Author's response to Editor's decision and comments from Reviewers

The most extreme rainfall erosivity event ever recorded in China:
The "7.20" storm in Henan province

Yuanyuan Xiao, Shuiqing Yin, Bofu Yu, Conghui Fan, Wenting Wang, Yun Xie

We would like to thank the Editor and Reviewers for all their comments and suggestions. In the revised version, we improved the text and figures, addressing all the issues raised by the reviewers. We hope this revision is satisfactory for the further processing of this paper.

Dear Editor,

We would like to thank you very much for your feedback. We revised the manuscript in detail, aiming to answer all reviewers' points. We would like to submit the revised version of our paper "The most extreme rainfall erosivity event ever recorded in China: The "7.20" storm in Henan province" by Yuanyuan Xiao, Shuiqing Yin, Bofu Yu, Conghui Fan, Wenting Wang and Yun Xie.

We believe that we followed all the proposed corrections by the reviewers that substantially improved the article and we are looking forward to hearing from you with respect to the review process.

We provide answers to comments from Reviewers below. For clarity, each answer is structured as follows: (1) RC# comments from Referees (black), (2) Author's response (blue).

Thank you for your time and consideration.

Sincerely,

Yuanyuan Xiao

RC1: 'Comment on hess-2022-351', Anonymous Referee #1, REPLY

General comments

In the submitted paper authors investigate the characteristics of one extreme rainfall event (20th July, last year) in comparison to spatial and temporal rainfall erosivity characteristics in China. The paper is interesting and within the scope of the HESS journal. However, there are several points that could be improved.

Author response: Thank you for your careful review and valuable suggestions. Here we present our responses in blue.

Firstly, the reported rainfall amounts are relatively extreme. Hence, more details about the measuring equipment used to measure rainfall (and accuracy of these instruments) should be reported since this could have an effect on the measured rainfall amounts (can at least for this extreme event uncertainty be estimated).

Author response: We agree that the accuracy of the instruments is important. Observed hourly rainfall data from 1951 to 2021 for 2420 meteorological stations in China were collected with siphon rain gauges (before about 2000~2005 varying across stations) and tipping bucket rain gauges since. The data had been quality-controlled and the homogeneity of the data had been checked by National Meteorological Information of Center of China Meteorological Administration. The details of the data will be added in the paper.

Secondly, the reported results are sensitive to the selected empirical equation used to calculate the energy. Hence, are there any other data available (e.g., optical disdrometer) measurements that could be used to validate these calculations in order to make less uncertain rainfall erosivity estimates?

Author response: As there was no direct measurements of kinetic energy, kinetic energy as a function of rainfall intensity, known as KE-I equation was used to estimate storm energy as recommended for RUSLE2, which is widely used as an empirical model.

Thirdly, the reported frequency analysis is missing uncertainty estimation (confidence intervals).

Author response: Uncertainty analysis is important for such a rare rainfall event. We carefully compare the simulation effects and confidence intervals of several extreme value distributions including Pearson-III, Log-Pearson-III, GEV and Gumbel, and we found Log-Pearson-III generated the most reasonable estimate. The logarithm of the observations was first taken before Pearson-III was applied, and an anti-logarithm transformation was performed thereafter. Based on Log-Pearson-III, the estimated return period of the “7.20” storm in terms of its rainfall erosivity is about 10,072 years. Based on the 95% confidence interval for the log-Pearson-III distribution, the estimated return period is most likely to be at least 516 years (Figure 1). Therefore, Log-Pearson-III will be used to replace GEV and its uncertainty associated with the estimated return period will be added in the revised version.

