Response to Anonymous Referee #3

We would like to thank the reviewer for their helpful comments on our manuscript. Below we list how we've addressed each of their comments (in blue italics).

(Methodology) I think this section would benefit with some clarification and simplification. The description of the approach is quite mathematical and may be difficult for some readers of the journal to follow. Given the potential value of this method as a tool for the wider community, simplifying certain explanations could encourage broader uptake. These are mostly suggestions, though there are places where I feel I could not replicate without making assumptions. I leave it to the authors to decide whether these changes would strengthen the text.

Thank you for this helpful comment. We have now added more descriptive detail around the assumptions (L128-133), this reads:

The distinction between the frameworks are their choice of independence assumptions:

– Frequency-Severity Independence (FS-Ind): The severity of hazards within a year are independent to the frequency of events (e.g. storm counts and gust speeds for a year have zero correlation).

– Hazard Independence (H-Ind): The wind and precipitation SIs from the same event are assumed independent (so have zero correlation).

– Serial Independence (S-Ind): Hazard SIs from different events are assumed independent (wind values from separate events have zero correlation with each other).

P5 L108-111, the subscript j is introduced without explanation. Furthermore, why use Cov() when var(X) is used later on L126? Although they are interchangeable, using only one would be consistent and enhance readability.

We have acknowledged the *j* index on L138:

As indices i and j take any value from 1 to N , dependency between all hazard pairs within a year is considered.

By definition, variance and covariance are not interchangeable. However the variance is a special case of covariance, (Cov(X,X)=Var(X)). For ease of interpretation we will continue to use both variance and covariance.

P5, L113-114: For the correlation , should this not be ? Likewise for P5, L131. I perceive as the correlation of one pair of X and Y values which is obviously not the case. I assume that the correlation is calculated between all X and Y values? Furthermore, it would be helpful to to use , to indicate individual events where appropriate, and X and Y to indicate the random variables.

Correlation (θ) is calculated for all X and Y values, which is now clarified on L144-145.

P5, L114: Which measure of correlation is used?

Updated to clarify that Pearson product moment correlation was used, L144.

P5, L125: Please clarify how and are calculated. I assume it is via the same equations on P6, L138 without conditioning on Z.

The original manuscript defined J_X and J_Y on L126 (now L159). The appendix has been updated to include the E[X] and Var(X) paramter estimates which do not condition on *Z* (L353-363).

P6, L136-139: I suggest providing some plain explanation on what these terms represent.

In the original manuscript this is provided later on L180-184 (in 3.3 Analysis of components in Framework C). It is now L223-228. Explanations were not given at this this point in the manuscript as it does not make sense to consider framework components when we do not know how the framework performs. We have signposted that these terms are explained later (L173):

Physical explanation of these three components is provided in Section 3.3.

Minor comments:

P6 L146-148: Could you clarify what you mean by 'smaller scale features'? ERA5 is unlikely to reproduce certain features such as sting jets and does not resolve convective rainfall, which are the features I would consider smaller scale. Also, I would double check the cited papers here as I don't see a mention of ERA5 within them, and one paper predates ERA5.

Yes, sting jets are not resolved in ERA5, this has been removed. This sentence was highlighting the use of high resolution hourly data will achieve better results than use of daily data. It has been reworded to say this and not imply that the references use ERA5 data when they don't (e.g. Whitford et al. (2023)). The sentences (L180-182) now read:

"The cyclones and hazard variables are extracted from 1-hourly ERA5 reanalysis at the native 0.25° spatial resolution from1980-2020 (Hersbach et al., 2020). Using hourly data is important for modelling sub-daily rainfall extremes (Whitford et al., 2023)."

P6 L156: Do you mean the maximum 3-second wind gust instead of the '3-second maximum'?

Yes, this has now been changed (L190)

P6 L160: Please clarify which measure of correlation is used.

Clarified Pearson's product moment correlation is used. (L196)

P6 L163: Are the thresholds applied to the event metrics and not the hourly?

Yes, this is now clarified in Section 3.1 (L192).

