

Dear Dr. Yves Bühler, Dr. Francesco Serinaldi, Dear Dr. Aloïs Tilloy, Dear Editors,

Thank you very much for the opportunity to revise my manuscript (MS) "About the return period of a catastrophe".

My detailed and long responses to referee's comments have been already uploaded to the discussion area (<https://nhess.copernicus.org/preprints/nhess-2021-86/>). However, I attach these again.

According to the suggestions of the referees, I have rewritten many parts of the MS, added details of references and new references, and have added new figures (and delete some old). Besides, I have changed the structure significantly (see below). The entire manuscript has been proofread by an expert for technical documentation in English.

Furthermore, I have also written a new section with an extension of a theorem of extreme value statistics and discussion of spatial dependence and characteristic. However, I cannot provide a textbook in statistics.

Unfortunately, I may only upload PDF files of the revision. In my PDF version, there is no navigation area, the document structure (headlines) is not shown separately. If the referees need, I can send a doc-file version for a better handling.

I hope my MS is now acceptable for a publication in your journal since I am convinced that the topic is very important for the NatCat science community and for practitioners in industry.

Thank you very much for your consideration and all your effort regarding my MS.

Yours sincerely,
Mathias Raschke

The new structure of the MS:

- 1 Introduction
- ▲ 2 Max-stability in statistics and stochastic
 - 2.1 The univariate case
 - 2.2 Max-stable copulas
 - 2.3 Max-stability of stochastic processes
 - 2.4 Spatial characteristics and dependence
- ▲ 3 The combined return period (CRP)
 - 3.1 The stochastic derivation
 - 3.2 Testability
 - 3.2 The scaling opportunity
 - 3.4 Risk estimates by scaling and averaging
- ▲ 4 Application to German winter storms
 - 4.1 Overview about data and analysis
 - 4.2 The CRP of past events and validation
 - 4.3 Spatial characteristic and dependence
 - 4.4 The risk estimates
- ▲ 5 Technical details of the application example
 - 5.1 Modelling and estimation of local hazard
 - 5.2 Modelling and estimation of vulnerability
 - 5.3 Numerical procedure of scaling
 - 5.4 Error propagation and uncertainties
 - 5.5 RP of vendor's risk estimate
- ▲ 6 Conclusion, discussion, and outlook
 - 6.1 General
 - 6.2 Requirements of the new approaches
 - 6.3 Opportunities for future research
- 7 Code and data availability
- 8 Author's contribution
- 9 Competing interest
- 10 Acknowledgement
- References

Reply to reviewer comments (Francesco Serinaldi/RC1) regarding MS "About the return period of a catastrophe" - nhess-2021-86

Dear Dr Francesco Serinaldi,

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 am not in the privileged position wot be paid for research and writing scientific papers in English. I rarely publish also because 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.

My detailed replies to your comments are below.

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

Sincerely, Mathias Raschke

General comments

I recognize that my report will look harsh, but reading this manuscript was a true nightmare. English is embarrassing, just to use an euphemism, and this makes many sentences/paragraphs completely incomprehensible. Sentences are generally disconnected one another, thus preventing the understanding of what the Author wants to communicate. The material is randomly spread throughout the paper without any logic like the paint in Pollock's artworks (... which would be good if NHESS journal were a Christie's auction); rationale, technical aspects, and results are mixed, and the Author moves back and forward among them without following any rational criterion. Technical aspects are introduced without clear terminology and specification of assumptions and mathematical derivations.

By the way, my frustration is exacerbated by the fact that (i) the material of the paper may be of interest if properly communicated, and (ii) the Author is not a young undergraduate student dealing with his first paper, but a researcher with some experience. Therefore, submitting a paper of such a low quality in terms of presentation is not a matter of lack of capability of writing a decent document. And this makes the poor presentation even less acceptable, as it denotes a sort of lack of attention and respect for editors, reviewers, and readers.

That said, I will try to provide some suggestions about how to reorganize the manuscript. However, let me clarify that a simple rearrangement of the material is not enough. Almost every line of the text requires rewording (to get a decent English) and restructuring (to make sentences and concepts understandable).

