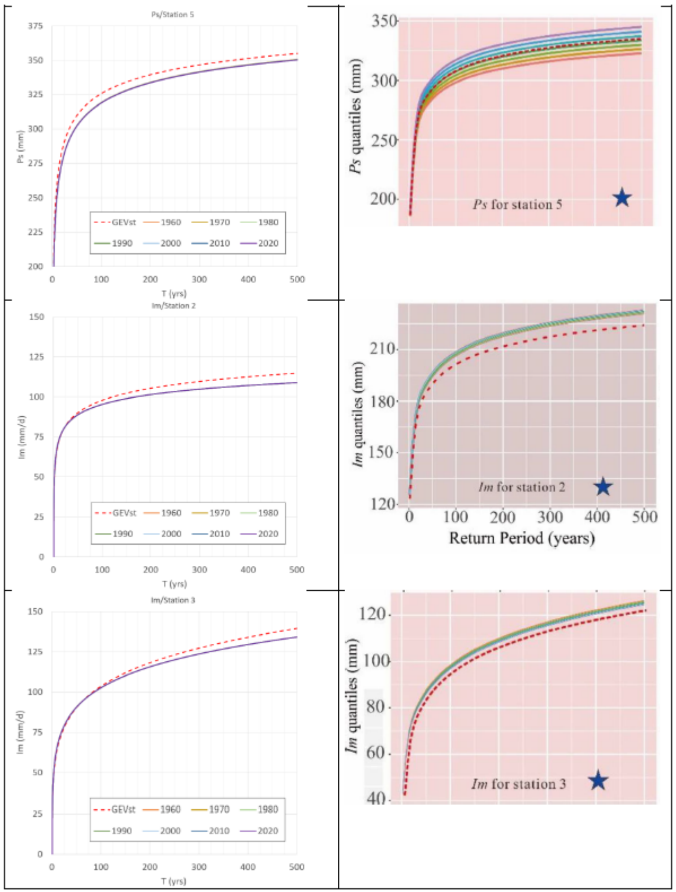

Fig. 3.

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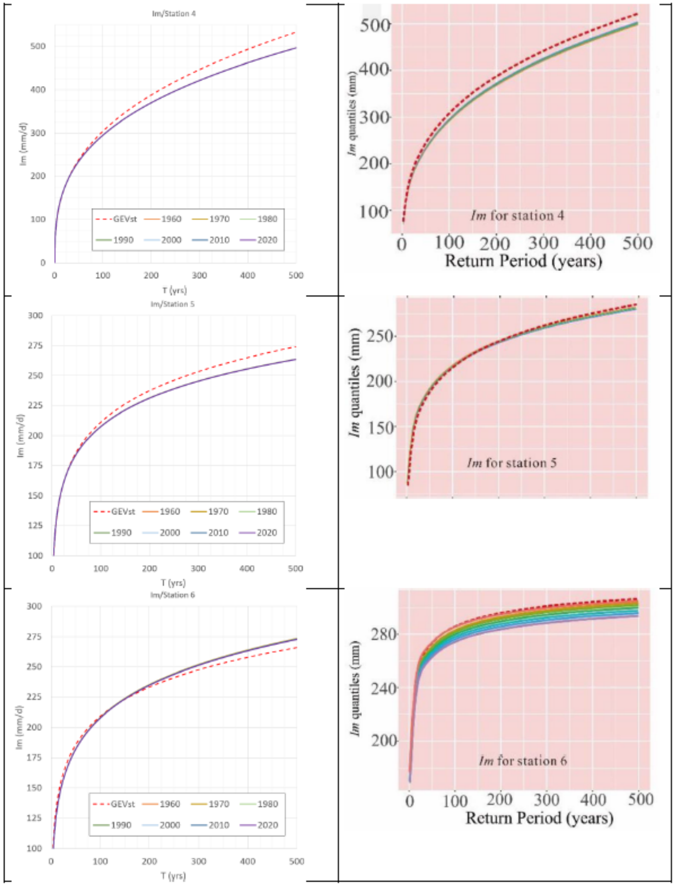

Fig. 4.

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