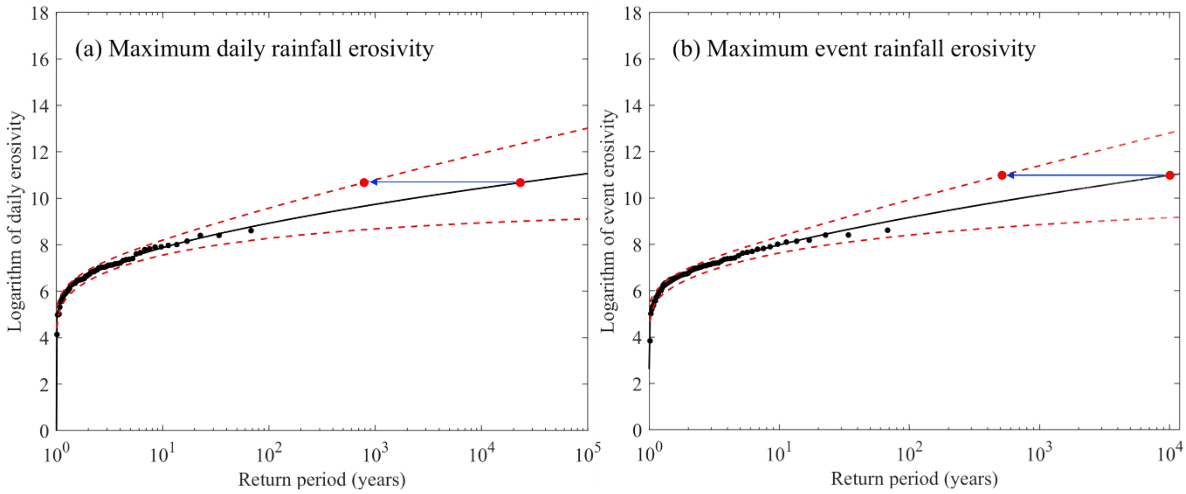

Figure 1. The logarithm of observed daily (a) and event (b) rainfall erosivity as a function of the empirically fitted return period for Zhengzhou meteorological station (black solid circles are observations from the period 1951-2020, the red solid circles indicate the "7.20" storm in 2021, the red dotted line is the upper and lower limit of 95% confidence interval, and the solid lines in black represent the fitted P-III distribution using the logarithm of observations from 1951-2020)

Fourthly, I am not sure what is the purpose of envelope curves (see also specific comment below). I would suggest to add some specific details about the impact of this extreme event on soil erosion (some measurements perhaps, if available) or at least on the sediment concentrations in rivers (some measurements) or something similar. Hence, could you say that extreme erosive events (with return period over 100,000 years) also leads to soil erosion rates with similar recurrence interval (the same for sediment transport rates).

Author response**:** Envelope curves were drawn to show the maximum event rainfall erosivity at different latitudes. In previous studies, the average rainfall and rainfall erosivity in China decreased from southeast to northwest. In the south of China, the frequency of rainstorm is higher than that in the north, and the likelihood of experiencing extreme rainfall erosivity in the south is also the highest, but this is no longer the case based on our research. The "7.20" storm indicates that extreme rainfall erosivity could also occur in mid-latitude in China. That is why these envelope curves were drawn. Since soil erosion and sediment concentrations in rivers are determined not only by rainfall but also by topography, soil erodibility, vegetation cover and so on, we don't expect a similar recurrence interval for the amount of soil erosion. However, we believe that this is a good suggestion to add some specific details about the impact of extreme rainstorm on soil erosion. Unfortunately, due to difficulties in obtaining data, we are unable to prove this conjecture.

Finally, some specific comments are provided below.

Specific comments

Figure 1: Maybe you could more clearly indicate Henan province in this figure.

Author response: We have marked the location of provinces in Figure 1.

Equation 1: You should cite the original source of this equation. Additionally, what is the sensitivity of results with respect to the selection of equations (1) and (4).

Author response: We have provided the original references of the two equations in the paper. The sensitivity of the empirical equation selected for calculating rainfall erosivity has been explained in the General comments.

Lines 132-135: Please provide more details about interpolation method used

Author response: We used the Inverse Distance Weighted (IDW) to interpolate the point data to the region. We will explain this in detail in the revision.

Equation 5: Please provide the original reference.

Author response: We have provided the original reference of equation 5 in the paper.

Line 147: Shape, scale and location parameters and not position parameter.

Author response: We have changed 'position,' to 'location' in line 147.

Equations (6)-(7): Please double check it, I am not sure if these are correctly written.

Author response: We have decided to change the method of frequency analysis. We will carefully check the accuracy of the formula.

Equations (8)-(17): I am not sure if these need to be reported in a paper about rainfall erosivity. More details about the rainfall erosivity calculation procedure and measurements could be provided instead.

Author response: After the paper is revised, if necessary, we can include this section as supplementary material.

Line 186: "It showed". Is this referring to Want et al. (2016) study?

Author response: Yes, "It showed" refers to the research of Wang et al. (2016) study. We will revise it to make readers understand it better.

Figure 3: It is not clear how was Figure 3a created, is this station-based data interpolated or this is from other source (radar)?

