

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

Thank you very much for all your effort regarding my manuscript (MS) and the opportunity to improve my MS by a revision.

I have considered your comments in the revision. As suggested by you, Dr. Tilloy, I have added a computation example for the CRP of a winter storm in Germany in the MS. The entire calculation for this event is presented in an additional worksheet in the Supplementary data. Furthermore, I have revised my notation, as suggested by you, Dr. Serinaldi, to ensure that applied scales (arguments of functions) are consistent regarding their physical unit. I do not follow your request, Dr. Serinaldi, to call the CRP a statistical index for following reason. An index ([wikipedia.org/wiki/Index](https://wikipedia.org/wiki/Index)) is constructed with real world variables; the CRP is a stochastic measure. An index is constructed for a special problem/question; the CRP is universal. My detailed replies to your comments are attached.

During my revision, I noted my mistake which you have not realized. The root of mean squared error is falsely named mean squared error in text and equation. I have also corrected this.

I am happy to note that you have no negative criticism regarding my main results (derived concept of combined return period (CRP), its scaling property, extension of Schlather's [2002] 1st theorem, new approach for risk estimation, and application example: winter storm over Germany). Because this circumstance and the universality of my approach, its testability, and corresponding new possibilities for research of natural hazard, I cannot understand your judgement, Dr. Serinaldi. Only one level better than "poor" for criteria "Scientific significance" and "Scientific quality" does not seem to be fair and plausible (there are four levels). This applies especially if I compare my results with other publications such as in the NHESS journal.

For your better understanding of my revision: I have carried out it in following steps:

1. Incorporating of your suggestions, Dr. Tilloy
2. Incorporating of your suggestions, Dr. Serinaldi
3. Using of two professional proofreading services (I attach the PayPal receipts)
4. Incorporating the results of proofreading services
5. Final check

Unfortunately, the results of the two proofreading services are not very consistent and I had to decide which one I follow at the relevant paragraphs.

I hope my MS is now acceptable for a publication in the NHESS your journal, Dr. Bühler, 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. I apologize for my awkward English.

Yours sincerely,  
Mathias Raschke

Annex: PayPal receipts for proofreading services and detailed replies to referee's comments

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Detailed reply to Referee I (*italic*)

*Dear Dr. Francesco Serinaldi,*

*Thank you for your considerable effort regarding my MS. I have widely considered your comments. However, I have used a professional proofreading service. May be, that in some cases, they suggested a modification. Besides, in some cases, your comments and suggestions irritated me.*

*Regarding the notation, I have revised and modified it to ensure consistence of physical scales.*

*Regarding your first suggestions regarding the writing, I had tried to consider it in my first revision. However, I had also to consider further aspects. Currently, I could only try to copy your comments from your PDF. This procedure does not perform well (changes are not copied). Nevertheless, I hope that none of your suggestions and corrections slipped me away.*

L6: "How often does such an event occurs? What is their its return period (RP)?"

*Reply: Thank you. I have corrected.*

L10: "event and their spatial"

*Reply: Thank you. I have corrected.*

L12 "For this purpose, we extend previous construction for max-stable random fields from extreme value theory and consider a recent concept from NatCat research" "a recent concept"? Which concept? If you refer to something mentioned above, please use "the above mentioned concept of..." otherwise "a recent concept from NatCat research, i.e. CONCEPT X".

*Reply: According to your previous advice, I do not want to use to many rare terms in the abstract. However, I will add the term area function.*

L20-23: please consider this version: "Moreover, we propose a new statistical index highlighting that the spatial dependence decreases with increasing block size. We compare our risk estimates with existing risk models that do not allow for such a detailed control of spatial characteristics and dependence."

Please do not discard my suggestions for text changes outright. It is not a matter of style, it is a matter of syntax. I spent time to suggest alternative versions of some parts of the text because yours is difficult to read.

