

Reply to reviewer's comments (Aloïs Tilloy/RC2) regarding MS "About the return period of a catastrophe" - nhess-2021-86

Dear Dr Aloïs Tilloy,

Thank you very much for all your effort regarding my manuscript (MS) and the helpful comments, notes and advice. I will consider some of these directly in a revision. However, I will also reject some of your concerns by arguments. Before, I would like to give following general reflections.

I aware that my English is poor and awkward. Therefore, I used already the help (proofreading) by two colleagues. The outcome of this procedure is unfortunately not good. Besides, I do science with my personal limited resources. Nonetheless, I will use a more professional service after a revision. The mathematical notation was already validated by a mathematician for the current submission.

Furthermore, there is no uniformity in scientific writing. The explanation style is extreme short in Mathematics compared (e.g.) with social sciences. I am more oriented to the first and prefer sparsity.

Besides, I was not sure about the level of mathematical/stochastic expertise of the different (and fragmented) science communities which deals with natural catastrophes. According to your and the other reviewers commends, more mathematical explanations are needed. I will add a subsection. However, I don't want to explain too many details which would be trivial for a student in mathematics (may be 3rd semester). A higher level of statistical expertise of the reader is assumed what I will mention in a revised introduction.

In addition, I will change the structure of the MS in a revision. Nevertheless, I underline that there are very different accepted or even prescribed structures for scientific papers (e.g., [Nature Scientific Reports](#)). And I keep the distinction between the new approach (CRP as main result) and technical details for the demonstration example.

A classification of a reference as ancient can be critical. Mathematicians prefer to refer to the original explorer/inventor of a theorem and its proof. I am oriented to this practice.

I do not share your opinion that the new opportunity for estimation of risk curves by CRP is not a clear result. Previous estimates were not reliable or are based on complex models with high numerical burden.

My replies to your detailed comments are below.

Once more, thank you very much for your advice, notes and comments.

Sincerely,

Mathias Raschke

General comments:

a) Structure

It is not very clear what is the exact aim or major finding of the article. In my opinion, it is the development of the CRP and its application to catastrophe modelling. Probably rewriting the abstract could help to point towards the main objectives and findings of the study. Section 1 and 2 are relatively clear in their objectives. However, I don't understand how Section 3 relates to the first two sections and the added value of discussing a "secondary method". Is there a comparison between this secondary method and the CRP, I believe there is one but it is extremely hard to identify how, why and where. I do not understand the why Section 4 can not be included into Section, to which it is related. Section 5 summarises well some of the key aspects of the paper. I think the paragraph on spatial dependence comes too late, as the stakes around this concept are never introduced in the article. The article needs a paragraph on spatial dependence right in the introduction.

Reply: As aforementioned, I will change the structure and modify the introduction. However, spatial dependence is not topic the first time in section 4 in the current MS. The issue of max-stability in section 2 is obviously related to spatial dependence. What is about Figure 2 a and b?

b) Unclear sentences, jargon and lack of context

The core issue of this article is around the writing and the communication of the science. There are plenty of jargony terms that are not introduced in the article. It starts in the abstract, where the concept of return period is thrown without being introduced. Later on in the abstract, max-stable dependence is mentioned, and the definition associated to it is simply not satisfying. It does not explain properly what max-stability means in that context. There are several places in the article where specific terms are used and not introduced (e.g., poisson process, 1.44, extreme value copula, 1.73). It is legitimate to use these models and concepts in the context of the article, however, it seems that many concepts are used here, without being properly introduced and without explaining what are their role and implications. Some sentences simply do not make sense or requires several reads for the reader to guess their meanings (e.g., 1.117-118, 1.220,

Reply: As aforementioned, I will add an additional sub section in a revision to explain some stochastic aspects more in detail. However, I don't write a textbook for statistics. The term *return period* (RP) is already common in the NHESS and is frequently used in abstracts. A recent example is the paper by [Letson et al. 2021](#). The principal of Poisson process is explained in beginning of section 2.1 and the extreme value copula is illustrative explained. The references are not only provided to validate/support the assumptions or results but as opportunity for the scientific reader to get more understanding of details if needed.

