

## **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. We apologise for the delay in submitting the revisions which was due to unexpected increases in workload of the corresponding author.*

### **Review by Mika Rantanen**

This paper investigates the relationship between large-scale climate modes and ETC-related hazards in Europe. The authors track cyclones, calculate their hazard footprints and then use linear regression to find how much the ETC-related extremes change with respect to PC1 (North Atlantic Oscillation) and PC2 (East Atlantic pattern). The key result is that PC1 or PC2 alone exhibit increases in hazards in relatively different geographical areas (i.e. PC1 in the east and north of the UK, but PC2 mainly in the west and south). In addition, there are areas which exhibit signals for several hazards at the same time, and areas where both PC1 and PC2 affect simultaneously.

I like the research idea and I think this is definitely something which is worth publishing in NHESS. The used datasets are appropriate for conducting this kind of study. I also liked that negative results (SDI) were mentioned.

However, I had some concerns related to how the key results are presented. I think this could have been done in a more explicit/quantitative way (see comment 1). In addition, I'm afraid that the daily precipitation associated with the ETCs might be overestimated (see comment 2). I hope that the authors could address these concerns before the publication of this study.

*We thank Dr Rantanen 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 are grateful for their constructive suggestions to improve the manuscript. We respond to their points below.*

### **Major comments:**

1. The presentation of the results. I don't know how to really formulate this, but I got the feeling that presenting the main results only in a rather qualitative way with Figs. 3-7 leaves the results a bit incomplete. Now you go through the regions rather subjectively (i.e. increase here, decrease there and so on). Could this be done in a more quantitative way, for example selecting beforehand relevant regions (domains) from Europe, e.g. countries or more wider regions such as Scandinavia, Western Europe, etc. And then calculate the regional statistics of how NAO and EAP affect the ETC hazards. These could be presented for example with boxplots which compare the climatology and then a unit increase of PC1/PC2. For example, the climatological daily ETC-precipitation in Scandinavia is this, but when NAO is positive, it's this, and so

on. This would provide more quantitative information on the regional distribution of the results. I hope you get the idea!

Thank you for this useful suggestion. We intended for Fig. 7 to display this information spatially, but we recognise it is difficult to extract detailed quantitative information from the maps. We have applied the 0.5 Mm<sup>2</sup> land regions defined by Stone et al. (2019) and have added a figure showing boxplots for the land-based hazards for these regions. For the ocean-based hazard we use the Marine Strategy Framework Directive (MSFD) European regional seas regions.

Stone, D.A. A hierarchical collection of political/economic regions for analysis of climate extremes. *Climatic Change* 155, 639–656 (2019).  
<https://doi.org/10.1007/s10584-019-02479-6>

MSFD regions: <https://www.eea.europa.eu/en/datahub/datahubitem-view/7144675c-5c84-456f-92e0-8f832239d880>

2. Precipitation footprints. I was not entirely convinced by the way the daily precipitation is assigned to the ETCs. Most importantly, you consider daily (24h) precipitation, but the passage of an ETC can last much less than 24 hours and can occur during two consecutive calendar days. For instance, if an area of ETC (i.e. the 10° circle) passes over a particular grid cell in 12 hours, say from 18 UTC to 6 UTC. What is the period used for calculating the daily precipitation that is attributed to the ETC? Is it a moving 24 hour window, i.e. the previous 24 hours after the passage, or some fixed time interval, like 00-00UTC? This can cause problems especially at the outer edges of the 10° circles, which are only briefly affected by the passage of the distant ETC, but the precipitation is still counted from a 24-hour duration, resulting in exaggerated ETC-related precipitation values. Or am I missing something here?

The precipitation is the 24h total for a calendar day and does not account for the timing of a cyclone passage which may span >1 day. The index is equivalent to the Rx1day wettest day metric applied to the cyclone related precipitation in the winter season. Rx1day is commonly used to assess precipitation extremes (e.g., Seneviratne et al., 2021). We agree that this metric does not necessarily capture the cumulative precipitation at a location from a cyclone. We have repeated the analysis using the maximum cumulative precipitation from each cyclone at a location and include these results in the Supporting Information.

### **Other comments:**

Section 2. It seems that the whole analysis is restricted to the NH winter but it would be good to mention the months (Dec-Feb) explicitly in the Methods section. Currently, this is mentioned only in Section 2.3 but I guess it applies to the whole analysis. Which leads me to the 2nd question. Why only DJF? At least in Fennoscandia, November is often a very active month in terms of windstorm hazards.

