Collective risk modelling for understanding the correlation between multi-peril accumulated losses
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This paper presents and explores three proposed risk frameworks for quantifying co-occurring risk, particularly with a focus on European windstorms. An interesting and necessary investigation into how correlation is measured and interpreted. A motivation for this is a discrepancy between the findings of past work, generally indicating a positive correlation between flooding and wind, and one of the authors' findings of a negative correlation for seasonally aggregated losses. Whilst the work appears sound, I recommend moderate revisions to increases the accessibility and communicative power of the work.

I have two specific suggestions, and a range of other comments below which are primarily aimed at increasing the transparency of the work and ability to link it to related papers.

1. Please concisely expand the context provided to give appropriate credit to past work, which is thin in some places (e.g. Sections 2.1 & 2.2).
2. To increase the readability of the work and make it accessible to a wider audience, I think that an additional Figure panel is needed to illustrate the data underlying the negative correlation i.e. a scatter plot showing the ASI data points for a particular (x,y) location, the two thresholds from Fig2 a-c, different colours for dots contributing to Fig. 2a-c at the location. Consider adding a trend line for the correlation(s). And, also mark this location on Fig2 a-c. This might be a second panel in Fig. 4.

A large caveat to this review is that my undergraduate training is neither as a mathematician nor statistician, and as such that I cannot guarantee an in-depth critique of the technical part of this paper (i.e. the derivation in Appendix A).

Comments on the manuscript

Abstract

L7 – Here, please make it clear that the assumption is of the same number of events for each hazard. Why might this be appropriate for extreme wind, precipitation and flooding - particularly when they exceed a threshold? This can be discussed later. The number of impactful flooding and wind events expected in the UK are, for instance, expected to differ (e.g. see Hillier et al 2024 - doi:10.5194/gc-7-195-2024). If I have mis-understood, please use this opportunity to make the paper more widely accessible i.e. is this handled by the distribution shapes of X and Y.

L14 – Please fix the grammar here. Perhaps, '… high thresholds'.

L57 – Is it important that the metrics used for 'correlation' in this paragraph are mentioned, as the difference between how these measures change in the same (or different ways) is an important to the interpretation of the models in this paper in the context of previous work. To illustrate that metric matters, it is perfectly possible for spearman's correlation to decrease as tail impact increases (i.e. see material at https://doi.org/10.5281/zenodo.12533412, Q11&12 on Task 2). Hillier et al (2025) – 10.1002/JOC.8763 – have also quantified co-occurrence and use Uplift.

L57 – Another detail that needs to be noted to allow the reader to compare this work to previous work is how annual/seasonal are defined i.e. Oct-Mar or similar in previous work, and Jan-Dec here, effectively taking part of two storm seasons.

L70 – Applying a correlation between hazard variables in order to understand potential losses is not, as far as I am aware common. It is done in one context by Hillier (2025). Please add references to put this choice of approach in context, particularly if they've been missed by Hillier (2025).

Models

L77 – The phrase '… following Jones et al (2024) ….' implies that Jones is the originator of this approach of SI over a threshold. Please add references and rephrase to better reflect the origin of the approach.

L79 – Similarly, the 20 ms^{-1} threshold is widely used and sensitivity tested (e.g. in Hillier & Dixon, 2020) although it long predates this. Please add references and rephrase to better reflect the origin of the approach.

L83 – Similarly, for Section 2.2 concisely add some background for the use of aggregate SIs for wind and cyclones.

L105 – Fig. 1 is very helpful to improve accessibility. Thank you.

L115 – Please clarify in words whether Z modulates X and Y in the same way (i.e. up or down), or whether it is possible for Z to affect X and Y differently.

Application

L160 – Following, or replicated from? Please clarify to make it clear whether Figure 2 is new work.

L163 – Please add a sentence here on whether or not the spatial pattern of high and low correlations match previous work (e.g. Martius, 2016; Hillier & Dixon, 2020). If there is a match, it will then be clear to the reader that there is agreement spatially, with the primary debate about the magnitude & sign of the correlation.

L193 – Red box for France. Is this 5° from the central point? If not, it would be better for comparison and consistency across the manuscript to make this match the approach on L157, and clarify there that it's a box not a radius.

L206 – This jet stream connection was first published in Hillier & Dixon (2020) - Dixon's idea. Please consider add the reference to this point.

L235 – Fix brackets for references.

L251 – To link this work into that in the introduction, it is probably worth showing the Oct-Mar results in Supplementary Material so that readers can decide how to link this to earlier work.

Figures

Figure 2 – On a-c, please add stippling or similar to indicate areas where the observations have statistical significance (i.e. can be safely interpreted as existing). And, in the caption, decide if units are in italics or not.

Figure 4 – Add error bars, or similar, to the sample correlation curve please.

Figure 5 – The two sets of numbers on the scale bar reduce clarity. Please find another solution. However, I like this figure; interesting similarities to Fig. 2g of Hillier & Dixon (2020) showing different wind directions for 'windy' and 'wet'.

Figure 6 – Please add stippling or similar to indicate where the correlations are statistically significant (or not).