Abstract: The structure should be: motivation/problem, proposed technique, and results. Here, the terms of the problem are quite simple and the abstract should look like this:

"Natural catastrophes are spatial process affecting a given area; however, natural hazards and their impacts/effects are generally monitored/measured locally. In order to quantify the degree of rarity (probability of occurrence/exceedance or return period (RP)) of a spatial event (hazard/loss), we need suitable metrics enabling to assess areal risk from the local one. In this study, we propose a metric called "combined return period" (CRP), which is the (weighted) average of local return periods, and can be shown to be a proper return period itself. CRP is characterized by some properties that allow the calculation of the areal/spatial expected loss for a given areal RP or the expected RP for a specified areal loss starting from local RPs, hazard values, and exposure. The paper also discusses the effect of considering max-stable and non max-stable spatial dependence, and introduces bias correction methods for local RP estimates, etc. As a case study, the proposed framework is used to quantify RP and losses of winter windstorms over Germany recorded from 1999 to 2019. Results are compared with those reported in the literature and show that...bla, bla."

Of course this is only a suggestion, but it gives an idea of how to reword the abstract in plain language, following a logical structure (a story, if you want), without mentioning things such as "*testable reproductivity*" (whatever it means) or "*pseudo-polar coordinates*", which require a technical introduction to be understandable.

Replay: I will improve the English in a revision. However, I am also convinced that it is acceptable to mention the theoretical basis of the new concept in the abstract. Therefore, I will also mention the *pseudo polar coordinates of extreme value statistics/theory* in the abstract of a revision. Besides, 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](#). Furthermore, an actual return period is not the same as an occurrence probability even though there is relation between both.

Introduction: This should slightly expand the abstract. References are OK. However, the message is not conveyed. If I understand, the Authors wants to say that the general approach in the existing literature is to classify hazard events evolving in space and time via simple indices, such as the Richter magnitude scale for earthquakes, and then assigning an RP or probability to the observed values of these indices. Conversely, the Author suggests assigning an RP to a spatial event by combining the RPs of the original hazard/loss variables recorded locally. And this approach has the properties/advantages mentioned (very confusedly) by the Author throughout the text. If my interpretation is right, the Author should make it clear.

By the way, please avoid expressions like "*the destruction's extent of the destruction*", or "*In sum, previous approaches are not very fruitful*"... perhaps the Authors means "previous approaches are not very effective". Please also avoid sentences like "*Also, their statistical models include assumptions and pitfalls*": all methods/models are based on assumptions, while pitfalls should be specified or properly referred via suitable references.

Replay: I will consider your suggestions and concerns in a revision. Probably, I will shift a part of the current section conclusion in the introduction following a suggestion of the other referee.

L17-20: These lines are an example of what I mean when I say that many sentences are disconnected. For better reading and understanding, it should read as “Natural catastrophe (NatCat), such as large windstorms or earthquakes, are natural hazards evolving in space and time. This means that the definition/identification of a NatCat event is not unique, and generally relies on both hazard magnitude indices and socio-economic aspects, such as the interest in short-term or long-term effects on the affected areas. Irrespective of a specific definition of NatCat event, this study deals with the assessment of RPs of complex hazard events and corresponding losses affecting multiple locations/areas and spanning a given time interval of interest...”

Reply: I will consider your concerns in a revision. However, your suggestion is very long.

L44-55: This part is an example of the general lack of clarity characterizing the discussion of technical concepts. These lines introduce the key concepts to understand the rest of the paper, and this is done superficially, without the required premises. The Authors merges methodological concepts and empirical aspects, and uses meaningless terms such as “*A Poisson point process... is also a random element*”: why “*also*”? is a Poisson process anything else? what is a “*random element*”? Which paper or book does refer to a Poisson point process as a random element?

Reply: Please aware, that the term *random variable* is mentioned in the previous sentence.

Obviously, *random element* is a generalisation and include *random variables*, *Poisson point processes* and so on. I will not provide a special reference since this is a trivial and self-explaining term and is even described in [Wikipedia](#). For more examples, please use google search (catchwords [stochastic “random element”](#) - more than half a million results).