We have added a sentence at the end of Sect 2.2: “The analysis focuses on the boreal winter season from December-February (DJF) when the North Atlantic storm track is most active, so the ETC tracks are filtered to retain DJF storms.”

Section 2.3. North Atlantic modes of variability. I think section 2.3 is a bit incomplete. It lacks justification why you chose the domain which you chose (90W-40E, 20-80N). Furthermore, I think this area is often called the *Euro-Atlantic* sector as it extends up to 40E, but you talk about the North Atlantic sector which is slightly misleading, given the area. Also, some studies (e.g. <https://rmets.onlinelibrary.wiley.com/doi/10.1002/qj.3341>) call the 2nd EOF as Scandinavian or European blocking. Is the East Atlantic pattern the same as Scandinavian blocking? If not, it might be reasonable to mention this in the text. It might help if the regression/correlation patterns against MSLP are shown, for example in supplementary material.

The domain follows Hurrell (1995), we have renamed this as the Euro-Atlantic sector. EOF2 is equivalent to the Atlantic Ridge pattern, this has been added. We have added a plot showing the EOF patterns to the supplementary information.

L46 and thereafter. You often cite Degenhardt et al. 2022 but there is only Degenhardt et al. 2023 and 2024 in the reference list.

Thanks for spotting this. It was due to the online appearance year being different from the print issue year. Corrected.

L136. This should be Section 2.4

Thank you for spotting this! Corrected.

L155: these metrics? which metrics?

Amended to “We also tested our analysis on these variables.....”

L157: Here you mention that linear regression performs poorly if the data is non-linear and contains lots of zeros. But isn't that the case for ETC-hazards too, for those regions which infrequently see ETCs during DJF months? So how do you deal with those regions that are far from storm tracks, and might not see ETCs every winter? Are there those regions at all?

This is a great question. The footprints are sufficiently large and the number of storms tracked per season sufficiently high, that in all seasons there is at least one value per gridcell for the domain studied here. If we extended further east that would become increasingly problematic.

L182: are shown

Corrected to “are shown as”

L200: show a reduction? How can you see this as the colour bars in Fig. 3 only show positive values? I see that the absolute anomalies in Fig. S3 also have negative

values, but I don't understand why the percentage anomalies in the  $\Delta PC1$  and  $\Delta PC2$  maps in Fig. 3 only show positive changes?

Thanks for spotting this mistake. We have extended the colour scales in Fig. 3 to include negative % changes. This shows the expected signals consistent with Fig S3.

L287. Previous work. Here it would be good to cite the actual previous work.

Citations added.

Fig. 5 and 6: the titles show 1981-2010, should it be 2020? And why does Fig. 7 have 2021 in its title?

Thanks for spotting these mistakes. All corrected to 2020.

Section 4. Please consider writing something about the limitations of your analysis. For example, the linear model does not naturally explain all the variability in ETC-related hazards. What other factors are there which add the variability? How could you improve your work in the future?

We have added to Sect 4: "The analysis assumes the relationships of the maximum winter ETC hazards with the seasonal models of variability are well described by a linear model. While we have shown that some common ETC and hazard indices are not well suited to the application of linear regression at the grid point level, notably the storm damage index (SDI) and discrete variables like storm frequency, there also remains unexplained interannual variance for the other ETC hazards studied here. It would be valuable to explore these residuals in further work and to test other higher order statistical models or machine learning techniques which are suited to identifying non-linear relationships."

## Reviewer 2

Summary: I think there is potential for this work to be developed into a very useful contribution to our understanding of the connections between weather hazards and variability on larger (both spatial and temporal) scales. To move towards that potential, a couple of important elements in the current manuscript need to be addressed, as follows:

We thank the reviewer for taking the time to provide a thorough review of our study. We are pleased they see the value of our work and thank them for their constructive suggestions which we have considered and respond to below.

### Main Comment

Reading through the analysis, the question I kept asking myself is: what is gained in the study by including ETCs? For Figs. 3 – 6, it is not clear to me how the ETC footprint step is useful. For instance, you could look at the relationship between the extremes for each of those hazards (regardless of ETC association) and the NAO and EAP. You might even have more data, which would make the analysis less noisy. So the question is: what does the utilization of ETCs add to the story? Does the link to the ETCs give us longer timescales in predictability? Or do they provide something else? This needs to be explained.