Figure 2: Could you include the motivation for using absolute thresholds instead of percentile thresholds? Also, you mention in the conclusions that the results are insensitive to this choice (P13 L246-247), but it would be informative to provide a map in supplementary showing what

percentile the absolute thresholds here fall under. I can't tell if 10mm or 20mm in a storm is extreme or not.

Fixed thresholds were used as this paper builds on results of Jones et al. (2024) where fixed thresholds are used. We have added a figure to the Appendix showing the percentiles that ($10ms^{-1}$,10mm) and ($20ms^{-1}$,20mm) fall under (this figure is shown below). This motivation is now explained on L197.

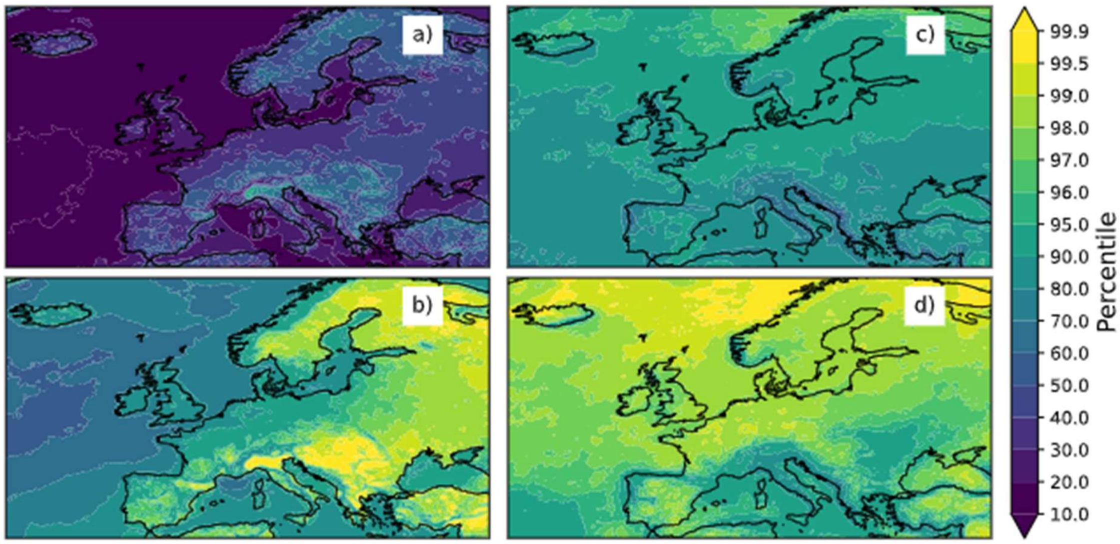

Figure 1: Equivalent percentiles for wind gusts (a & b) and precipitation (c & d) for thresholds u=10 (a \& c) and u=20 (b & d).

P8 L176: Framework C Indicates the presence of a distinct land-sea contrast in the correlations for joint exceedance of 20 ms-1 and 20mm, while there are indications of this in the sample correlation. Can you comment on why we see the land-sea contrast and why the sample correlations are noisier?

The land-sea contrast is primarily caused by the fact that wind speeds are generally greater over sea (reduced surface roughness) and so wind exceedances over a fixed threshold over sea are less in the extreme tail of the distribution than those over land. The negative correlation emerges over sea at higher thresholds (e.g. above 40m/s for the Azores in Fig. 6 of Jones et al. 2024). This is now mentioned in Section 3.2 (L204-206). We suspect that the sample correlations look noisier because their values are slightly closer to zero over land.

P8 L180: Could you clarify what is meant by 'simultaneous correlation'? Do you mean the local correlation each grid cell?

This has been removed and redefined as: "the 'average' of yearly wind-precipitation correlation, computed between hazard pairs occurring from the same storm." (L227)

P8 L187-188: Should 3g and 3i be 3d and 3f?

Yes, this has now been changed. (L236)

P8 L197-198: I think there is a typo here:"The positive within year dependency component (solid thin line) is largely compensated at all temperature thresholds by the negative within year dependency component", i.e. "within year dependency component" is repeated twice.

