Dear Dr. Raschke.,

Thank you for giving me a chance to review your manuscript " About the return period of a catastrophe".

I think the article tackles a very relevant problematic, the estimation of return periods of winter storms and the linkage between return periods and losses. However, the quality of the writing and the poor organisation of ideas and concept make the article relatively inaccessible. There are too many rough statements without justification and unclear sentences.

If I understood well, the article is divided into two main parts. The first part develops the concept of CRP and applies it to Strom Kyrill (2007), CRP estimates are then compared to other RP estimates. The second part analyses the risk associated to winter storms, without clear results. I have provided a number of general comments and some specific to different sections.

In summary, the topic of the article deserves attention, and it is clear that the development of the method presented required skills and time from the author. However, the quality of the writing is not up to the standard for scientific publication. The results are poorly communicated (in text and in figures). Additionally, there are too many statement without supporting evidences and many references are relatively ancient. I would suggest major revisions and an intensive work on the quality of the writing for the article to be published.

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.

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 property 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, l.44, extreme value copula, l.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., l.117-118, l.220,

l.222).

c) Lack of supporting evidences
Another main issue of this article is the numerous statements made without supporting evidences. For example, l.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 l.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:

Cooley, D., Thibaud, E., Castillo, F., and Wehner, M. F.: A non- parametric method for producing isolines of bivariate exceedance probabilities, Extremes, 22, 373–390, 2019.,

Davison, A. C. and Huser, R.: Statistics of extremes, Annu. Rev. Stat. Appl., 2, 203–235, 2015,

Davison, A. C., Huser, R., Thibaud, E.:Geostatistics of Dependent and Asymptotically Independent Extremes, Mathematical Geosciences, 2013

Tilloy, A., Malamud, B. D., Winter, H., and Joly-Laugel, A.: Evaluating the efficacy of bivariate extreme modelling approaches for multi-hazard scenarios, Nat. Hazards Earth Syst. Sci., 20, 2091–2117, https://doi.org/10.5194/nhess-20-2091-2020, 2020.

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” l.44, “ A NatCat event is measured by its local intensity” l.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).

Specific comments:

a) Line 30 p1. In sum, previous approaches are not very fruitful. Fruitful for what purpose?
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?
c) Line 34 p2. Very unclear sentences, requires rewriting
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.
e) Line 40 p2. Section 4 is not introduced. 1 or 2 sentences regarding this section need to be added.
f) Line 46 p2. “This local intensity occurs”, do you mean a local extreme associated to an event?
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).

h) Line 68 p3. A sentence explaining what is a copula is required.
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.
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.
k) Line 94 p4. It is unclear what is the scaling factor S.
l) Eq.9 p4. T_{CS} is not introduced.
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).
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.
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.
p) Line 122 p5. What do you mean by “pure phenomenon in the geographical space”?
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.
r) Line 131 p5. “Usually, level 5% is used; however”. There is a problem with this sentence.
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.
t) Line 142 p5. Should it be figure 2b? and 2a line 140?
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?
v) Line 181 p7. What is this ^{0} doing here?
w) Line 194 p7. Not clear what the conclusions of the section are, and how one should interpretate Figure 4.
x) Line 219 p9. 141 stations over how many in total?
y) Line 220-223 p9. I don’t understand these sentences.
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.
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.
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.
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).
dd) Line 314 p13. If it is once again please provide supporting references.
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.

ff) Line 323 p14. “simplicity and clarity”. It is not simple and clear at the moment.
gg) Line 368 p15. A kind of? Really?

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.

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