As an example, this part could read as follows:

“To put our discussion in the context, let us assume that an environmental process of interest, such as river flow or wind, is monitored at a given location by gauging devices that measure for instance river stage/discharge or wind speed and direction. A NatCat event occurs when the measured variable X assumes a value equaling or exceeding a critical value x , i.e. $X \geq x$, thus resulting in possible damages. The occurrence process at a given location can be described by stochastic process, which is a collection of random variables. In particular, a Poisson point process or briefly Poisson process is a convenient model to describe the occurrence of independent events such as rare NatCat events. In Poisson point processes, the number of events, K , over specified time intervals \mathcal{I} (e.g., a season or a year) follows a Poisson distribution with expected value

$$E[K(x)] = \Lambda(x), \quad (1)$$

where $\Lambda(x)$ is the exceedance frequency function, EF, and $K(x) = \sum_{i \in \mathcal{I}} \mathbf{1}(X_i > x)$. The reciprocal of the local EF yields the local RP

$$T(x) = \frac{1}{\Lambda(x)}. \quad (2)$$

Since local EF Λ is uniformly distributed, and the relationship between Λ and T is monotonic, according to the rule giving the distribution of functions of random variables (e.g. Kottegoda and Rosso 2008; pp. 133-142), the EF function of T has Pareto form

$$\Lambda(T) = \frac{1}{T}. \quad (3)$$

etc. ”

Reply: Thank you for the suggestion, I will consider parts in my revision. However, your notation does not convince (e.g., equation (3)). In stochastic, one notation is used for the scale of real numbers. For example, random variables X and Y have cumulative distribution functions (CDFs) $F_X(x)$ and $F_Y(x)$. The domain scale is symbolized by x in both CDFs. My current notation was validated by a mathematician with special expertise in extreme value statistics and theory.

This should give an idea on how to present such a kind of things. Analogously, Eq. 4 should be written as

$$\begin{cases} R = T_1 + T_2 \\ V = \frac{T_1}{T_1 + T_2} \end{cases} \iff \begin{cases} T_1 = RV \\ T_2 = R(1 - V) = T_1 \frac{1 - V}{V} \end{cases} \quad (4)$$

Again, when referring to books such as Coles (2001), Beirlant et al. (2004), and Falk et al. (2011), please indicate the exact pages, as going through a whole book to double check what an author writes is almost impossible. For Falk et al. (2011), please provide reference details (publisher and address).

Reply: I will use your suggested notation (4) in a revision. I will also mention the pages of references in the text (not in the reference list since I do not want to list the same literature twice).

L63: ‘*random element*’??? If T_1 and T_2 are random variables, every quantity resulting from their combination is also a random variable.

Reply: As explained before, *random element* is a general term in stochastic. And T_1 and T_2 are not random variables. “ T is also a point event” as written in line 54.

L70: Please consider something like “Exploiting the properties of Poisson processes, the univariate CDF of maximum RP values occurring in k unit periods can be expressed in terms of the EF $\Lambda(x)$ in Eq. (1) (see e.g. Stedinger et al. 1993; Ch. 18, pp. 37-38)

$$G_K(x) = \exp(-k\Lambda(x)) = \exp(-k/T(x)). \quad (5)$$

etc.” I am not sure that Eq. (3) is needed here.

Reply: I will modify my expression and will not use your suggestion since it is longer. My notation has been validated by a mathematician.

L73-75: “*The independence gives this max-stability of the dependence structure between pseudo angle V and pseudo radius R in (4) (Coles, 2001).*”??? Please clarify, and report pages of Coles (2001) discussing this property.

“*The occurrence of the pseudo radius is once again a point process with EF $\Lambda(x) = 2/x$ - the double of (3).*” Please provide a reference (with pages, if it is a book). By the way, if this sentence refers to pseudo radius, it should be $\Lambda(r) = 2/r$. My understanding is that the Author uses x as a generic variable when he presents an EF of some quantity (e.g. T , R , etc.). However, this introduces

lot of confusion, and makes reading and understanding very difficult, leaving aside possible errors when handling and combining equations. Please use a consistent notation.

Reply: Regarding the notation, please see my replies before. May be your misunderstanding of the notation the reason for your confusion. 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.*”.

L98: Please, clarify.

Reply: I will add a new sub section about Schlather’s theorem/approach.

L106: “*The CRP T_C represents the expectation (or its estimate).*” ... of what?

Reply: I will clarify.