Author response: Figure 3 is obtained by IDW interpolation based on observation data, which will be explained in detail in the paper.

Figure 4: The same as for Figure 3.

Author response: Figure 4 is obtained by IDW interpolation based on observation data, which will be explained in detail in the paper.

Figure 5d: Here these results are probably quite sensitive to the selection of the empirical equations used to calculate the rainfall erosivity. It would be nice to elaborate a bit about this issue.

Author response: This problem has been explained in the general comments. More explanation will be added in the revised version.

Table 1: Why ha? I suggest to use either km2 or 1000*km2 or something similar. Also in this table you are comparing areal rainfall erosivity with station-based (gauge, probably 200 cm2 or something similar).

Author response: Yes, we are comparing areal kinetic energy with station-based. In section 2.2.2, the unit of kinetic energy is $MJ{\cdot}ha^{-1}$, the standard unit of measurement used for the USLE/RUSLE, while the unit of rainfall erosivity is $MJ{\cdot}mm{\cdot}ha^{-1}{\cdot}h^{-1}$, so we also use ha for the area unit in Table 1. The unit of 'ha' has been widely and conventionally used in agriculture.

Figure 6: Here you clearly need the conference intervals. I am not sure if you could just say that the return period of this event is exactly 154,154 years. Additionally. You should note that in (flood) frequency analysis there are usually some specific rules about the longest return period that could be estimated based on specific data length (sample size). Different rules can be found in the literature. At least some discussion about this should be added.

Author response: Obviously, we cannot say that the return period of this event is exactly 154,154 years. It is quite uncertain to use about 70 years of data to estimate the return period in excess of 100,000 years. We have used the 95% confidence interval to assess the uncertainty.

Section 3.2.2: I am sorry but I do not completely understand the purpose of defining these envelope curves? How could these be used? It is clear that the shape of the "curve" is defined by the extreme events (as authors also indicate in the last sentences of this paragraph) that are a result of stochastic process.

Author response: Please refer to the reply in the general comment.

RC2: 'Comment on hess-2022-351', Anonymous Referee #2, REPLY

General comments

The authors studied and characterize a record extreme rainfall observed in China, in July 2020 in terms of its erosivity. The authors did a great job regarding the conciseness of the paper, and it is within the scope of the HESS journal. I have some concerns that need to be addressed before the paper is considered for publication, mainly regarding the frequency analysis.

Specific comments:

There are many instances of grammatical and incomplete sentences (some examples are given below). I, therefore, recommend a full language review.

The authors used GEV for the frequency analysis, but a lot of information is missing. For instance, the estimated parameters (most importantly the shape parameter) are presented and the uncertainty i.e confidence intervals are missing in the plots (Figure 6). The plot is also so condensed below the 100-year level to make any comments regarding the quality of the fit. The return period of the largest value is given as a point value, at least the lower and upper bounds should be given knowing that a lot of uncertainty is expected given that only around 70 years of data is used to infer a 100,000-year return period.

Author response: Thank you for your careful review and valuable suggestions. According to your suggestions, we carefully compare the simulation effects and confidence intervals of several extreme value distributions including Pearson-III, Log-Pearson-III, GEV and Gumbel, and we found that Log-Pearson-III generated the most reasonable estimate. Therefore, Log-Pearson-III is used to replace GEV and its uncertainty estimate will be added in the revised version.

Technical corrections

L 101-102 : "Hourly rainfall data from 1951 to 2020 were as historical data" > the sentence is not complete

Author response: The sentence is revised to read 'Hourly rainfall data from 1951 to 2020 were used to calculate the annual maximum daily and event rainfall erosivity.'

The caption of figure 2 seems too short

Author response: Figure 2 caption is changed to "Workflow of this study".

Figure 6: The caption : "Observed daily (a) and event (b) rainfall erosivity as a function of the empirical and return period …." > this seems incomplete. Empirical what??

Author response: The caption of Figure is revised to read 'Figure 6. The logarithm of observed daily (a) and event (b) rainfall erosivity as a function of the empirically fitted return period for Zhengzhou meteorological station'.

The relevance of Figures 7 and 8 should be made more clear, also different colors could be used to distinguish the two curves

Author response: We have used two different colors to distinguish the envelope curves.

L 265: "Post the "7.20" rainstorm, the ….. " > This is not clear

Author response: We have changed 'Post the "7.20" rainstorm,' to 'Because of the "7.20" storm,' in Line 265.

