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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 #2

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General Comments

The authors explore the joint spatial structure of extreme windstorms and the investigation is of good quality and described very clearly in the manuscript. I ask for a revision to address some outstanding issues, before publication.

Specific Comments

1. Observational errors

The analysis is performed on mean winds from a numerical model which are post-processed to gusts using a highly simplified model given on Line 124. These estimates

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of gusts will differ considerably from the true storm-max gusts experienced at sites and the influence of this error on results has potential to be significant. Therefore, errors in estimated gusts need to be quantified, and their impacts on results should be measured and presented to readers.

A comparison of MetUM-derived estimates with weather station data would provide a realistic measure of observational uncertainty. I suggest the rms difference in max storm gust between the authors' dataset (grid-cell encompassing Heathrow) and observed gusts for Heathrow is computed using the top N storm max gusts *observed* at Heathrow, where N is approx. 50 to focus on tail extremes. GSOD is a free source of observed weather for many stations, including Heathrow.

The confidence intervals in Figure 4 of original manuscript are based on sampling error and need replaced to include these estimates of observational error for each MetUM storm max gust. Figures 3 and 5b would also benefit from the inclusion of estimates of uncertainty in plotted values, due to both observational and sampling errors.

2. Events analysed in Figure 4, and interpretation of results

Fig 2 indicates approx. 25 points above quantile=0.99, which suggests that the quantile=0.5 in Figure 4 is based on over one thousand storm events in a 35 year period. The inclusion of about 30 events per year on average will contain many breezy days. These data points are potentially misleading to include, because the spatial structure of days with weak winds is likely to be substantially different from the spatial structure of severe events producing tail winds. I request that Figure 4 is re-drawn using data from quantile=0.9 and upwards. This would still include weak winds from minor cyclones, but is a step in the right direction, while maintaining sufficient sample sizes.

The conclusions to be drawn from Figure 4a should be reviewed in a revised version of manuscript. First, the results in Figure 4a indicate rising values of the coefficient of tail dependence for quantile thresholds above 0.9, towards a value of unity for the highest quantile. Given the aim to capture behaviour in the limit as p tends to 0, it seems unsafe to conclude that London-Amsterdam has tail independence. Second, the inclusion of observational uncertainty (point 1 above) will broaden the confidence limits which may require a new interpretation of results.

3. Section 3.4 conjecture

The conjecture to explain the tail independence in section 3.4 begins by representing storm winds as isotropic turbulence. It is standard to represent storm winds as the sum of a mean wind and a smaller turbulent contribution. This is also consistent with the MetUM model gust dataset used by the authors (description around Line 124).

The authors then assume that gusts at two locations are bi-variate normal. While the turbulent contribution to winds at two distinct locations might be bi-variate normal at any instant in time, the gusts analysed for tail dependence are the maximum gust over the whole storm. The storm-max gusts between neighbouring locations are expected to have strong tail dependence since they would have very similar mean wind and max gustiness from isotropic turbulence.

Regarding the text on Lines 254-258: McNeil et al. (2005) showed that if correlation is less than one, then the coefficient of upper tail dependence equals zero, their Example 5.32.

McNeil, A J, Frey, R, and Embrechts, P: Quantitative Risk Management Concepts, Techniques, Tools. Princeton University Press, 2005

4. Section 4 on losses

Lines 277-283: the authors state a simple loss function provides better storm loss estimates than the Klawa and Ulbrich (KU) loss function. There are various reasons why this judgement on loss functions is misleading. Besides the minor fact that the two articles quoted excluded population weighting hence did not test the KU loss function, there is a more significant issue that ‘better’ is defined in non-standard and highly specific terms as ‘a subset of 23 significant storms completely contained in highest quantile of all storms'. Further, there is much published work on how loss severity is a function of wind speed, and KU's loss function certainly captures this effect more accurately than a step function.

The 'conceptual loss function' used by the authors could be more accurately described as areal frequency of loss occurrence, and ignoring loss severity. Its usefulness in estimating total loss is an interesting result, since it suggests the area of storm above a loss-causing threshold is the dominant contributor to total storm loss. I suggest the authors describe their loss function in more specific terms as 'areal frequency of loss' in the text.

Further, if the authors wish to retain text comparing a step function to the superior KU loss function, then the authors should include more information for readers: errors in loss estimates depend on wind speeds, loss functions and exposure density, and the success of the simplest loss function over KU in tests performed by Roberts et al. is very likely due to its relative insensitivity to errors in other components of their loss estimates, such as estimated gusts. This helps resolve the dilemma of a rapid growth of loss with windspeed indicating KU, while a less sophisticated testing framework indicated a step function.

The over-estimation of joint loss probabilities in the maps in Figures 7e & f are explained as a mis-specification of asymptotic dependence (lines 308-309). However, it could be due to a too high estimate of the dependence parameter r. Could the authors include in the text the group of data used for estimating the dependence parameter?

Technical Corrections

There are many instances of 'apposing' when 'opposing' may be more appropriate?

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