Author response to reviews of "North Atlantic seasonal climate variability significantly modulates extreme winter Euro-Atlantic extratropical cyclone hazards" by Maycock et al., submitted to NHESS

We thank the Editor for sourcing three detailed reviews of our study. We thank the reviewers for their time in providing constructive comments to improve the manuscript. We have taken on board many of the suggestions. We respond to the specific points raised in blue.

Review by Lisa Degenhardt

Summary: This paper is investigating meteorological parameters (wind, precipitation and wave swell) that are caused by European tropical cyclones and how whey are influenced by atmospheric variability patterns as NAO or EA. Beside analysing a regression analysis for each variable they also look into compound events. Even though their compound is defined as extreme variables happening in the same season at the same location rather than at the same accurate time. This study is fully based on ERA5, meaning the result can be seen not as prediction skill but rather as atmospheric conditions.

This paper is a very interesting study with impact-based data. The results are important for different fields like insurances. This study fits well into the NHESS-journal. In the following I have only a few comments or questions but nothing too major.

We thank Dr Degenhardt for taking the time to provide a thorough review of the manuscript and for their supportive comments and constructive suggestions to improve the manuscript. We have taken on board their comments and respond to each point below.

Major Corrections:

Paragraph 2.3: You explain, that you use the maximum of each variable for each grid cell. I understand that this is giving very extreme values per season and I also see that neighbouring grid cells are not from the same storm. It think this is a fine way of doing it and it is well explained, but have you tried to use a seasonal average or sum per grid cell for your method? How are the results looking then? If you have tried it, maybe add a short paragraph, to show both sides.

We chose to look at the seasonal extreme hazards partly because these are more likely to be associated with impacts and because other studies have looked at seasonal averages or seasonal totals for hazards, so we wanted to distinguish our work from what has been shown before.

Paragraph 3.1 (e.g. L198): here you are talking about "increase" and "decrease". I don't fully understand how you see the difference between increase and decrease, when your scale is from 0-25%. I guess I don't fully understand the "percentage anomalies from the climatology". Maybe a short explanation or equation in the supplementary material would help. I tried to write down an equation, but I believe this is not fully correct as this could result in negative values as well:

x = 100∗(regression−climatology) / climatology

Thanks for spotting this mistake. The colour scales for the % anomalies in Fig 3 have been changed to include negative values, consistent with the sign of the regression slopes in Fig. S3.

Paragraph 4: I like that this is quite dense, but for me the Discussion-part is a bit missing. I think this is more a summary and conclusion. I would like to have a few more sentences about how the results relate to other studies.

We have added some sentences to discuss the limitations of the analysis and how it could be extended and improved in future work.

Fig. 5: Could you make more distinction with coloured here? Meaning for the "one var" panels, one colour for all grid cells where only wind is related, one colour for only precipitation, one colour for only wave swell and one colour, where not only one variable is related. Same for middle figures, one colour for grid cells that have wind-prec combination, one colour for wind-swell combination, one for prec-swell combination and one where more than one combination is related.

Thanks for this interesting suggestion. We have tried this and will include it in the revised manuscript.

Fig. 7: It took me quite a while to understand the red dots. Maybe this is because I am not an English native speaker and the word "unitary" is explaining it fully. If not, I would consider to make it more clear that the red dots are predictions with your regressions for the case of PC1/PC2 +1/+1. I believe this is the statement in L245, but I think I would include it in the figure caption as well and make it very clear in the text.

The Fig 7 caption has been amended to: “predicted by the linear regression model for ±1σ combinations of PC1/PC2 indices (four combinations shown by red dots in middle panel).”

Minor Corrections:

L63: You introduce a new abbreviation for East Atlantic Pattern here, even though you have introduced one before. You also mainly use EAP instead of EA. I would choose one. (same in L139)

Thanks for spotting this, we now use EAP throughout.

L100: Here you write "wave/swell height" but mostly it is "wave swell height" is there a difference? If not, I would use only one way.

Changed to “significant wave height” throughout for consistency with other studies.

L160: Shouldn't it be 2°W
Corrected

L182: Shouldn't it be "are shown as percentage"
Corrected

L185: We are in Section 3.1 here, do you mean something from section 2?
Thanks for spotting this. Corrected to Sect 2.4

L191: For clarifications, I usually like it, when there are more often annotations about which figure you are currently talking. Meaning "parts of western UK (see Fig. 3a right)". This would help in more situations throughout the manuscript (e.g. L260, L266, L273)
We have added more references to the relevant figure panels throughout the results section.

L214: This is just an idea, but "shaded" is for me the grey area. Maybe "coloured" fits better here?
Changed

Paragraph starting L213: To clarify: this means the compound does not has to be at the same time, only in the same season?

Yes, we do not distinguish whether the hazard occurs within the same cyclone. The rationale for this is that the timescale for societal recovery from an extreme hazard (e.g. severe windstorm or large flood) is likely to be longer than one season so two severe hazards happening within a season could be considered compound.

L246: Here you use the wording "+1 PC1", in L255 you use PC1-, I think I like the second version more. But in general, just use one if they mean the same. Or if my comment above is correct, make it more clear that the first version explains the red dots.

The first use explains the magnitude of the applied PC anomalies. We have added a σ symbol to make this clear and referred to the middle panel Fig. 7. The latter use is more general referring to the sign of the PC index anomaly so we have left this as in the original manuscript.

References: It looks like you use different citation styles. I believe this will be unified in the type setting, just so you are aware.
Thanks

Fig. 4: If I see it right, the maximum over all panels is roughly 70%. Have you tried to adjust the scale to 0-70%, to make the changes more clear?

We tried various combinations of colour bars and found this to be most suitable. We prefer to show the full range so people can put the values into the context of a perfect model (100% R^2).

Fig. 5: Maybe try white continent borders, especially for the "three var", the borders are hardly seen

Thanks for this great suggestion. We have changed this.

Fig. 5 ("three vars"): Maybe include the grey mask to cover the land masses as used before for wave swell, as in the three vars version the land is definitely excluded because of the wave swell, right?

Thanks for this great suggestion. We have added it.

Fig. 6: Why do you start to have the lon/lat at the wave swell now? I think you don't need them, as you didn't had them before neither (same in Fig. 7)

In all the figures one panel should show the lon/lat bounds to demarcate the plotting area. These were in Fig 2-4 but were missing in Fig 5 and have been added

Personal Note:

I am happy to see my paper (Degenhardt et al., 2023) as reference in your study, but I have a bit the feeling at some points, that you are not agreeing with our way of analysis. I just wanted to state, that we are aware of the difference between discrete and continuous data, but because we are using seasonal averages per grid cell and also model member means we hardly have any discrete data. We also agree that linear models have to be used with caution, but we verified our results with a more flexible regression (Poisson) and the results didn't change (also stated in the paper last paragraph of section 3.3).

We thank Dr Degenhardt for clarifying the methodological details of the Degenhardt et al. (2023) paper. However, we are not convinced this resolves all the issues discussed in the manuscript. For example, in their Fig. 5 linear regression patterns for ERA5 are shown. These are then used with model predicted modes of variability to predict cyclone hazards. Based on our understanding of what is shown in Degenhardt et al. (2023), the ERA5 regression maps shown in Fig. 5 are similar to what is shown in Fig S1 and Fig S2 in our manuscript, which exhibit the issues with performance of the linear model. We would be grateful if the reviewer can clarify the differences and why this issue would not also affect Fig.5 in Degenhardt et al. (2023).