RC3: 'Comment on hess-2022-351', Anonymous Referee #3, REPLY

General comments

This paper aims to characterize the heaviest recorded rainfall in Henan, China, in recent times. Towards this, the available rainfall data is well presented and compared with historical data of the past 70 years from meteorological stations all over Mainland China. However, in terms of stated objectives, specifically 'characterizing spatial and temporal distribution', the paper seems to fall short.

Author response: Thank you for your careful review and valuable suggestion. We included the characteristics of the storm and its temporal and spatial distribution as background information. This is not one of the research objectives, and we have removed this from the list of objectives in the revised manuscript.

In the frequency of occurrence analysis (section 3.2.1), the GEV distribution is fitted with '7.20' rainstorm. This being so rare (which is stated as an outlier in line 235) could be biasing the curve as far as the previous occurrences are concerned. To complement this analysis, it would be relevant to assess GEV parameters without accounting the '7.20' event and quantify the difference when the extreme is considered. Further, the uncertainty associated with GEV parameters could also be illustrated.

Author response: We agree with you on this. In the paper, we used observed largest events each year other than the "7.20" storm for curve fitting (e.g. black solid circles in Figure 6). Based on the fitted curves, we can estimate the return period (e.g., red solid circles in Figure 6) of the "7.20" storm. The uncertainty associated with this estimate will be added in the revised version.

Since the frequency analysis is only performed at Zhengzhou meteorological station, the result cannot be completely said to be characterizing spatial distribution. Maybe, it will be worth exploring similar behaviour at other stations available. A map indicating the return period of the ‘7.20’ storm for remaining stations at Henan province could be helpful in this regard as well.

Author response: We agree that it would be useful to show the extreme nature the "7.20" storm using data from Zhengzhou Meteorological Station alone. We will include a map of the return period of the "7.20" storm for all stations in Henan Province.

Here are some of the other concerns and suggestions for improving the paper. Though the paper is relevant in the application of existing techniques to an extreme event, it could benefit from clear definition of purpose as well as innovativeness in analysis. Addressing these, acceptance of the paper is recommended after a major revision

Detailed comments

（1）Introduction

- The importance of establishing rainfall erosivity values is rightfully stated. However, it is also dependent on how the rainfall kinetic energy is obtained. Since the paper is not using directly measured energy but rather a derived value from rainfall rate (eq.1, RUSLE2), I would suggest mentioning probable biases expected in this approach. It would be worth stating that drop size distribution (dsd), as well as drop velocity, are following certain assumptions in this approach rather than being directly measured.

Author response: We agree that there is uncertainty in the estimated kinetic energy from rainfall intensity. We have explained this in the revised version.

- line 75: ‘The strengthen and ‘– was this supposed to be ‘strengthened’

Author response: We have changed ‘strengthen’ to ‘strengthened’ in line 75.

(2.1) Data source and pre-processing

- If the historical data is available to the general public, I would suggest putting a link to that information. More information on nature of data is also desired here

Author response: We have added more detailed information on the nature of precipitation data. The link to access the data will be added.

- Similar comment on rest of the data; please insert information on devices used, quality of data as well as resolution available

Author response: We have added data sources, observation equipment, quality control measures and other information in the revised version.

- Though later mentioned in section 2.2.3, I would suggest including the number of years discarded here as well, as it is defined at this section. If it could be clarified that missing years didn’t have extreme events (to the level that it could bias current analysis) that would be good too.

Author response: We agree that the treatment of missing years is very important for the extreme value analysis. We have checked this using daily precipitation observation from rain gauges and provide clear explanations in the revised version.

- line 100: ‘Hourly rainfall data from 1951 to 2020 ….’ Please reframe this sentence.

Author response: The sentence has been revised to read ‘Hourly rainfall data from 1951 to 2020 were used to calculate the annual maximum daily and event rainfall erosivity.’

- Figure 1: seems like the scale should be outside the inset area since it corresponds to the mainland than nine-dash line map.

Author response: We have revised the scale in Figure 1.

(2.2) Framework of the study

- There are some minor continuation issues in the usage of the terms ‘maximum event rainfall amount’ and ‘maximum daily rainfall amount’. For example, in line 115 they are ‘maximum event rainfall’ and ‘maximum daily rainfall’ while in figure 2 they are ‘maximum event amount’ and ‘maximum daily amount’. The former is again used in figure 5. Though minor, it’s better to follow single usage for readability.