> Yes, thank you. This has been changed to "*The positive event dispersion component (solid thin line)…*" (L247)

Figure 4 and 5:

The negative within-year correlations (dashed line) appear much stronger over this area of France than the grid-cell correlations shown in Figure 3d–f, which are close to zero across the domain. Could you comment on why this might be the case? Does the larger domain introduce spatial correlations between wind and rainfall? This is an interesting result and may relate to the findings of Manning et al. (2024), already cited here, given the cancelling effect on the dispersion component. For example, one might expect a negative spatial correlation on an event basis, as the highest rainfall typically occurs to the northeast of a cyclone centre, while the strongest winds occur to the south. This spatial separation could explain the cancelling influence on N.

> As SIs are aggregated over a larger domain there is greater spatial distribution of hazards for the France region. For the 98th percentile, storms are typically windier in the north west and wetter in the south east (shown in Figure 2c & e). At higher thresholds the SI for the region tends to be extreme for just one hazard, giving negative correlation.
>
> Figure 3 shows splitting the France region into north and south regions. Figures 4 and 5 are the same as Figure 4 in the manuscript but recalculated on these regions. As expected the value of the within-year correlations is lower when splitting the regions, but more negative for the northern region.

You see a similar effect of the relative positioning of wind and rainfall extremes in cyclones in Figures 5b and 5c. Most wind-event tracks pass to the north of the domain, exposing a larger portion of the domain to the part of the cyclone that generates strong winds. Conversely, rainfall-extreme tracks tend to lie farther south, meaning a greater part of the domain overlaps with the cyclone sector producing heavy rainfall. See Figure 3 in Manning et al. (2024) and related discussion that shows similar results.

Overall, I think the manuscript would benefit from further discussion linking the statistical results to the underlying physical processes, as well as examining the sensitivity of the findings to domain size (e.g., grid cells in Figure 3 versus the larger domains in Figures 4 and 5). The paper presents interesting results on the propagation speed of systems, but I believe there are further nuances to discuss, such as storm positioning, as noted above. It would also be valuable to extend the discussion to consider the influence of the jet stream, which is a dominant driver of propagation speed.

> The manuscript has been updated to comment on the sensitivity of results relative to domain size (L251-254). We have expanded the discussion to highlight similar results found in Manning et al. (2024) and Owen (2022) (L281-286). We feel discussion regarding the jet stream is beyond the scope of this study, although have highlighted Hillier et al. (2025) on L278.

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Title: I suggest amending the title to highlight its application to wind and rainfall extremes. While the paper presents an excellent tool, it also makes a valuable contribution to the understanding of multivariate wind and rainfall extremes which might otherwise be overlooked.

The title has been updated to "Collective risk modelling of multi-peril severities: the correlation between European windstorm annual accumulated gust speed and precipitation exceedances".

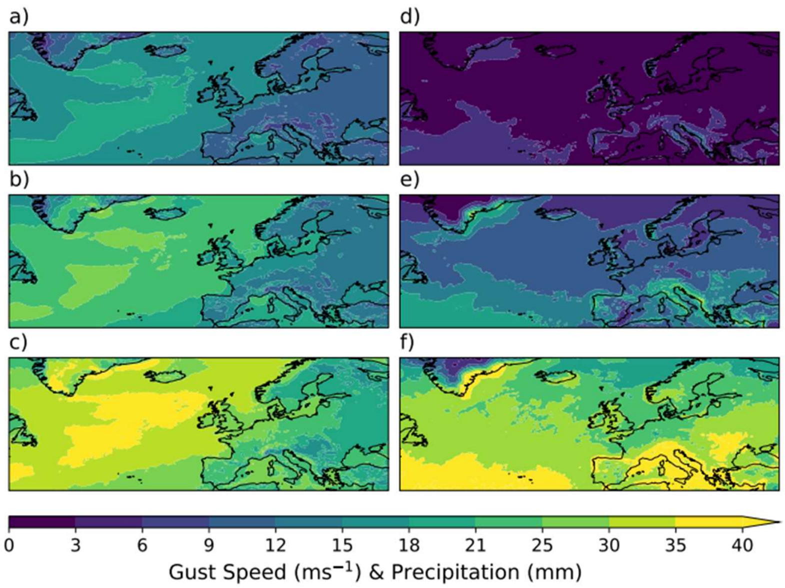

Figure 2 – Percentile values for wind gusts (a-c) and precipitation (d-f) for 0th (a&d), 80th (b &e) and 98th (c & f) percentile thresholds.