L117: “*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.*”??? What about using subject, verb, and object? Just to write a sentences with some meaning.

Reply: I apploogize and will correct.

L141: “*The scatter range of the half seasons is smaller than for two seasons due to different sample sizes.*” In my opinion, the difference depends on the fact that the ‘two-season’ sample actually merges data from two seasons that are expected to be less correlated, as they are likely non-homogeneous (seasonality effects, etc.).

Reply: No, the reason is the difference between the sample sizes. For every estimate applies that the corresponding standard error decreases with increasing sample size. The samples of the half seasons are concerned by the same seasonality as the sample of a complete of two seasons (two years). The season is divided according to reasonable physical criterion. The first half is from September to December, the second half from January to April. The sub sample of the first half do not differ significantly from the sub sample of the second half according to t-test and F-test.

L151: ‘*this confirms the non-max-stable behavior of Kendalls τ .*’ How can rank correlation coefficients be max-stable?

Reply: If the dependence structure/copula is max stable (the same for one or ten seasons or years), the corresponding dependence measure (which also parametrise the copula) must be also stable. As aforementioned, I will add a further sub section which explains this amore in detail.

L183: “*is more minor than*” → ‘less than’

Reply: I will consider it in a revision.

L241: Please provide reference or derivation for Eq. (13)

Reply: As written in the MS short before equation (13), its parameters correspond with parameters in equation (12). In addition, the relation between extreme value distribution and frequency function was already presented in section 2.1. I will consider your concerns it in a revision.

L290: “*Our estimation variants are formulated by (11).*” Variants of what? SARS-CoV-2? What about making things readable? For example, “In this section, we show how to use Eq. (11) to derive alternative estimates of this and that... bla, bla”

Reply: Thank you once again for the helpful notes. I will consider it in a revision.

L297: “*The estimation is based on following stochastic relations and assumptions (or proxies)*” Estimation of what? Proxies? Is it so difficult to start a section trying to explain what is gonna be presented?

Reply: I will modify the sentence it in a revision.

L299: “The origin is (5); the well-known delta method (Coles, 2011) for computation of propagation of errors is also a base. A more illustrative explanation is provided for the loss scaling by Figure 6 a.” Origin of what? Why talking about delta method without any justification? Is it a base for what? I hope the Author will recognize that these sentences are presented without any logic and explanation. The scope of a paper is the communication of ideas; this manuscript is more similar to a collection of personal notes reporting only some keywords for Author’s record, and neglecting the fact that a reader is not clairvoyant, and cannot read the Author’s mind to shed light on those short notes.

Reply: When an equation is presented immediately before a sentence (no new paragraph) then it is very likely, that the latter is related to the equation. Why is a justification of the delta method needed? The delta method used in many statistical analyses and is well explained, for example, by Coles (2001). There are further course books for statistics which explain this approach. However, I will reformulate the sentences in a revision.

L359: “Further arguments...” → ‘There are *further arguments...*’: subject, verb, object... it is not so difficult, I think.

Reply: Even though, we could discuss about the style, I will consider your concerns in a revision.

To conclude, as mentioned above, this manuscript is one of the most badly written documents I handled in the last months as reviewer and editor. Nonetheless, the topic may be of interest; so, I think it deserves a chance to become a readable paper. However, I want to be clear: cosmetic changes are not enough. Every sentence, paragraph, section, and the overall structure require to be carefully revised. The Author can consider involving colleagues that can help in this respect. Concerning the language, the Author can consider the use of proofreading services.

Reply: Thank you very much for all your advice, notes and suggestions. I will consider these in a revision.

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

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 γ would be estimated. I will try to formulate some sections clearer.

"Once again" - Do you really want more examples about the important rule 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 1st 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 I 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 different 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 τ (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 ≤ 0 ; this confirms the non-max-stable behavior of Kendall’s τ ”. Which test did you do?

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

- v) Line 181 p7. What is this ⁰ 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 (γ) 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 phaenomen should explain why. Why can the wind speed infinite at the one station but has an upper finit 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 this 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 scetch the idee of the method in a revision.

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

Reply: My senentes would be a platitude if stochastic and statistics would be applied appropriatly in every research about NatCat. Unfortunatly, 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 modells.

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 derivates 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