*Reply: I strongly reject your suggestion that is mor an instruction than a comment/suggestion. You do not present any scientific argument for your heavy intervention in my terminology. Usually, an index is a construction/statistic of real-world variables ([https://en.wikipedia.org/wiki/Index\\_\(statistics\)](https://en.wikipedia.org/wiki/Index_(statistics))). I use the stochastic measures return periods and generate a new stochastic measure which is also a return period. Or would you call a return period as index? Or is the product of probabilities of two independent events an index?*

*Besides, your suggested statement is wrong. The decreasing of spatial dependence is a property of the application example and not my general statement. The combined return period can be applied to both as shown in the risk analysis or German winter storms. One estimate is under the assumption of max-stability, the other (more realistic one) with decreasing spatial dependence.*

L25: "... the question arises, how does"? are you sure? I prefer the version "... a question arises: how often...", alternatively "... a question arises: what is the frequency of occurrence...?"

*Reply: I have changed. It was the suggestion of my first unprofessional proofreading service.*

L27-29: please consider “definition of event. This definition may vary by peril, but it is not the topic of this study even though it influences our research object – the RP of hazards and NatCat events.”

*Reply: I have changed. Thank you.*

L30: “index is so far frequently used”

*Reply: I have changed. Thank you.*

L31: “magnitudes scales... But However, their RP may not correspond well with”

*Reply: I have changed. Thank you.*

L34: “measure; but however,”

*Reply: I have changed. Thank you.*

L36: “The different definitions of these indices result”

*Reply: Thank you for the suggestion. I keep the current formulation “Their different...”.*

L38: “In sum, as previous approaches are not satisfactory regarding the stochastic quantification of a hazard or NatCat event, we develop a new approach.”

*Reply: Thank you for the suggestion. I use the first part in my revision but not the second because the subsequent sentence.*

L40-43: “Building on results of extreme value theory and statistics, we mathematically derive the concept of combined return period (CRP), which is the average of RPs of local event intensities. As we will show by a combination of existing and new approaches from stochastic and NatCat research, the concept of CRP is strongly related to the spatial association/dependence between the local event intensities, their RPs, and corresponding block maxima, such as annual maxima.”

*Reply: Thank you for this very nice suggestion. I use it.*

L44: “Spatial dependence is not suitably considered in previous research about NatCat. This issue...”

*Reply: Thank you for this very nice suggestion. I use it.*

L45: “... by Mitchell-Wallace et al. (2017, Section 5.4.2.5) about NatCat modelling for insurance industry. Jongman et al. (2014)...”

*Reply: Thank you for this suggestion. I use it.*

L48: “... assumption; examples are the snow model...”

*Reply: Thank you for this suggestion. I use it.*

L52: “Also Raschke et al. (2011) proposed a winter storm risk model for a power transmission grid in Switzerland without validation of stability assumption.”

*Reply: Thank you for this suggestion. I use it.*

L56: “Youngman and Stephenson (2016) suggested a statistical model to simulation hazard events.”

*Reply: Thank you for this suggestion. I use it.*

L55-61: No, your interpretation is wrong. Neither Youngman nor Papalexioiu simulate “local annual loss from catastrophes”. Youngman generates events with local POT, and then they extract spatial “catastrophes”: they explicitly state that some of their events are not extreme in terms of financial losses.

Youngman generates events with local POT, and then they extract spatial “catastrophes”: they explicitly state that some of their events are not extreme in terms of financial losses. Papalexioiu perform continuous simulation of the whole process (e.g. daily random fields/time series): extreme

events in terms of financial losses or whatever else can be extracted from the whole process. Criticizing papers is legitimate and necessary, but be sure of what you criticize before doing that  
*Reply: I did not state, they would stimulate "local annual loss from catastrophes"! This was an example for the difference between random variable with sure drawn and a point event with accidental occurrence (or nonoccurrence). I have already reformulated this sentence for the other reviewer.*

L66-67: "The corresponding assignment of Asadi et al. (2015) of one local flood peak to peaks at other sites does not convince us completely." I'm sorry but this kind of statement is uninformative and not fair. As mentioned above, criticism is legitimate, but we cannot say "I do not like this or that, period" without explaining why. Such personal opinions without supporting arguments are good for newspapers, not for scientific journals. My suggestion is to focus on your own work rather than expressing vague opinions on others' work. If you want, you can write a discussion paper focusing on those criticisms.

*Reply: I accept your criticism and focus on Jongman et al. (2014) and the comment by Raschke, 2015b).*

L68-70: please, reword in a readable language.