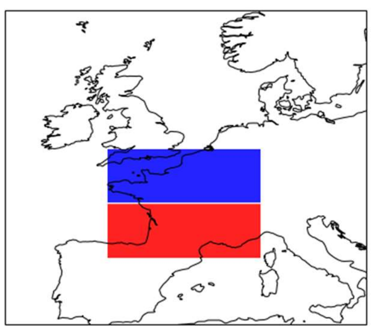

Figure 3 - North (blue) and South (red) France regions used in Figures 3 & 4.

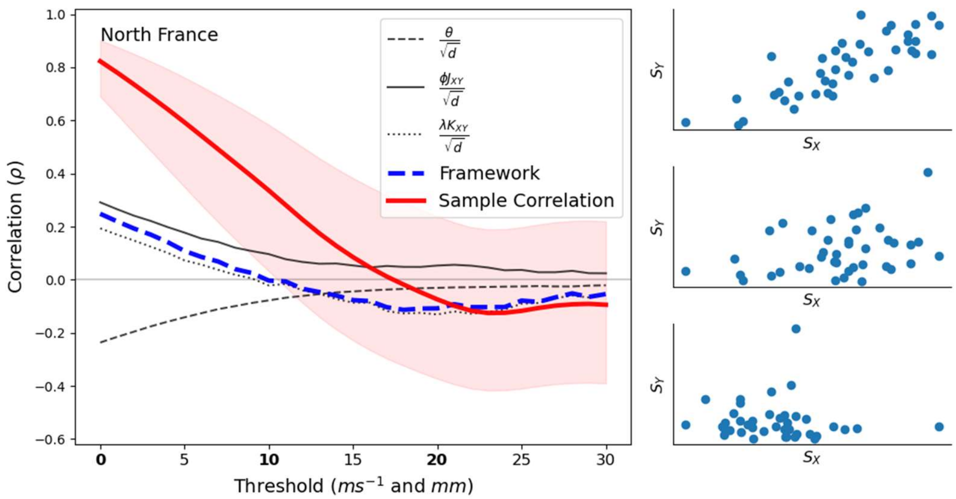

Figure 4 - Same as the article's Figure 4 but computed for just the Northern half of the France region (shown in Figure 2 above)

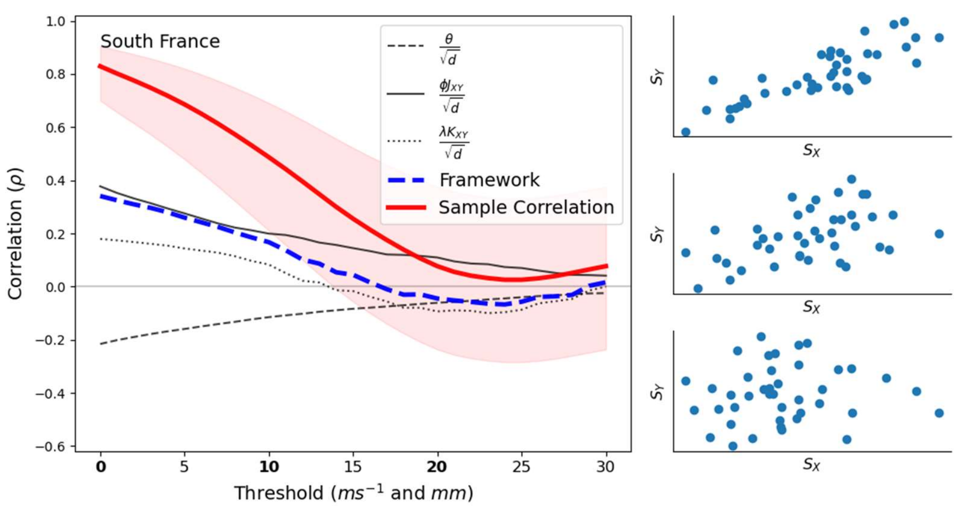

Figure 5 - Same as the article's Figure 4 but computed for just the Southern half of the France region (shown in Figure 2 above)