c) Lack of supporting evidences

Another main issue of this article is the numerous statements made without supporting evidences. For example, 1.226 "We do not consider the generalized extreme value distribution with index  $\gamma \neq 0$  in (12) for the following reasons". A reason is provided but no source supporting the statement. Same issue 1.314 where "once again" is used without justification. I spot more occasions where references are needed in the detailed comments. Another issue is the age of references used, most reference used in Section 2 and 3 are relatively old (1980's, 1990's), other more recent references are available. Here are some recent articles dealing with multivariate extreme value analysis, copulae and spatial dependence:

Reply: I cannot reproduce your concerns. I have listed statistical indications (evidence) with corresponding references for the assumption  $\gamma=0$  (Gumbel distribution!). These are goodness-of-fit test, criterion of model selection and sample mean and variances if  $\gamma$  would be estimated. I will try to formulate some sections clearer.

"Once again" - Do you really want more examples about the important role of statistics/stochastic in the research of (NatCat) risk?

Besides, the age of a reference is not a universal criterion for its appropriateness. Why I should use younger publications in the current context, to move me away from the source (inventor/explorer of a knowledge)?

## d) Introduction of Section 2.

The introduction of Section 2 is very unclear, it consists in a succession of unsourced statements “Stochastic deals with more than only random variables” 1.44, “ A NatCat event is measured by its local intensity” 1.45, etc. it does not provide a clear vision of the concepts used to design the CRP. Maybe a figure could help the reader to understand what questions the CRP is answering to. I am not even sure that I understand how to practically compute a CRP, is it calculated using only stations impacted by each storm event? Or over the whole Germany? It is also not clear what is done in case of non max-stability (despite the supplement).

Reply: As aforementioned, I will add a sub section in a revision to explain some stochastic details better. However, a “clear vision” of the CRP was already mentioned in section 1 – the quantification of a return period of a NatCat event. And my statement “Stochastic deals with more than only random variables.” is a platitude. I will change a bit the formulation, but I refuse to provide references. Don’t you know random fields or random graphs? The application of CRP to winter storms over Germany is not part of the introduction of section 2. However, I will modify my explanation.

Specific comments:

## a) Line 30 p1. In sum, previous approaches are not very fruitful. Fruitful for what purpose?

Reply: Obviously, return periods (RP) of a NatCat are the topic. I will modify it.

## b) Line 32 p2. In the end, the RP of losses and damage (the risk curve) is needed. It is needed for what? By who?

Reply: Thank you for the note. I will change it. (“...to quantify the risk”).

## c) Line 34 p2. Very unclear sentences, requires rewriting

Reply: I will modify it.

## d) Line 37 p2. “Furthermore, we use the derived scaling opportunity of historical event fields to”. I think this sentence is very hard to understand for any external reader, needs rewriting and introduction of the jargon used.

Reply: I will change it.

## e) Line 40 p2. Section 4 is not introduced. 1 or 2 sentences regarding this section need to be added.

Reply: You are right, and I apologize. I will change.

## f) Line 46 p2. “This local intensity occurs”, do you mean a local extreme associated to an event?

Reply: It depend on the concrete NatCat model. Frequently the extremes are used. I will modify it.

g) Line 66 p3. The explanation around the angle  $V$  seems accurate but not so well explained. Is  $V$  the “exponent measure”? I recommend you use these references provided in General comment c) to explain the role of  $V$  (maybe it is better explained in the appendix).

Reply: As written already, the angle  $V$  is not of interest for the CRP. Therefore, I only refer to previous stochastic publication. In the reversion, I will cite Coles (2001) “*In other words, the angular spread of points of  $N$  [the point process] is determined by  $H$ , and is independent of radial distance.*”. Besides, I will add a sub section to explain max stable random fields (Schlather’s 1<sup>st</sup> theorem, 2002) better.