*Reply: I have changed.*

L75: "opportunities for" → "approaches to"

*Reply: I have changed to "opportunities and approaches"*

L78: "opportunities"... again; please, use appropriate terms.

*Reply: I have changed.*

L88: "2nd Section and illuminate..." → "Section 2, where we recall the concept of max-stability for single random variables, bivariate dependence structures (copulas) , and random fields"

*Reply: I use your suggestion.*

L90: "The more recent approaches of hazard event related area functions (Raschke, 2013) and survival functions (Jung and Schindler, 2019) of local event intensities within a region are implemented therein to characterize spatiality." ??? Do you mean "The more recent approaches to area functions (Raschke, 2013) and survival functions (Jung and Schindler, 2019) of local event intensities within a region are implemented therein to characterize spatial properties of areal hazard events."?

*Reply: I use a modification of your suggestion.*

L92: "In the 3rd Section" → "In Section 3"

*Reply: I use your suggestion.*

L93: "scaling opportunity" → "scaling property"... please, stop using "opportunity" in every sentence and out of context. Please, use terms that are more appropriate.

*Reply: Please do not state something wrong such as I would use the term in every sentence. I also write about opportunities/possibilities in NatCat research. I have modified the sentence.*

L97: "to remain clarity of the main paper and limit its extent" → "to keep the paper clear and focused on main results"

*Reply: This is already changed according to the suggestion of the other reviewer.*

L98-99: OK. However, please note that you use “x” to denote different things in the same section and same equation. This is not simplicity but only confusion. Please, see my example reported above about  $X = \text{flow}$ , and  $Y = \text{rainfall}$ .

You should establish a clear notation. If “X” denotes a random variable and “x” the values assumed by “X”, if you introduce another random variable “Y” its values cannot be denoted by “x”. Please think about that... and avoid answers like “my mathematician friend told me... bla, bla”, as this does not justify bad notation.

*Reply: Please don't be rude. I have revised my notation regarding the reasonable part of your comment.*

L99: “We also expect that the reader is more familiar with statistics and stochastic than only with basics about random variables. Statistical significance, goodness-of-fit tests, random fields, or a Poisson (point) process (Upton and 100 Cook, 2008) should be familiar terms.” → “We also expect that the reader is familiar with statistical and stochastic concepts such as statistical significance, goodness-of-fit tests, random fields, and Poisson (point) processes (Upton and 100 Cook, 2008).”

*Reply: Thank you for the suggestion. I use it.*

L104: “Before we formulate the CRP and discuss their opportunities, we must present, discuss, and extend a corresponding topic – max-stability in extreme value statistics especial of random process and fields.” → “Before introducing CRP and its properties, we discuss and extend the concept of max-stability in extreme value statistics with focus on random process and fields.”

*Reply: Thank you for the suggestion. I use it.*

L121: “can replace by a copula” ... grammar...

*Reply: Is corrected.*

L123: “a universal distinction”??

*Reply: I have changed.*

L124: “principals”??? → principles!

*Reply: Thank you. I have corrected.*

L157: “verities of standard deviation”... verities ???

*Reply: I have changed.*

L227: “exception” → “expectation”

*Reply: Thank you. I have corrected.*

L248: “it’s” → “its”

*Reply: Thank you. I have corrected.*

L309: “3.2 The scaling opportunity” → “3.2 Scaling property of CRP”

*Reply: I use your suggestion.*

L325: “We introduce an alternative method. According to (18), the expectation of an unknown ELRP between ELRP and CRP provided). To get a good estimate of ELRP, we must average the of many is the CRP of the local event intensities; the CRP is an estimate of the ELRP (max-stability events with the same event loss. We cannot observe such, but we can stochastically scale historical events respectively their local intensity observations.”

This concept is the fundamental message of this study!

Please, emphasize it and put it clearer!