Author response: The terms “maximum event rainfall” and “maximum daily rainfall” follow a single usage. We have revised the terms throughout the manuscript as follows: “maximum event rainfall” and “maximum daily rainfall”.

- Figure 2: misplaced ‘and’ in the top row ‘Hourly and rainfall data..’

Author response: We have deleted "and" in "hourly and rainfall data" in Figure 2.

- line 125: er unit is repeated (make sure to keep the format used throughout the paper while removing the extra entry of unit)

Author response: We have maked sure to keep the same format used throughout the paper while removing the extra entry of unit.

- line 130: ‘Henan province’ seems to be repeated in the text as it is already covered in the study area

Author response: Our study area covers Henan Province, but considering that the "7.20" storm mainly occurs in Henan Province, we have compared the rainfall and kinetic energy of the whole study area with Henan Province.

- line 150: eq.8 is using ‘E’ for expected values. As ‘E’ is already used as total energy, another variable would be better

Author response: We have revised the manuscript accordingly.

- line 160: This is a follow-up comment to the one mentioned in the introduction part. The usage of various moment estimators is known to have an effect according to the type of dsd (and hence to energy and erosion), thanks to the measuring instrument. If it is relevant in this data used, think about including a line at the introduction or here.

Author response: We have explained this in the revised version.

- line 165: eq.17 is placed after its description

Author response: We have adjusted the position of equation 17 and its description in line 165.

(3.1) Temporal and spatial characterization of the “7.20” rainstorm

- Since “7.20” storm is defined at the start of this chapter, it would be better to follow that terminology for the rest of the paper for better readability. The dates were found to be repeating in line 180, line 190, Fig. 3, etc. A general overview of the extreme event (exact duration and maybe the range of accumulated rainfall between relevant stations) would do more justice to the third sentence in 3.1.1.

Author response: First, we have used the "7.20" storm throughout the paper. Second, we will uniformly define the duration and cumulative range of extreme event to avoid excessive repetition in the section.

- Could you provide more information on the definition on of ‘early, middle, and late’ period in line 185? Maybe, mention the criteria followed in Wang. et. al.

Author response: We have added the criteria adopted by Wang et. al. (2016) in line 185. In that study, storms were classified into four patterns: advanced, intermediate, delayed, and uniform depending when rainfall is most concentrated. The dimensionless durations were separated into three equal time periods. Advanced pattern, intermediate pattern and delayed pattern when more than 40% of the rainfall occurs in the first, second and third time periods, respectively. The rainfall distribution is regarded as uniform pattern otherwise. The detailed information on this has been added to the manuscript.

- line 210: ‘new hourly rainfall intensity record’ – could you provide the intensity value (mmh-1) in brackets if available? If rainfall amount (depth in mm) was referred to here, rephrasing so would be more accurate.

Author response: We have inserted brackets after the ‘new hourly rainfall intensity record’ and fill in "201.9 $mm\cdot h^{-1}$" in the brackets.

- line 225: the last sentence feels misplaced here as no information was discussed on the vegetation of the region.

Author response: We have deleted this sentence.

- A minor suggestion: could you synthesize the results from sub-sections into one at the end of section 3.1. Since erosivity is estimated from energy, it will be worthwhile to make some general comments from the separate observations.

Author response: We have synthesized the results from sub-sections into one at the end of section 3.1 for better readability.

(3.2) How extreme is the event recorded at Zhengzhou meteorological station?

- 3.2.1: preposition missing in the title - page 11: line 254

Author response: We have changed ‘Frequency of occurrence the maximum daily and event rainfall erosivity’ to ‘Frequency of occurrence of the maximum daily and event rainfall erosivity’.

- ‘43354’ to ’43,354’, line 265: ‘58874’ to ’58,874’

Author response: We have changed ‘43354’ in line 264 to ’43,354’, and ‘58874’ in line 266 to ’58,874’, as well as other places to make the format consistent.

- Since 7.20 is suggested to be regarded as an outlier in frequency analysis (line 235), should there be additional justifications for using this for pushing envelope curves further?

Author response: This is a good point. Our frequency analysis suggests that the return period of the 7.20 storm is highly likely to be at least > 500 years, and could be as large as around 10,000 years. The envelope curve was pushed upward around the mid latitude in China because of the unusual extreme event.

- refer the points mentioned in general comments for this section

Author response: Please refer to the response on the general comments.