Egusphere-2024-2079

Concurrent modes of climate variability linked to spatially compounding wind and precipitation extremes in the Northern Hemisphere

Francois *et al*

Comments to the authors

The paper assesses wintertime, northern hemisphere modulation of the frequency of joint precipitation and wind events by four modes of variability. ENSO is found to be the most influential mode, combinations of modes are asserted to essential for the co-occurrence. A doubling of the area affected by joint events is highlighted in the abstract. The analysis is probably sound although it is not currently possible to be certain, and it is currently not clear whether or not it is novel work as the Abstract and Introduction do not convincingly define the research gap being filled. Yet, it is entirely plausible that the gap is real. The manuscript would also benefit from substantive rewriting to improve its clarity.

In short, I feel this has the potential to be an interesting paper if it is very significantly re-written, simplified and/or focussed and clarified.

Major concerns

1. As written, the abstract does not acknowledge any previous work linking joint extremes of precipitation and wind to modes such as ENSO. Linking extremes to ENSO and similar has been a common thing to do for at least 40 years, singularly and jointly. The abstract should be re-written to clearly express/define the research gap with respect to the existing literature. Also note that the use of multiple modes to understand the occurrence of extremes, particularly hurricanes, has a large literature (e.g. Vecchi, 2014). In short, by the end of the Introduction, it is still unclear what the novelty of the paper is (i.e. what is being done that has not been done before). To be clear, there could be a gap (my knowledge of the literature is not comprehensive) but the authors have not convincingly stated what it is. Some papers that might be relevant in the re-framing are:

    - Khouakhi (2017) *Contribution of tropical cyclones to rainfall at global scale*. i.e. ENSO and extreme rainfall.
    - Bloomfield (2024) *Synoptic conditions conducive for compound wind-flood events in Great Britain in present and future climates*. Rain/flood and wind compound and NAO.
    - Hillier (2020) [already cited] *Multi-hazard dependencies can increase and decrease risk*. Flooding and wind compounding and NAO.
    - Vecchi (2014) *On the seasonal forecasting of regional tropical cyclone activity*. Introduction contains literature on use of multiple modes.

2. As certain key pieces of information are not prominent early on, the manuscript (up to Results) can currently only be understood upon re-reading. Please rectify this.
3. Throughout, more care should be taken to ground the work in related literature. Some parts are well referenced, but others are lacking (illustrative examples below).
4. The manuscript lacks a substantive discussion, e.g. about issues relating to the key findings of the paper. Perhaps, this will become clearer when the research gap and focus of the paper is more clearly defined. Perhaps separating the material in the Results into a descriptive (Results) and explanatory (Discussion) parts would help, and clarify the themes the authors wish to discuss.

Detailed Comments

Abstract:

L11 – The phrase '*combinations of modes are essential for the occurrence*' is problematic. They might be necessary for a better description of the co-occurrence from this particular statistical viewpoint, but they are large-scale indicators of conditions, not processes that drive co-occurrence or not. So, please rephrase to a more precise statement.

Introduction:

L25 – ‘co-occurring compound’ – this is tautology, remove one word.

L26 - (Jeong et al., 2020), this is not the only reference for this. Add ‘e.g.’ or/and cite another couple.

L44-45 (and throughout the manuscript) – Again, please use ‘e.g.’ when various papers could be cited. The manuscript should be improved by pairing an older reference with each of the recent singular references used. For example, convective storms were known to produce multiple hazards long before Dowdy & Catto in 2017.

L56 – Why is an AMV abbreviation used here, but is the only mode in the abstract that is not abbreviated. Please be consistent.

Methods:

L91: Daily data used. The focus on this timeframe should be made prominently and clearly (e.g. in the Abstract). [Returning to this, I suggest my comment also highlights that the explanation needs to be clarified]

L98-104: This is unclear, I’m afraid (i.e. not reproducible). E.g. is NAO the mean of DJF, and if so, which days is it applied to – the Jan-Dec year? Please clarify.

L106: First explanation that winter is defined here as DJF. This is a key piece of information, and should be prominent (e.g. in the Abstract). Also, a brief explanation of why DJF is selected is needed.

L106: First indication that this analysis is based on seasonal counts. Again, this should be prominent, because not knowing it leads to a need to re-read the sections above. Please fix this to improve the readability of the manuscript.

L109: Why the 98th percentile?

L111: ‘more robust evaluation’ – this needs to be more specific please.

L115: It would be beneficial to set the choice of this metric (i.e. a count-based approach e.g. Hillier 2015; Bevaqua 2021 – Guidelines paper; Owen et al 2021 in Weather & Climate Extremes, χ) in the context of the metrics/timescales in previous work (e.g. Hillier & Dixon, 2020; Bloomfield, 2023 in Weather & Climate Extremes)

L119: Two other metrics are ‘introduced’. Although I cannot recall exact papers, I struggle to believe this is the first time these sorts of metrics have been used. Again, please place in context of similar metrics and usages with a few references.

L139: ‘some confounding effect may remain’. This is a rather important statement. It is good that the authors acknowledge it, however the key question is: How much, and does this impact the key results of the paper? Either by testing with simulated/idealised data, or perhaps another statistical method, I believe that the authors need to answer this question.

L151-152: Scope limiting statement. Fair enough, but I believe this needs prominence in the paper, and am hoping it’ll be so in the Discussion at least. It might also need to be in the abstract as it is a potential bias on all conclusions drawn, and so should be prominent for clarity.

Section 2.2.4 – This approach seems reasonable, and statistically significance testing is critical in a paper like this, although I’d need to do a really careful read in a revised manuscript. Illustratively, the permutation procedure would need to account for dependency / relationships between the modes, or statistical significance of any results could be over-estimated (i.e. appear significant when they are not). And, whether this has been done is not currently clear to me.

Results:

L211: biases w.r.t ERA5. Fair enough, although I expect any relevant ones to be explicitly referred back to and results interpreted in light of this during the Discussion.

Section 3.1 & 3.2: From a reader's point of view, it would be nice if this were significantly shorter, drawing out the main points of interest (i.e. that are new).

Please review Section 3.1 as in a number of places it starts to discuss / explain the results to a level that is at or above the limit expected in a Results section.

L257 – 'we move to discussing' Please do not move to discussing in the results section. Please discuss in the Discussion.

L315-326 – This seems like an expansion of or repeat of Methods. Consider moving to methods.

L336-7 – This long-distance correlation is interesting. It is an example of the type of thing that could be expanded upon and discussed in a Discussion.

L340 – '*we find that dependencies among regions overall enhance the potential for spatially compounding events'* I am unsure how you can make this conclusion given that you were explicit earlier about only looking at enhancement not reduction of co-occurrence. Surely, both need to be looked at to comment on an overall effect.

L341 – This, and similar mentions of methodology in Results, should be put into Methods please.

L366 – '*causal links among climate variability modes and oceanic modes exist*'

Discussion:

L425-436 These are assertions, picking highlights from the results. These results are not discussed, i.e. reflected upon and put in the context of the literature. Suggest removing, or including in the Results.

L437 – 445: Is a justification of the Methods, which I think is a repeat from the Methods section. Remove.

L456 – This paragraph is a restatement of the approach, until L456 where an alignment with existing results is stated. So, it would be good to clarify what the new insights provided by this paper are.

L461 – This paragraph is a caveat, which is OK, but should come after a substantive discussion.

Conclusions:

The conclusions are suitable in style, but are difficult to comment while the assertions being made have not previously been discussed.

Fig. 6 – It's good to see the Bonferroni correction being used.