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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "On the relevance of extremal dependence for spatial statistical modelling of natural hazards" *by* Laura C. Dawkins and David B. Stephenson

Anonymous Referee #3

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The study investigates whether the commonly-made assumption of asymptotic extremal dependence in windstorm footprint between geographically remote locations is supported by data and if not, what a suitable alternative approach may be. This is a very important question, and the introduction motivates in a clear and concise manner the relevance of the study. The rest of the paper is also generally well-written and clearly structured. As such, form-wise it would be suitable for publication after some very minor revisions. I have more serious concerns regarding the substance of the study: in particular, the basis for the repeated claims of novelty in the introduction, a seeming lack of contextualization relative to the very broad field of extremal dependence modelling and the fact that the whole analysis is centred on two case studies. I

detail these and some other minor points below.

Main Comments

1. a. The authors repeatedly claim the novelty of their approach (ll. 63, 100, 103). As they implicitly note on ll. 63, the main novelty lies in the combination of existing modelling approaches rather than in some fundamental statistical advance. However, conceptually very similar approaches for investigating the appropriate dependence class for spatially remote geophysical extreme events have been implemented before, within a more comprehensive theoretical framework (for example, see Keresztúri et al. 2016). Other than being applied to a different variable, what broad additional insights does the present study provide?

b. On a related note, the authors suggest that an important result of their work will be to simplify the development and use of models that correctly represent extremal dependence for the variable of interest, removing the need to apply more complex – but more flexible – models which account for the different possible dependence classes (ll. 91-95). There are a number of these models available, including those of Wadsworth et al. (2017) and Huser and Wadsworth (2018). The actual benefits of the approach proposed by the authors are not explicitly described in the manuscript. Are the authors suggesting that the final result stemming from their approach outperforms these models (or that the results are comparable but require less work?) If so a comparison should be provided. Or that the reduction in computational time is so large as to make a difference in practical applications (if so, some indicative figures should be provided)? Or that the ease of implementation of their approach makes it applicable to datasets where other models couldn't be applied? Again, some examples should be provided and the extent/range of validity of this advantage should be discussed. Any one of the above points would be a sound motivation for the present work, but they would need to be explicitly stated and factually supported.

c. As a final note, very little is said in the introduction of the above-mentioned models which account for a broad range of dependency classes (see also references in Huser et al., 2017). There is a growing literature in this subfield, which should be discussed.

With the above I don't suggest that the work of the authors is devoid of interest, but they should certainly explain more clearly what the real novelty of the study and what the advantages it will provide to the community are. In my view, it will not be sufficient to alter one or two sentences in the manuscript: this will require a substantial clarification and contextualization effort, and likely some additional analysis to support the claims made.

2. My second major concern regarding this study is the fact that the results are presented only for two location pairs (with one location common to both). The authors briefly mention the fact that they have tested their results for other locations (ll. 250-252), but this is not substantiated in any meaningful way. Is there a way to systematically test the robustness of the results obtained by the authors across a western European domain, perhaps presenting the results in a form similar to Fig. 7 but for different reference locations or a 2-D version of Fig. 8 showing location on one axis, conditional joint loss on the other and density as colours/contours?

Additional Comments

3. The title suggests a very broad relevance of the paper. Even though the techniques discussed in the study are general, the analysis effectively focusses on windstorms at three specific locations. As such, the current title is misleading and should be changed to reflect the contents of the study. Alternatively, the approach proposed by the authors should be applied to other geophysical variables and geographical domains.

4. l. 124: Please include a reference for how the wind gusts are calculated. This parametrisation is very simple. If it works well, simplicity is obviously good, but a brief discussion of its performance versus alternative approaches should be provided.

5. Section 3.4: Are wind gust speeds really independent Gaussian processes? Can

this be tested on the data available to the authors?

6. Fig. 1. The labels/city names are very difficult to see in print.

References

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