## h) Line 68 p3. A sentence explaining what is a copula is required.

Reply: It is already explained – it is the dependence structure. I cannot provide a textbook about stochastic within a scientific paper. I will point this out to the reader in the introduction. In addition, I will try to rewrite.

- i) Line 73 p3. “The independence gives this max-stability of the dependence structure between pseudo angle  $V$  and pseudo radius  $R$  in (4) (Coles, 2001)”. I don’t understand what this means. Please rewrite the sentence.

Reply: As aforementioned, I will cite Coles (2001).

- j) Line 72-82 p3. General comment on this paragraph, it is very hard to follow the author here. Links between sentences are not working. I suggest to rewrite the entire paragraph and work on linkages between sentences/concepts.

Reply: Thank you for the advice. I will modify the paragraph. I note the "accident" of proofreading procedure.

- k) Line 94 p4. It is unclear what is the scaling factor  $S$ .

Reply: Have you read equation (9) in line 95? I will try to modify.

- l) Eq.9 p4.  $T_{CS}$  is not introduced.

Reply: I thought it would be self-explaining by equation (9). I will change.

- m) Line 107 p4. “It can also be derived from the moments of random variables that the coefficient of variation (CV; Upton and Cook, 2008) for (10) **is not be concerned** about scaling (9) for max-stable situations”. I think the English is not accurate here. The coefficient of variation needs to be defined ( $cv = sd/mean$ ).

Reply: I will change it and extend the explanation.

- n) Line 112 p4. Please choose one name for the storm you are analysing. in the article, you use winter storm, extratropical cyclone, winter windstorm in different places.

Reply: I will check the opportunity. I prefer the application of synonyms since these are also applied in the different science communities.

- o) Line 117 p4. “The reason is explained in Section 3.1 and the appropriateness of the Gumbel distribution for the block maxima of local event intensities and corresponding computation of RP per event with bias correction.”. Sentence does not make sense. Please rewrite.

Reply: Once again an “accident” of proofreading procedure. I apologize and will correct it.

- p) Line 122 p5. What do you mean by “pure phenomenon in the geographical space”?

Reply: The natural event for itself without the consequences. I will extend explanation.

- q) Line 127 p5. It is very hard to follow the argument about the difference between empirical results and model results. I think some clarification in the writing is needed.

Reply: I will try to modify. However, the current detail in the bracket provides information about.

- r) Line 131 p5. “Usually, level 5% is used; however”. There is a problem with this sentence.

Reply: I will delete “however”.

- s) Line 140 p5. “The plot of the estimates of dependence measure Kendall’s  $\tau$  (Upton and Cook, 2008) is depicted in Figure 2 b”. I think you need to introduce the whole figure 2 before that sentence to reduce confusion for the reader.

Reply: I modify it. However, a sentence, that details of the spatial characteristics are presented in Figure 2, does not provide more information for the reader. Independent on it, I must correct figure numbers in the text (2 b is 2 and so on).

- t) Line 142 p5. Should it be figure 2b? and 2a line 140?

Reply: Yes.

u) Line 151 p6. "The  $p$  value is 0.002 for an exponent  $\leq 0$ ; this confirms the non-max-stable behavior of Kendall's  $\tau$ ". Which test did you do?

Reply: t-test in an excel tool. I will mention it.

v) Line 181 p7. What is this <sup>0</sup> doing here?

Reply: It is a relic of a previous draft for a different journal. I will delete it.

w) Line 194 p7. Not clear what the conclusions of the section are, and how one should interpretate Figure 4.

Reply: I will add a little conclusion for the subsection that it was shown that the scaled CRP can be used to estimate a risk curve. However, not every sub section needs a conclusion since I do not write a textbook. Figure 4 is not related to section 2 but to section 4. If you mean Figure 3 - I refer already 4 times to the figure in the current draft. It simply illustrates the results.

x) Line 219 p9. 141 stations over how many in total?