For example

“We introduce an alternative method. Under the assumption of max-stability between ELRP and corresponding to (conditional on) the CRP, is the CRP of the local event intensities. This means CRP, according to (18) with  $T_1 = \text{CRP}$  and  $T_2 = \text{ELRP}$ , the expectation of an unknown ELRP that the CRP is an estimate of ELRP under max-stability assumption, that is, we can use widely averaging the of many events with the same event loss to get a good estimate of ELRP. However, available hazard data to make inference on the risk of less available loss records. In fact, (18) implies observations of events with same loss are not available. Nonetheless, we can exploit the stochastic scaling property of CRP to rescale the local intensity observations of historical events to get the required information”  
*Reply: Thank you. I have widely considered your suggestion in my modification. However, your suggestion is not really perfect. “In fact, (18) implies observations...” does not make sense for me.*

L335: “be adjusted in an iteration until the defined event loss is the result of (26).” → “be adjusted iteratively until the result of (26) converges to the desired event loss.”  
*Reply: Thank you. I use your suggestion*

L343: “Delta method, well explained by Coles (2001, Section 2.6.4),” → “Delta method (Coles, 2001; Section 2.6.4),”  
*Reply: Thank you for suggestion. I use it.*

L345: “derivates” → “derivatives”, “derivatives” ... English!!!  
*Reply: I have changed.*

“In its meaning, we can also average the event loss for a fixed/determined CRP respectively its scaled variant.” ... reword in understandable English!!!!  
L346-359: please, reword in decent English  
*Reply: I have modified.*

L413: “accepts” → “does not reject”: statistical tests cannot accept anything; they only reject or not reject. The latter outcome only means that data/info is not enough to discard  $H_0$   
*Reply: I have modified. However, there is a number of statistical publications which use “accepts”.*

L423: “half-season maxima and two season maxima” ... “two season” is confusing, as you actually have season/annual maxima and two half-season maxima per year. I suggest being consistent throughout the text, and use “season maxima” or “whole-season maxima” referring to the period from September to April, and “half-season maxima” for the data corresponding to Sept-Dec and Jan-Apr.  
*Reply: I have modified.*

L426-429: Please, reword.  
*Reply: I have modified.*

L435-436: English, please!  
*Reply: I have modified.*

*Once again, thank you very much.*

*Sincerely,  
Mathias Raschke*



Detailed reply to Referee II (*italic*)

Dear Dr. Alois Tilloy,

*Thank you for your considerable effort regarding my MS. I have widely considered your comments. However, I have used a professional proofreading service. May be, that in some cases, they suggested a modification.*

The structure of the article has really improved and the article is much easier to follow. However, Section 5 does not fit in the flow of the article (data, method, analysis, result) in my opinion. One option could be to put it in appendix or to merge it with Section 4. I let the author decide on what is best.

*Reply: Since my basic result is a new stochastic approach/concept for describing/modelling and estimate of natural hazard and risk, the conventional flow (data, method, analysis, result) does not work. My flow is previous stochastic approaches and issue, derived new stochastic approach, application example, and technical details of the application example. I think section 5 is important for the reproducibility of the application example even though it is not relevant for my new stochastic concept.*

a) Abstract, communication on the potential of the new method

I still think that the main results of the study and the potential of the CRP are not clearly highlighted in the abstract and the conclusion. For example, spatial dependencies and spatial characteristics of events are extensively discussed, but I am still unsure about how you accounted for spatial dependencies in the CRP and how this is impacting the loss estimates. Here I explain what I understood from the method and why it leads me to that comment. Please correct me if I am wrong.

- You estimated the CRP for each historical event by averaging local RPs of these events (obtained with Gumbel distribution fitted on annual maxima).
- You then associated these CRPs with losses to obtain a vulnerability curve.
- You then scaled up the CRPs assuming non max-stability (as you show statistical indicators provide evidences in that direction) with scaling factors specific to each event (l.435).

My question is: how do you then obtain a “generic” loss estimate for high return periods if each storm in the region has specific set of parameters?

*Reply:*

*Basically, you have understood my procedure. However, I must clarify following details*

- I have scaled the RPs and CRPs and estimated the risk for both, max-stable and non-max-stable spatial dependence as I have already written in the abstract (recent version) “We also state that our risk estimate is higher for the max-stable case than for the non-max-stable.”. The two risk curves are already shown in figure 7 c (last version of my manuscript).*
- The local RPs of the events are obtained from local hazard function with the same parameters as the Gumbel distribution. The latter was fitted on maxima of a half season (half of a year, not of a year) what can be easily transformed to the distribution of annual maxima.*

*To your question: The risk estimate for higher RP is done for both cases by “...averaging corresponding risk parameters such as...” (abstract). There are three variants/opportunities*

*of averaging as explained in the manuscript. I have modified the abstract in the current revision (“...and averaging the computed event loss for defined CRP or averaging of computed CRP (or it’s reciprocal) for defined event loss.”). The specific parameters are only related to the scaling of CRP and local RPs in case of non-max-stable spatial dependence. I will try to add sentences to make it clearer.*

Specific comments:

1) L.58, p2: “An example illustrates the difference; the random variable *local annual loss from catastrophes* is realized every year even though not one catastrophe and loss event need to be occurred”. I don’t understand which difference you are referring to in this sentence.