Thanks for the comment. ETCs are the primary feature of interest for winter weather forecasts in Europe. The starting point is therefore the extensive interest and literature on European weather hazards from ETCs; the goal is to understand how these hazards are shaped by seasonal North Atlantic climate variability which may be predictable. The goal is not to understand the weather hazards associated with the modes of variability themselves, since these modes are not a feature of weather forecasts. In that sense, the goal is to better connect weather and climate in the context of seasonal variability. Regardless, in winter the extreme wind, precipitation and coastal wave hazards analysed would almost exclusively be related to ETCs. There is an added benefit that if the modes of variability can be skilfully predicted by seasonal forecast models (e.g., Scaife et al. 2014), we would have early information about the likelihood of an impactful ETC affecting in different parts of Europe.

### Reference

Scaife, A. A., et al. (2014), Skillful long-range prediction of European and North American winters, *Geophys. Res. Lett.*, 41, 2514–2519, doi:10.1002/2014GL059637.

Related to this: In Section 3.2 (Line 211), based on the title, my hope was that you would focus on the compound events caused by individual ETCs, but you actually go in the opposite direction (based on my interpretation of what you write on Line 216). You might want to consider including an analysis that is focused on describing the ETCs that cause the compound hazards.

The rationale for this choice is that the timeframe for society to recover from a severe hazard would be longer than one season, meaning that damage to e.g., infrastructure occurring from compound hazards within the same season could be

more impactful than a single hazard even if they do not occur very close together in time.

### Minor Comments

Unfortunately, I think the title is too general of a statement. For instance, when I read that title, I think: I have high confidence in that statement, without doing any research. So, I suggest you come up with a new title that demonstrates more of the knowledge gained by this research.

Here is a possible replacement (mostly taken from text that you wrote in the introduction): “The relationships between extreme winter North Atlantic extratropical cyclone hazards and modes of seasonal climate variability”

We have suggested a new title of “The observed winter relationships between extreme North Atlantic extratropical cyclone hazards and modes of seasonal climate variability” which avoids using acronyms and makes clear the study focuses on winter.

Line 19: Throughout the abstract and the text, I think it will be more intuitive for the reader if you replace “PC1” with NAO and “PC2” with EAP. I appreciate that PC1 and PC2 are more precise, but they offer less connotation with physics.

Thanks for the suggestion. We also considered this and decided to stick with the PC notation because we refer to these quantitatively at some points in the text and we feel it is clearer that they are standardized indices when referred to as PCs.

Line 46: In this section, you may also want to refer to Pinto et al. 2009 (DOI 10.1007/s00382-008-0396-4)

Added, thanks

Line 51-52: You write: “A positive EAP phase is associated with an increase in cumulative winter storm severity in the UK, which is weaker than for the NAO ...”

What is weaker, the storm severity or the association? Please re-write the sentence to clarify.

Amended to: “A positive EAP phase is associated with an increase in cumulative winter storm severity in the UK, though for an equivalent change in index the amplitude of the storm severity signal is weaker than for the NAO,....”

Line 82 - 86: You have provided some discussion on the potential bias in the reanalysis. Please expand a bit more on this. Please add some discussion specifically about the wave swell. And for winds and precipitation, some examples that you could reference:

Ramon J, Lledó L, Torralba V, Soret A, Doblas-Reyes FJ. What global reanalysis best represents near-surface winds?. Q J R Meteorol Soc. 2019; 145: 3236–3251. <https://doi.org/10.1002/qj.3616>

Chen, T.-C., Collet, F., & Di Luca, A. (2024). Evaluation of ERA5 precipitation and 10-m wind speed associated with extratropical cyclones using station data over North America. *International Journal of Climatology*, 44(3), 729–747.  
<https://doi.org/10.1002/joc.8339>