Reply: What is your definition of "total"? The number of DWD wind stations in 2005 or in 2010? I can only mention the number of stations of the DWD data portal.

y) Line 220-223 p9. I don't understand these sentences.

Reply: I will either modify and extend the text or shorten it and only inform the reader, that I have tested for autocorrelation.

z) Line 226 p9. So you decide that the shape parameter ( $\gamma$ ) must be equal to 0. I am not convinced by your justification, I think this requires more supporting evidences as the shape parameter is often subject to debates in EVA.

Reply: Please do not reduce my result to an subjective decision and I do not state that it "must be". All relevant statistics (Goodness-of-fit test, information criterion, sample mean and variance of estimated extreme value index) confirms that the Gumbel distribution is reasonable. In contrast, who declares that the extreme value index may vary between the positive and negative range for the same physical phenomenon should explain why. Why can the wind speed infinite at the one station but has an upper finite limit at an adjacent station? The burden of prove is on your.

aa) Line 258-264 p10. It is very unclear what parameter is related to which equation. I think the paragraph needs rewriting to improve clarity.

Reply: I will rewrite it.

bb) Line 276 p11. The reference Della Martin et al. needs a date and a small introduction as you compare your results to this study's results.

Reply: I will add the date. This reference is already introduced in the first section.

cc) Line 299 p12. I think this method is not that well-known, it would be better to explain quickly this method for the reader. Additionally it is Coles (2001), not (2011).

Reply: I will mention synonyms for the method and briefly sketch the idea of the method in a revision.

dd) Line 314 p13. If it is once again please provide supporting references.

Reply: My sentences would be a platitude if stochastic and statistics would be applied appropriately in every research about NatCat. Unfortunately, this is not the case as shown in the following discussion. I will only swap the word "prove" by "show". If you have any reference that indicates the opposite of

my statement (means that statistics and stochastic would not be a central element of risk analysis), please send it to me.

ee) Line 315 p13. “The CRP is a simple, reasonable, and testable stochastic measure for a catastrophe”. This sentence simply does not work, there are too many adjectives and it does not bring new information.

Reply: I will modify it. However, purpose of section *conclusion* is not the presentation of new information.

ff) Line 323 p14. “simplicity and clarity”. It is not simple and clear at the moment.

Reply: As it is written, the measure for my statement are the vendor models.

gg) Line 368 p15. A kind of? Really?

Reply: I do not understand your comment. “a kind of regionalization” is written and a reference is mentioned.

To conclude, It was very difficult for me to understand the methods and processes developed in this paper. I believe this is partly due to my limited knowledge of catastrophe modelling, but the main reason is in my opinion the writing style of the article. The communication of the science is not good enough in the current version. To finish on a positive note, I found Section 5 very informative. It is only after reading the last paragraphs that I finally understood many aspect and problematics of the article. I think the author should move some of the paragraphs in Section 5 to Section 1 and 2 in order to improve the clarity of the manuscript and its readability.

Reply: I completely agree that the current witing style is not acceptable and must be improved. I will change this and the structure and add a further sub section to explain more details of the stochastic. Maybe, the applied stochastic concepts and its derivate are too new for science communities which dealing with NatCat. However, I cannot present a textbook. Furthermore, I will try to use some issues/paragraphs, currently mentioned in section 5.3 and 5.4, in the introduction.

I look forward to reading a revised version if asked to by the editor.

Reply: I would be grateful if you would agree to review a revised draft. Once again, thank you very much for all your effort regarding my MS.

#### **References (which are not listed in the MS)**

Letson, F. W., Barthelmie, R. J., Hodges, K. I., and Pryor, S. C.: Intense windstorms in the northeastern United States, *Nat. Hazards Earth Syst. Sci.*, 21, 2001–2020, <https://doi.org/10.5194/nhess-21-2001-2021>, 2021.

Wikipedia [https://en.wikipedia.org/wiki/Random\\_element](https://en.wikipedia.org/wiki/Random_element)