*Reply: I will change to “The draw of the annual random variable (e.g. sum of annual NatCat loss) is sure; the occurrence of a point event (e.g. NatCat loss) in this year is not sure but random.”.*

2) L.97 p4: “to remain clarity of the main paper and limit its extent”. I think “remain does not work here, you could use “preserve”. But in my opinion this sentence is not very useful, most readers know that supplements are here to provide complementary information and reduce main article length.

*Reply: I delete this part of the sentence.*

3) L.109, p4: Why is there an indent for that sentence?

*Reply: I delete this.*

4) L.115, p4: “These are well-known facts...”. You can keep this sentence if you wish, in my opinion it is over-emphasizing the fact that these are “basics”. You do this in other places in the manuscript (e.g., L.343) and it gives the impression that you are proving something to someone, which is not the aim of an article like this in my opinion.

*Reply: Thank you for the advice. I delete the “well-known”. My issue is that I have not the impression that all players in NatCat communities know the basics from statistics and stochastic.*

5) Section 4: Maybe a figure showing how the method computes in practice the CRP of a given event and its position Figure 7b could be beneficial.

*Reply: The computation of a CRP simply follows equation (20). I don’t know how to provide a corresponding reasonable figure. If the local RPs are known and the weightings are defined, then the computation is simple. However, I could and have add a table with a computational example section 5 – the technical details for the application example. Here, the steps from windstorm event over wind station to the CRP are explained.*

6) L.362, p13: “wind winter windstorms”. Is it really what you want to write?

*Reply: I delete “wind”. Thank you.*

7) L376, p14: “wind gust” instead of “wind guest”

*Reply: I have corrected. Thank you.*

8) L.426, p15: I think it should be “normally” instead of “normal”

*Reply: I have corrected. Thank you.*

9) L.461, p17: I don’t think TSI has been introduced before, I personally don’t know what it means

*Reply: I apologize. There was already an introduction but deleted during a revision. I have add the full term.*

10) L.498, p18: “A further bias was discovered, the EF is well estimated by (30) in contrast to the RP  $T$ , this is strongly biased”. I do not understand what you mean here.

*Reply: I change to “In addition, the exceedance frequency is well estimated by (30) in contrast to the RP T. The latter is strongly biased.”.*

11) L. 513, p19: “At first, an extensive physical explanation would be required if some wind stations are concerned by a finite upper bound for  $\gamma < 0$  and other stations not with  $\gamma \geq 0$  according to (29). Why should be local wind hazard short tailed for some wind stations and heavy tailed for others?”

This is almost inquisitorial, I don't think it is appropriate and needed in the manuscript. I understand that you did not like my comment (comment z) in the previous round of review. In the first version of the manuscript, you stated rather arbitrarily that it was the best choice to constraint the shape parameter. I personally believe that invoking physics to justify values of statistical parameters is always tricky, but I accept the explanation you offer. I am more convinced by the by the fact that statistics suggest that the shape parameter is indeed very close to 0. Thank you for you making the effort to estimate the shape parameter for every station.

*Reply: Thank you for the advice. Since the question “Heavy or short tailed?” is important (as you also have written in your first review report) and the stochastic differences implies physical differences, I let my note in the manuscript. However, I reformulate it.*

12) L.515, p19: “(catchword Reynolds number)”. Maybe something else should be written here? Same thing happens L.635 and L.648.

*Reply: I have deleted two of the three brackets and replace “catchword” by “keyword”.*

13) For supplement, it is SupplementAry not SupplementOry.

*Reply: I could not find “SupplementOry” in my last revision of manuscript and Supplementary.*

*Once again, thank you very much.*

*Sincerely,  
Mathias Raschke*