Thanks for suggesting additional references for ERA5 evaluation. We have expanded this paragraph in Sect 2.1 as follows: “Winter average near-surface wind speeds in Europe in ERA5 have been shown to agree better with observation towers than several other reanalysis datasets (Ramon et al., 2019). As is standard for climate models, wind gusts are post-processed in ERA5 following the approach described in ECMWF (2016). Comparison with meteorological station data in Sweden indicates strong wind gusts (>~15 m/s) are generally underestimated in ERA5 (Minola et al., 2020). This is consistent with Chen et al. (2024), who evaluated ERA5 against weather station observations in North America and showed that in DJF spatially-averaged near-surface wind speeds within ETCs are well represented ( $r \sim 0.9$ ), but wind speeds associated with the most intense ETCs and local extremes within ETCs are generally underestimated. Lodise et al. (2024; their Fig 7) also compared Northern hemisphere ETC wind speeds in ERA5 with radar altimeter measurements and found ERA5 has a low bias by ~5% in the region of strongest 10m wind speeds on the eastern hemisphere of the cyclone relative to the translational direction. For significant wave height, Bessonova et al. (2025) and Fanti et al. (2023) compared ERA5 to global buoy measurements and found an underestimation in ERA5 which was most pronounced for larger wave height measurements. Lodise et al. (2024) also found that in ERA5 significant wave heights within Northern hemisphere ETC footprints are biased low by ~5% compared with radar altimeter data. For daily precipitation, ERA5 shows the smallest biases in the winter extratropics compared to other global regions and seasons (Lavers et al., 2022) and captures observed variability across Europe with significant skill compared with E-OBS observations (Bandhauer et al., 2021). However, ERA5 underestimates the magnitude of extreme daily precipitation but it can generally capture the location and timing of precipitation extremes (Lavers et al., 2022). Therefore, based on studies that have evaluated ERA5, we conclude that the extreme European ETC hazards derived in this study are likely to be conservative estimates.”

Line 107: What is “Rx1 day metric”? I tried searching within your doc and did not find Rx anywhere else. Sorry if I missed it.

We have clarified this as follows: “The first measure relates to the likelihood of flooding, particularly pluvial flooding in urban areas, and is akin to the Rx1day metric which represents the wettest calendar day in the year and is widely used in the climate extremes literature (e.g., Seneviratne et al., 2021).”

Line 194: You write: “The lack of significant relationship with PC1 could be a result of the relatively noisy data, since at each gridpoint we are taking the wettest day in the winter associated with any ETC and regressing this against the seasonal NAO.” Have you tried interpolating the seasonal NAO to daily? Or is there a reason you have to use the seasonal NAO? If so, remind the reader of that reasoning. Also,

have you tried relaxing the definition of the “extreme” precipitation to include more data (i.e., instead of using the maximum, use the top N percentile)?

We use the winter average NAO because it can be predicted on seasonal timescales (Scaife et al. 2014). Calculating the NAO on daily timescales would negate the utility of the low frequency long-range predictable signals which motivate the study. We have calculated the relationships using the cumulative precipitation at each location over the ETC lifetime and this shows less noise than the winter daily maximum precipitation originating from an ETC.

## Reference

Scaife, A. A., et al. (2014), Skillful long-range prediction of European and North American winters, *Geophys. Res. Lett.*, 41, 2514–2519, doi:10.1002/2014GL059637.

Line 213-214: You write:

“Here we consider the overlap of the shaded areas for each hazard in Fig 3 separately for PC1 and PC2 to determine the relative exposure to multiple ETC hazards at a given location.”

Please explain more clearly the method for capturing the overlap and explain what you interpret this overlap to mean.

This has been expanded to: “To determine all the locations where either PC1 or PC2 modify at least one ETC hazard, we overlay the coloured areas for the three hazard regression maps in Figure 3 and plot the net geographical coverage. This is shown in the left column of Figure 5. To determine regions where there are coincident signals in two variables for either PC1 or PC2, we identify where any two coloured areas from the regression maps in Fig. 3 coincide at the gridpoint level. This is in the middle column of Figure 5. Finally, to locate regions with altered exposure to all three hazards, we identify the points where the coloured areas from Fig. 3 coincide when all three fields are overlaid. This is the right column of Figure 5.”

Line 237: You write:

“... we next consider the overlap between the PC1 and PC2 patterns for each variable separately, ...” How do you do this analysis. Explain it clearly.

This has been expanded to: “Many winters show anomalous North Atlantic atmospheric circulation that partly projects onto both PC1 and PC2, so it is possible that at some locations the exposure to ETC hazards will be modulated by both modes of variability. To determine these regions for each hazard type, we follow a similar process to Section 3.2 where the coloured areas from the regression maps in the middle and right columns of Fig. 3 are overlaid along each row. Where the two fields coincide shows the locations where the exposure to the hazard type will be affected by both PC1 and PC2. The resultant locations are shown in Figure 6 for the three hazard types.”



## 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 they 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:

$$x = 100 * (\text{regression} - \text{climatology}) / \text{climatology}$$

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  $\pm 1\sigma$  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  $\sigma$  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).