Response to Anonymous Referee #1

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).

I was intrigued by the title of the paper which states that it investigates multi-peril accumulated losses. Therefore, I was surprised to find that there is in fact, no loss data used in the paper. The paper only looks at the 'severity' purely based on ERA5 data of wind and precipitation. 'Losses' may cause the reader to expect a more quantifiable impact, such as damages, or have some sort of vulnerability or exposure included in the loss function. I understand that the authors is using severity as a proxy for loss, however, the hazard already seems to be a proxy of the severity through the use of a function. Perhaps the authors can consider to change the word losses to severity instead, in order to avoid further confusion.

This is a good point to make and so we have replaced the word "losses" to "severity" in the article title. The title is now:

"Collective risk modelling of multi-peril events: correlation of European windstorm gust and precipitation annual severity"

In Line 173, the authors mention that "negative correlation over the northwest of mainland Europe is correctly captured" and only framework C is able to capture the correlation at each threshold. It seems that it is 'correct' because it matches the sample correlation? What makes the sample correlation from Jones et al. 2024 'correct'?

We are aiming for the framework to capture the broad spatial features in the sample correlation (shown in Jones et al. (2024)) rather than reproduce exactly the grid point values which are prone to sampling uncertainty. Our use of the word "correctly" is therefore confusing. L173 (now L216) has now been changed to read:

"The spatial structure of negative correlation over the northwest of mainland Europe is broadly reproduced at the highest thresholds (Figure 2k)"

I wonder why only data from 1980-2000 has been investigated, while the data is available to the present. The authors state that data prior to 1980 has not been included due to data quality, however, this should not be an issue for recent data. Does the cutoff in 2000 mean that the current day climate is not reflected in the results?

Sorry – this was a typographical error in the abstract that has now been corrected to "1980-2020".

The introduction of the paper reads very well and highlights the general need for the proposed research. I find the findings related to the negative correlations and the difference between storms with a short and long duration interesting. However, as a reader I am left wondering exactly why these findings are important. Who is it relevant for? How may these results improve our multi-peril risk management? Additionally, a negative correlation between wind and rain can already be deducted by looking at Figure 5, which provides enough insights for the relationship between hazard intensity and duration as well. Can the authors explain the need of extensively testing the three frameworks that are presented as the main focus of the paper?

It is relevant to risk managers such as catastrophe reinsurers who could in principle exploit the negative correlation (if it exists at a portfolio level between wind and flood losses) to help diversify their risk. For example, they could try to balance windstorm cover with flood insurance cover in countries such as France.

Figure 5a only shows SI for individual events and this, as framework C shows, is not necessarily sufficient to guarantee negative correlation in annual "aggregate" SI across the year (positive correlation from clustering could cancel it). The frameworks are important for understanding the causes of the resulting correlation in ASI.

These explanations have now been added to Section 1 (L55) and Section 3.2 (L198) of the manuscript.